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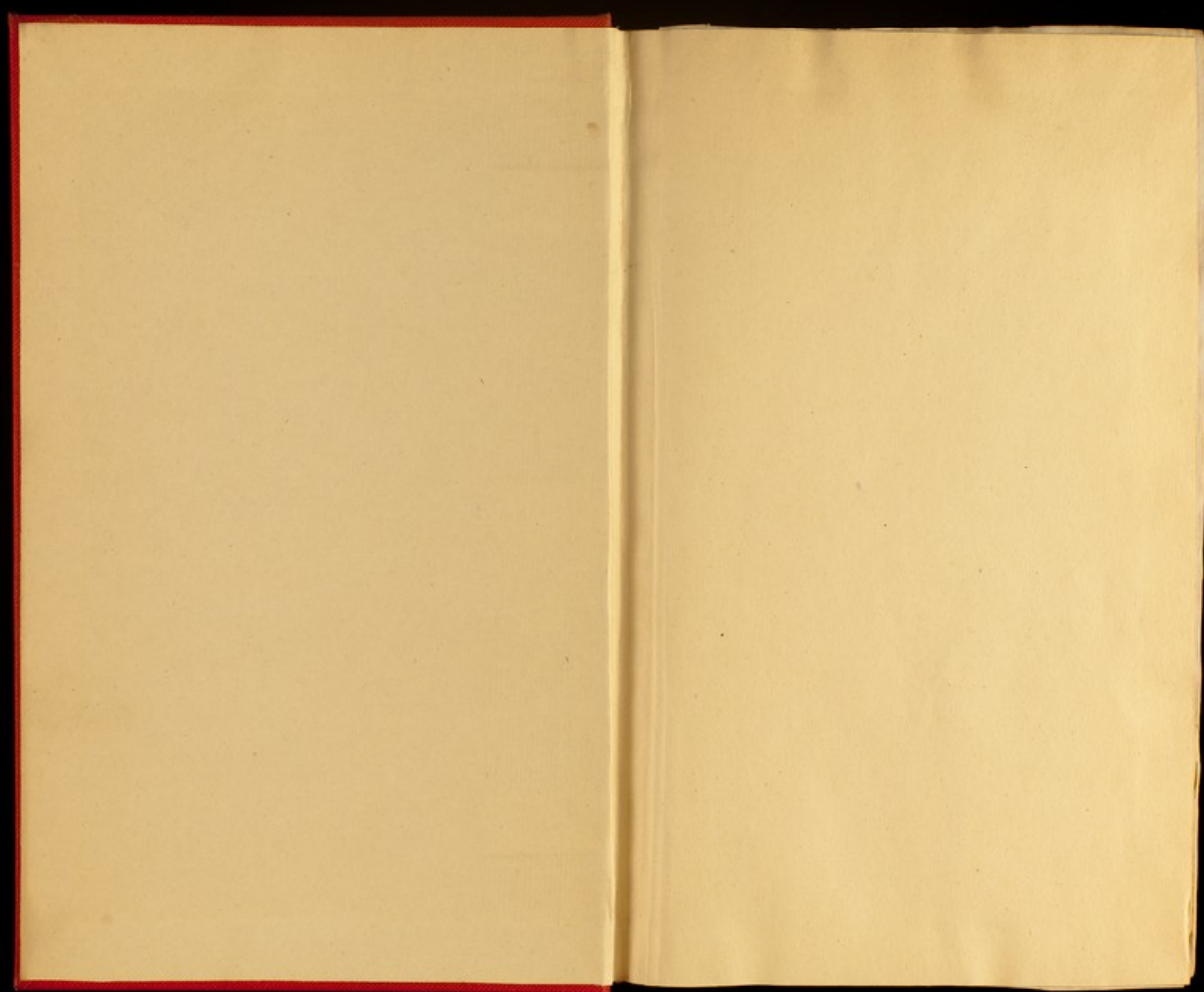
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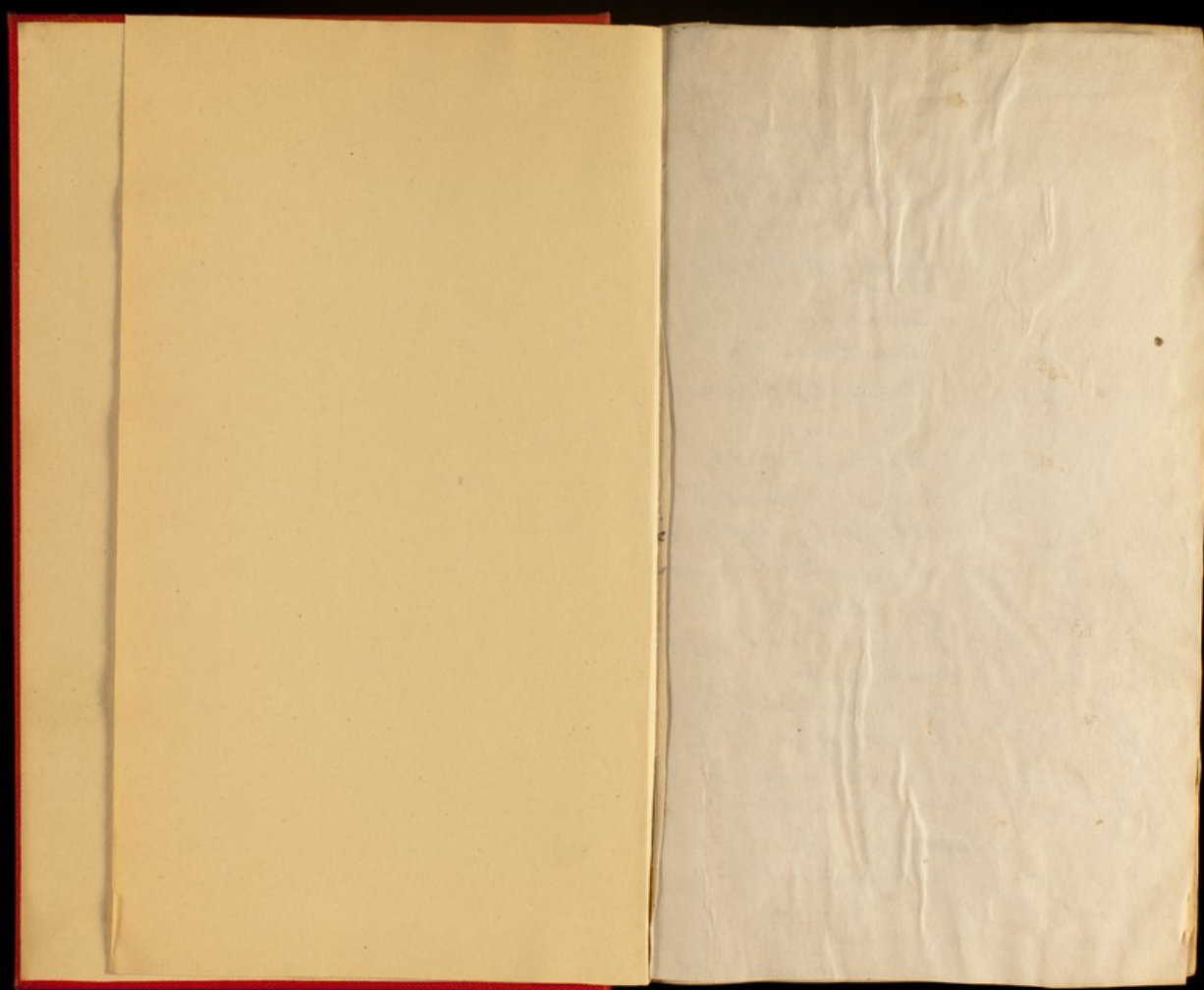
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Dworschak

Davy	Urea
Munclison	Cont ^d . Fevers
Oeland	Fevers in Africa. Dist.
Ballard	Sanitary Condition of Valparaiso p. 1858-59-60
Ringa -	Treatment of Neway Ague
Gairdner -	Aneurism
Kalford	Heart sounds
Macpherson	Mortality of Cholera
Gairdner	Pneumonia
Roberts	Diabetic sugar
Hammond	Chronic Intoxication
Gairdner -	Alcohol in hepatic
Roberts	Phosphate of lime in
Laurence	Eye colour
Kalford	Acute vent. valves
Marat	Gastric juice
Keile	Gut hospit. statistics
Ringa	Scarlet fever
Regle	Clinical Medicine
Marat	Chem. of digestion
	Laryngoscopy
Naughton	Hydrocyanic poisoning
Steubel -	Prostitution
Vogel	Milk probe
Mulins -	Royal Navy

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DIE CHOLERA

UND IHRE BEHANDLUNG.

VON

Hofarzte Dr. C. Dworschak.

Aus dem Russischen

VON

Dr. M. Magnus.

ST. PETERSBURG.
GEDRUCKT BEI CARL KRAY.
1853.

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DIE CHOLERA

ПЕЧАТАТЬ ПОЗВОЛЯЕТСЯ,

съ тѣмъ, чтобы по отпечатаніи представлено было въ Ценсурный Ко-
митетъ узаконенное число экземпляровъ, С. Петербургъ, 5 Апрѣля
1853 года.

Ценсоръ Н. Пейсера.

Vorrede.

Das mehrmalige Erscheinen der Cholera im Ver-
laufe der lezt verflossenen zwanzig Jahre, und die
Intensität, mit der sie wüthete, machten sie zum
Gegenstand der genauesten Untersuchungen gelehrter
Aerzte und Naturforscher. Allein das Wesen der
Krankheit bleibt bis zur Stunde unbestimmt, und
ihre Verheerungen fast dieselben, wie bei ihrem
ersten Beginnen. Alle Muthmassungen, gelehrte For-
schungen und Versuche, haben sehr wenig zur
Bändigug dieser Krankheit beigetragen.

Wenn ich nach so vielen Untersuchungen und
beachtungswerthen Schriften, die in allen gebildeten
Ländern herausgegeben worden sind, abermals meine
Meinung über die Cholera auseinandersetze (*), so
geschieht dies bloss in der Absicht, den praktischen

(*) Im Jahre 1848 wurde von mir herausgegeben: Mémoire
sur le développement, les causes, et le traitement du choléra.

Aerzten meine Ansicht über die Krankheit mitzu-
theilen, von deren Richtigkeit mich der Erfolg der
Behandlung, die sich auf die Erklärung der nächsten
Ursache gründet, mehr und mehr überzeugt hat.

St. Petersburg im November 1852.

Die Entwicklung der Krankheit.

Der allgemein angenommenen Meinung nach ist
die Cholera eine schnell verlaufende, und in wenig
Stunden die gesündesten Menschen tödtende Krank-
heit: die genaueste Beobachtung ihres Verlaufs über-
zeugte mich aber, dass sie nur in äusserst seltenen Fäl-
len, und zwar nach groben Diätfehlern, einen solchen
schnellen Gang annehmen kann. Gewöhnlich bemerken
die Kranken den Anfang des Übels nicht, weil die
von ihnen verspürte leichte Unpässlichkeit, ihnen in
ihren Geschäften nicht hinderlich ist. Den Durchfall,
der durch irgend einen Diätfehler entstanden ist, be-
achten sie wenig; manchmal schreiben sie ihn auch
einer Erkältung zu, auch glauben sie oft, es sei
ein wohlthätiger Hämorrhoidalzufall. Die an Leibes-
verstopfung leiden, sind mit ihrem Zustande zu-
frieden, der allgemeinen Behauptung zufolge, die
Cholera könne nur mit Durchfall beginnen.

So steigt die Krankheit und erreicht oft die Hälfte ihrer gewöhnlichen Laufbahn, ohne von dem Kranken beachtet und gehörig behandelt zu werden. Die Zeichen einer leichten Unpässlichkeit sind: Schmerz des Kopfes, Schwindel, Schlaflosigkeit, Schmerzen unter den Rippen, Kollern im Unterleibe und ein kleiner Durchfall. Zu einer andern Zeit könnten diese Zufälle von selbst vergehen, aber während der Epidemie sind sie von grosser Bedeutung und können, alle vereinigt, für die erste Periode der Cholera gelten. Besässen die Leute Kenntnisse von den ersten Zufällen des Uebels, wüsste jedermann, welche Ursachen zur Entstehung desselben beitragen können, und hätte man die nöthigen Mittel, für die erste Hülfsleistung, bei der Hand, wahrlich die Verheerung wäre bei weitem nicht so gross.

Der grösste Theil der Menschen, besonders in den unteren Ständen, sieht in den ersten Zufällen der Cholera keine Gefahr; sie scheinen ihnen der Beachtung nicht werth, oder sie gebrauchen irgend ein Hausmittel, das, wenn es nicht schadet, meistens unzulänglich ist, den Verlauf der Krankheit zu hemmen. Die ärztliche Hülfe wird erst dann gesucht, wenn sich schon Erbrechen und Krämpfe eingestellt haben, die Extremitäten kalt und blau geworden sind und der Puls nicht mehr zu fühlen

ist. In diesem Zustande gelingt es dann leider selten den Kranken zu retten. Es ist zum Erstaunen wie weit auch bei den gebildeten Klassen der Leichtsinn und die Sorglosigkeit in Vernachlässigung der Vorsichtsmassregeln gehen, und alles dies kommt daher, weil man an die Gefahr der ersten Zufälle in der Cholera nicht glauben will!

Um die Krankheitszufälle der Cholera genauer schildern zu können, theile ich ihren Verlauf in drei Perioden ein, in denen sich alle Abstufungen derselben einschliessen lassen. Diese Eintheilung enthält nichts neues, indem viele Aerzte darin übereinstimmen, dass die Cholera ihre Vorboten hat, dann die *Cholera* und zuletzt die *eigentliche Cholera* folgt. Der Gebrauch aber, einer jeden dieser Perioden einen besondern Namen zu geben, könnte leicht zu den Glauben veranlassen, eine jede Periode sei eine andere Krankheitsform.

In der ersten Periode zeigen sich Zufälle, die auf einige Störung in dem Kreislauf des Blutes hindeuten. Die Kranken empfinden Schwere im Kopfe, Schwindel, Druck in der Brust und unter den Rippen, zuweilen einen vorübergehenden Schmerz im Kreuze, ermüden leicht, und der Puls schlägt stark. Diese Zufälle findet man meistens bei vollblütigen und an Blutlassen gewöhnten Subjekten. Da, wo es dem

Organismus nicht gelingt das krankmachende Princip zu überwinden, so beginnt der beschwerte Kreislauf des Blutes die Thätigkeit der Verdauungsorgane zu unterdrücken; die Esslust vermindert sich; die Zunge wird weiss, der Stuhlgang seltener, und die Ausleerungen selbst consistent und gehen in geringer Quantität ab; der Unterleib ist merklich aufgetrieben, von den in den Gedärmen angehäuften Gasen. Diese Gase verdünnen das in den Gedärmen Enthaltene, und verursachen, während ihres Übergangs aus einem Darm in den andern, das Kollern; anfangs gehen die Winde schwer ab, hernach aber leichter, wobei die Ausleerungen flüssig werden; der Urin hat anfangs eine dunkelgelbe Farbe, dann vermindert sich seine Menge, er wird röthlich und bekommt beim Erkalten, einen röthlichen Bodensatz von ziegelartiger Farbe; seltener empfindet der Kranke einen Drang zum Harnlassen. Diejenigen die früher furchtlos waren, fangen an kleinmüthig und verstimmt zu werden und leiden an Schlaflosigkeit; das Gesicht wird blass, fast bleifarbig, der Blick finster, unbelebt; zuweilen stellen sich nach einem unruhigen Schlaf, während des Ausreckens, Krämpfe in den Waden ein. Dieser Zustand dauert, ohne sich zu verschlimmern, einige Zeitlang fort, und ist der Kranke vor-

sichtlich im Essen und hütet sich vor Erkältung, so kann genannte Unpässlichkeit auch ohne alle Behandlung vorübergehen. Wahrscheinlich wird mir die Frage aufgeworfen: warum sollen wir diese schnell vorüber gehende Unpässlichkeit, als eine Periode der Krankheit annehmen? — Ich nehme sie darum als den Anfang der Krankheit an, weil ich keine offenbare Cholera sah, der nicht einige von den genannten Zufällen vorangegangen wären. Schliessen wir diese Periode aus, und nehmen den Anfang der Cholera von der folgenden an, so verlieren wir die günstigste Zeit zu ihrer Behandlung. Der grösste Theil der Fälle, der offenbaren Cholera, von welchen man sagt, dass sie den Erkrankten plötzlich getroffen habe, waren die Folge dieser ersten Periode. Menschen, die sich um ihre Gesundheit wenig kümmern, übersehen diese Zufälle, und wundern sich dann über den raschen Verlauf der Krankheit.

Mit dem Anfang der zweiten Periode steigern sich vorbenannte Zufälle: das Gesicht des Kranken drückt Leiden, Angst und Niedergeschlagenheit aus; das Kollern im Unterleibe wird so stark und laut, dass man es einige Schritte weit vom Kranken hören kann, und ist zuweilen auch von Schmerz begleitet. Der flüssige Stuhlgang erscheint anfangs 2-3

mal täglich, dann öfter und wässerig, ist weniger gefärbt, und nach einem jeden derselben fühlt sich der Kranke schwächer. Zuweilen klagt er über Übelkeit, bekommt auch manchmal Erbrechen. Der Art Zufälle dauern von 2 bis 4, so gar bis 6 Tage fort. Darauf folgen viele weissliche Ausleerungen, abgekochten Reisswasser ähnlich, in denen weisse Schleimflocken herumschwimmen (der weisse Durchfall). Die Ausbildung der Cholera geschieht in dieser Periode weit rascher, wenn der Kranke, vor dem Eintritt des Durchfalls, mit Leibesverstopfung behaftet war.

In der dritten Periode der Cholera sieht man in den Ausleerungen weniger weisse Schleimflocken; sie werden ganz wässerig, haben einen besondern, ihnen eigenthümlichen Geruch und enthalten schon gar keinen natürlichen gallichten Koth. Der Kranke leidet an unaufhörlichen Schwindel und Beängstigung; zuweilen kommen Übelkeiten und nach einer jeden raschen Bewegung Erbrechen. Anfangs erbricht er sich mit den Speisen, die im Magen waren, dann wirft er eine wässerige weisse Flüssigkeit aus; kalte Getränke, nach denen die Kranken gierig verlangen, erregen gleich Erbrechen, warme hingegen sind ihnen zuwider. Während diese Zufälle fort-dauern erscheinen Krämpfe, mehrentheils in den Waden, seltener in den grossen Zehen. Diese Zu-

sammenziehungen der Muskeln verursachen so heftige Schmerzen, dass der Kranke in Geschrei ausbricht. Die Urinabsonderung ist gänzlich aufgehoben; der Puls schwach, kaum fühlbar und langsam; die Ausdünstung ist reichlich, der eigentliche Schweiss aber kalt und klebrig (*). Die Haut verliert ihre natürliche Elasticität, die Falte, die man durch das Zusammendrücken mit den Fingern darauf macht, bleibt eine kurze Zeit zurück; das Gesicht des Kranken verändert sich so, dass man ihn kaum erkennen kann, es scheint länger geworden zu sein, ist bleifarbig und beim Befühlen kalt; die Augen sind eingefallen; die Extremitäten kalt und bläulich; der Puls nicht mehr zu finden; die Zunge ist kalt, aber feucht, sie scheint breiter geworden zu sein und bekommt ein weissliches Aussehen; auch der Athem ist kalt (Zustand der Erstarrung); die Stuhlgänge bleiben vollkommen wässerig, sind fast durchsichtig, haben eine gelbgrünliche Farbe, enthalten gar keinen natürlichen Koth und gehen in geringer Menge und unwillkürlich ab; das Er-

(*) Das ist wahrscheinlich das Blutwasser des gestockten Blutes, welches in den Haargefässen der Haut sich zu zersetzen anfängt.

brechen dauert fort. Unterdessen behält der Kranke seine Geistesgegenwart bis zum Tode, er klagt nur über brennenden Schmerz in der Brust und Druck in der Herzgrube; die Stimme wird schwach, der Kranke spricht leise, abgebrochen, kaum verständlich, wirft sich unaufhörlich im Bette herum und schläft endlich den Todtenschlummer ein.

Zuweilen, was jedoch selten zu geschehen pflegt, gehet die Cholera plötzlich, ohne allen Durchfall, aus der ersten in die dritte Periode über. Diesen Fall nennen die Schriftsteller die *trokne Cholera* (*cholera sicca*). Sie befallt gewöhnlich Menschen von starker Leibesbeschaffenheit und Vollblütige, zufolge irgend einer moralischen Erschütterung, oder durch Vernachlässigung eines gewöhnlichen Aderlasses. So ein Zufall ist nichts anders als ein Schlagfluss. Hr. Professor *Lindgren* sagt (*): „Der schnelle Tod an der selbständigen *sicca* ist wohl entweder die Folge einer Hirnapoplexie wegen plötzlicher Stockung des Blutes, oder entsteht durch ein völliges Aufhören der Ernährung und Reproduktion überhaupt“.

(*) Versuch einer Nosologie der Cholera orientalis von J. G. Lindgren. Kasan. 1848. S. 29.

Die Ursachen der Cholera.

Über die *entfernte Ursache* der Cholera ist bis jetzt noch nichts Bestimmtes gesagt worden. Viele glauben mit Sicherheit, dass hier der Mangel der natürlichen Elektricität in der Luft unser Nervensystem unterdrückt, wodurch die Verdauungsorgane und die Thätigkeit des Blutsystems geschwächt werden, und dass durch diese Störung das Blut unvermögend ist, sich vom Kohlenstoff zu befreien, um seinen normalen Kreislauf zu vollbringen. Zwar lassen sich nach dieser Theorie der grösste Theil der Krankheitszufälle erklären, mir aber scheint sie nicht ganz genügend zu sein, indem wir im Anfang der Krankheit keine Zufälle gewahr werden, die auf eine Schwäche des Nervensystems hindeuten. In dem eigentlichen Cholera-Durchfall sogar, vermag der Kranke ohne den mindesten Schmerz, nach Willkühr tief einzuathmen, was er bei angegriffenem Nervensystem sicher nicht im Stande zu thun wäre. Ferner, beginnt die Cholera nicht mit Schwäche der

Stimme und Krämpfen, sondern diese Zufälle stellen sich erst zu Ende derselben ein und sind wahrscheinlich Folgen des Druckes des gestockten Blutes auf die Nerven. Der Kranke behält die volle Geistesgegenwart und auch Stärke genug, bis zur letzten Stunde die Füsse zu bewegen und seine blauen erkalteten Hände nach Willkür zusammen zu drücken. Woraus denn folgt, dass wenn das Nervensystem zu Ende der Krankheit nicht geschwächt, es zu Anfang derselben um so weniger ist.

Das krankmachende Prinzip der Cholera, ob es aus dem Innern der Erde sich erhebt, oder aus den höheren Regionen der Atmosphäre heruntersteigt, so bleibt dies doch am aller wahrscheinlichsten, dass das krankmachende Prinzip in der Luft, die wir einathmen, verbreitet ist. Was mag aber das eigentliche Wesen dieses verderblichen Prinzips sein, das von einem Ort zum andern wandert und sich dergestalt ausbreitet? Nach der Art seiner Verbreitung zu urtheilen, hat das Prinzip grosse Aehnlichkeit mit dem des Scharlachs, Krampfhustens, Grippe und Wechselfiebers. Ob aber dies Wesen gasartig ist, oder ob es vielleicht aus Myriaden unsichtbarer Infusorien besteht, wie Professor Ehrenberg (*) und andere glauben, — ist bis jetzt noch nicht entschieden.

(*) Professor Ehrenberg hat eine neue Welt von Thie-

Wiewohl die Chemie in unserem Jahrhundert auf eine hohe Stufe der Vollkommenheit gestiegen ist, so können wir doch nicht für sicher behaupten, alle gasartigen Körper zu kennen. Wem ist es bis jetzt noch gelungen genau zu bestimmen, woraus das schädliche Prinzip besteht, welches aus den Pontinischen und andern Sümpfen emporsteigt und Wechselfieber erzeugt? Ware auch das Prinzip, welches die

ren entdeckt, die aus so kleinen Infusorien besteht, dass man sie mit dem blossen Auge nicht sehen kann. Er fand sie im Nebel, im Regen, im Ocean, im stehenden Wasser, in thierischen und vegetabilischen Flüssigkeiten, in vulkanischer Asche, im Bimstein, im Opal, wie auch in dem feinen Staub, der zuweilen auf den Ocean fällt. Er sah achtzehn verschiedene Gattungen von mikroskopischen Thierchen in den Torfmorasten auf zwanzig Fuss unter der Oberfläche derselben. Eben so fand er Infusorien, die im Eise und kochenden Wasser leben. Nachforschend der Ursache, der im Jahre 1848 in Berlin herrschenden Cholera, entdeckte Herr Ehrenberg vier hundert verschiedene Arten mikroskopischer Thierchen in den Schichten der Atmosphäre, so dass die Luft der Vertheilung ihrer Bewohner nach, ganz dem Ocean entspricht, in welchem, nach Verschiedenheit seiner Tiefe, sich auch verschiedene Thierarten befinden. Der grösste Theil derselben steht auf der niedrigsten Stufe des thierischen Lebens, und neue Gattungen derselben erscheinen täglich. Einige dieser Infusorien, durch ein Mikroskop betrachtet, scheinen bloss aus durchsichtigen Bläschen zu be-

Cholera hervorbringt irgend ein bekanntes Gas, das Zufälle verursachte, wie die des Kohlendampfes, so muss es sich wahrscheinlich, nur in sehr geringer Quantität in der Luft befinden, da im umgekehrten Falle, kein einziger Mensch die Epidemie überleben würde, wenn man die ungeheuere Menge der atmosphärischen Luft berechnet, die wir im Verlaufe von 24 Stunden durch unsere Lungen durchziehen lassen (*). Da dieses Prinzip wahrscheinlich als ein Hundert, oder besser zusagen, Million Theil in der Luft enthalten ist, so mag dies die Ursache sein, warum

stehen, andere wieder haben Schwänzchen, bewegen sich mit grosser Schnelligkeit, wobei sie eine Art Instinct zeigen, indem sie Hindernisse, die ihnen auf ihrem Wege begegnen, zu vermeiden suchen. Es ginge über jeden Ausdruck und überstiege alle Vorstellung, wenn man die Myriaden dieser unsichtbaren Bewohner des Meeres, der Luft und der Erde beschreiben wollte. *Physical Geography* by Mary Somerville. Vol. II. pag. 244. London 1851.

(*) Wenn wir annehmen, dass ein Mensch von mittlerer Grösse und im ruhigen Zustande, bei jedem Athemzuge 15 Kubikzoll atmosphärische Luft ein und, eben so viel, ausathmet, jede Minute zu achtzehn Athemzüge gerechnet, so macht dies 270 Kubikzoll auf die Minute, oder 16200 auf die Stunde aus. Nach dieser Berechnung athmet ein jeder Mensch, im Verlaufe von 24 Stunden fast achtzehn Kubik Arschin atmosphärischer Luft ein und eben so viel aus.

es ungeachtet der chemischen Analysen, die während der Choleraepidemie gemacht wurden, unentdeckt geblieben ist. In dem Verhältniss, wie sich die Quantität des schädlichen Prinzips in der Luft vermehrt, oder vermindert, wächst oder vermindert sich auch die Zahl der Erkrankten. Zu der Zeit, in welcher die Epidemie ihre höchste Entwicklungsstufe erreicht, wobei das schädliche Prinzip sich verhältnissmässig auch in der Luft vermehrt, sind fast alle Menschen dem Kollern im Unterleibe und gestörter Verdauung unterworfen.

Nun fragt es sich: *ist die Cholera ansteckend oder nicht?* Diese Frage ist in vielen Beziehungen sehr wichtig. Zwar sahen wir in der vergangenen Choleraepidemie, wie sich mehrere Staaten, Z. B. Dänemark (*), durch Quarantinen zu schützen suchten; jedoch ist es schwer zu entscheiden, ob solche Orte, die von der Epidemie manchmal ver-

(*) Im Jahre 1849, als die Cholera noch in Petersburg herrschte, machte ich eine Reise, auf dem Dampfbote *City of Aberdeen*, nach England; wir kamen den 18 Juni um 5 Uhr des Abends nach Kopenhagen. Um aber die Reise fortsetzen zu können, mussten wir uns hier mit Steinkohlen versehen. Es stiessen auch zwei grosse Böte mit Kohlen zu uns; da aber alle Kommunikation streng verboten war, so entfernten sich die Dänen von uns, wie von der

schont geblieben waren, es der Cernirung oder andern Gründen zu verdanken hatten (*). Ich glaube, dass der grösste Theil der Aerzte, die Cholera-kranken behandelt haben, darin übereinstimmen werden, dass die Cholera durch die Berührung nicht ansteckend sei. So viel mir bekannt ist, war niemand von uns dadurch von der Cholera ergriffen worden, weil er solche Kranke berührt hatte. Ich selbst liess oft Cholera-kranken, in der letzten Periode mein Gesicht anhauchen, und fand immer, dass die ausgeathmete Luft kalt war. Würde die Cholera ansteckend sein, so müsste sie auch mich unvermeidlich befallen haben; auch hätte ich mich dann zu einem solchen Experiment nicht entschlossen. Dass während

Cholera angesteckten. Beim Anbruch des Tages aber, als ich aufs Verdeck trat, sah ich wie die Dänen mit unsern Leuten beim Ausladen der Kohlen beschäftigt waren, ohne irgend eine Vorsichtsmassregel zu beobachten. So wurden dort die strengen Quarantaine-Gesetze befolgt!

(*) Die Salzsiedereien im Wologdoschen Gouvernement können hier als Beweis dienen. Dort war die Cholera weder im Jahre 1848, noch 1851, trotz der Nähe dieser Salzsiedereien von Ortschaften, in denen die Epidemie grassirte, und trotz der Kommunikation mit denselben. S. das Journal des Ministeriums des Innern 1849. September, neuntes Heft S. 334.

der Choleraepidemie in einer und derselben Familie mehrere Personen erkrankten, beweist noch nichts von ihrer Ansteckungskraft, sondern nur, dass alle diese Personen gleichen Einflüssen ausgesetzt waren, und gleiche Art Speisen genossen hatten. Den besten Beweis, dass die Cholera nicht durch Ansteckung, sondern nur durch das schädliche Prinzip in der Luft, oder der Erde, mitgetheilt und verbreitet wird, geben, die ihr zwei oder mehrere Wochen vorangehende Durchfälle, Wechselfieber und Schlagfluss, wie es auch jetzt im Königreich Polen der Fall war (*). Gewöhnlich wird erzählt, die Cholera sei von irgend jemanden hereingebracht worden, der aus einem Orte kam, wo sie herrscht. Mir scheint es aber eher, dass der, aus einer von der Cholera befallenen Gegend, angereiste dort schon von dem schädlichen Prinzip durchdrungen, zu eben der Zeit ankam, als, so zu sagen, die Cholera-wolke just über den Ort hinzog, wo die Epidemie sich zu zeigen begann, denn von denjenigen, die von einem Choleraort in eine gesunde Gegend angelangt und gesund geblieben sind, spricht niemand.

Viele Nachforschungen sind über die *nächste Ursache* der Cholera angestellt worden. Leichenöffnungen

(*) S. St. Petersburgische Zeitung 1852 № 221.

zeigten uns die pathologischen Veränderungen im Organismus; chemische Untersuchungen der Ausleerungen und des Blutes haben uns zur Genüge bewiesen, welche von den zum Leben unumgänglichen Bestandtheile des Blutes durch die cholerischen Ausleerungen verfliegen, und fügen wir noch einige von den hauptsächlichsten Zufällen der Cholera hinzu, so werden wir im Stande sein, mit grosser Wahrscheinlichkeit, die nächste Ursache der Krankheit bestimmen zu können. Betrachten wir also jeden dieser Punkte insbesondere, so werden uns die einzelnen Schlüsse zu einem allgemeinen Resultat führen.

Ohne von den Nerven selbst zu reden, da die Veränderungen, die in denselben vorgehen sehr schwer zu bestimmen sind, haben uns die Leichenöffnungen gelehrt, dass die kleinen blutführenden Gefässe der Häute, die die Eingeweide des Kopfes, der Brust und des Unterleibes bedecken, so wie auch das ganze Venensystem, mit schwarzen und gestockten Blute überfüllt sind. Eben solche Veränderungen bemerkt man in den Leichen, der vom Schlagfluss, oder Kohlendampf Verstorbenen.

Die chemischen Analysen, die man in den Epidemien der Jahre 1832 und 1847 angestellt hat bewiesen, dass mit den cholerischen Ausleerungen sich auch die wässerigen, und mit ihnen die auflösbaren

salzigen Theile des Blutes vermindern. Woraus zu schliessen ist, dass das Blut, aus Mangel an diesen Stoffen, dick und schwarz wird.

Einige der hauptsächlichsten Zufälle der Cholera, als: Schwindel, Schmerzen in der Herzgrube und den Hypochondrien, Erbrechen, Durchfall, Krämpfe und das Blau werden der Haut, sieht man auch bei den durch Kohlendampf Erstickten. Das aus den Venen, in einem sowohl als den andern Fälle, gelassene Blut besitzt, dem Anscheine nach, gleiche Eigenschaften, es ist nämlich dick und schwarz.

Da die Leichenöffnungen, die chemische Analyse und die Analogie der Zufälle mit denen der andern Krankheiten uns überzeugen, dass das Blut bei den Cholerakranken schwarz und dick sei, so sind wir berechtigt zu glauben, dass die nächste Ursache der Krankheit in dieser veränderten Eigenschaft des Blutes liegt.

Das Blut in der Cholera wird, aller Wahrscheinlichkeit nach, schwarz und dick vom Überflusse des venösen Blutes, im Verhältniss zu dem arteriellen, oder von der Übersättigung desselben mit Kohlenstoff. Aus physiologischen Untersuchungen ist bekannt, dass die Verwandlung des venösen in arterielles Blut in den Lungen vor sich gehet. Da die Lungen aber, im Anfange der Krankheit, gesund

sind, wie dies die grosse Leichtigkeit beweist, mit der die Cholerakranken tief Athem holen, so fragt es sich: wenn die Lungen gesund sind, warum wird der Prozess dieser Verwandlung geschwächt? — Auf diesen Umstand sind viele Aerzte aufmerksam geworden, und der bekannte Professor Delpech stellt deswegen folgende Frage auf: „da das Blut noch vor dem Eintreten der Leiden des Athemholens schwarz wird, und wenn diese Veränderung der Farbe desselben einzig vom Überschuss an Kohlenstoff entsteht, so haben wir das Recht zu fragen: ob es nicht andre Wege giebt, auf welchen das Blut vom Kohlenstoff befreit wird?“

Bei Untersuchung der Stuhlgänge eines Cholerakranken sehen wir einen vollkommenen Mangel an Galle in ihnen, die im gesunden Zustande ein Produkt der Leber ist. Die physiologische Bestimmung dieses Organs überzeugt uns, dass es wirklich einen grossen Antheil an dem *ursprünglichen Ueberfluss* des venösen oder schwarzen Blutes hat. Die Leber zeichnet sich von allen andern Absonderungsorganen dadurch aus, dass ihre eigenthümliche Flüssigkeit, die Galle, wenn auch nicht einzig, doch vorzüglich von venösem Blute hervorgebracht wird. Die Pfortader, durch welche das venöse Blut aus dem Darmkanal zurückgeführt wird, vertheilt sich in

der Leber in Zweige, eben so, wie sich in andern Absonderungsorganen die Arterien vertheilen. Dr. Tiedemann macht die scharfsinnige Bemerkung, dass bei dem Kinde im Mutterleibe, bei welchem die Lungen unthätig sind, das Blut sich doch vom Kohlenstoff befreit, und dass in diesem Falle die Leber die Stelle der Lungen vertritt. Er sagt ferner, dass je weniger vollkommen bei einigen Thieren der Prozess der Verwandlung des Blutes in den Lungen aus dem venösen ins arterielle (die Blutoxydation) vor sich gehe, desto grösser der Umfang der Leber und die Quantität der gebildeten Galle sei. Aus diesen Beobachtungen können wir daher schliessen, dass die Leber ein Hilfsorgan ist, das mit den Lungen zugleich im Prozesse der Verwandlung des venösen Blutes ins arterielle wirkt, und dass die *vorzüglichste Bestimmung der Leber* darin besteht, das Blut vom Kohlenstoffe zu befreien, so wie es die *vorzüglichste Bestimmung der Lungen* ist, dem Blute Sauerstoff zuzuführen. Wir sehen, dass sobald bei einem Cholerakranken der allgemeine Kreislauf des Blutes und die Verrichtung der Leber hergestellt sind, die ersten Ausleerungen anfangs schwarz und hernach gelb werden. Ich glaube, dass dieser schwarze Koth von der Beimischung des venösen Blutes herkömmt, welches wegen aufgehaltener Verrichtung der Leber

während der Krankheit, durch die Pfortader in dieselbe nicht übergehen konnte.

Nach Erwägung der oben angeführten Beobachtungen, der physiologischen Verrichtungen der Organe, und nach Berücksichtigung einiger Zufälle der Krankheit, erkläre ich die Entwicklung der Cholera im Organismus (pathogenia) folgendermassen: während der Epidemie athmen wir, wenn auch in kleiner Quantität, mit der atmosphärischen Luft, das uns unbekannte krankmachende Prinzip ein, welches mehr als zur gewöhnlicher Zeit Kohlenstoff im Blute erzeugt. Dieser Überfluss an Kohlenstoff im Blute wird gewöhnlich ununterbrochen aus dem Organismus durch die Wirkung der Lungen, der Leber und der Haut ausgetrieben (*), wodurch wir gesund bleiben. Diese Absonderungsorgane aber, da sie sich während der Epidemie in einer beständig gesteigerten

(*) „Wir haben Gründe zu vermuthen, dass das Wesentlichste dieser Metamorphosen in dem Adernetze, welches den Übergang von den Arterien zu den Venen ausmacht, vor sich gehe, und dass Kohlensäure dabei ein Produkt dieser Metamorphose sei; denn das vom Adernetze kommende venöse Blut ist nicht mehr so beschaffen, wie das, welches hinein kam, und enthält nun mehr Kohlensäure, als da es in das Adernetz eingeführt wurde. Berzelius 9-ter B. Thier-Chemie. S. 432.“

ten Thätigkeit befinden, werden allmählig geschwächt, und die Menge des venösen Blutes nimmt gegen den normalen Stand, mehr oder weniger, die Oberhand. Die Leber, gestört in ihrer Funktion, sondert eine Galle ab, die keine gehörige Qualität hat, wodurch Leibesverstopfungen und Schwäche der Verdauung entstehen. Wenn bei diesem Zustande der Magen mit leicht gärenden und schwer verdaulichen Speisen belastet ist, so wird die Absonderung der Galle aufgehoben, die im Magen enthaltenen Speisen nicht verdaut, gehen in eine Art Gährung über, und die Folgen sind, dass die sich entwickelnden Gase den Milchsaff übersättigen, der mit dem venösen Blute sich vermischend, durch den Inhalt der schädlichen Gase, die Dicke des Blutes vermehrt. Der Kreislauf in den Blutgefässen wird geschwächt, weil sie vom normalem Blute nicht belebt werden; die Zusammenziehung der Arterien besitzt keine hinlängliche Kraft, um das stockende Blut fortzutreiben, der Pulsschlag wird schwächer und das Blut gerinnt in den feinen Adern, wodurch die Theile des Körpers kalt und blau werden; das zersezte Blut schwitzt, durch die Capillargefässe der Haut, sein Serum aus, welches in Gestalt eines klebrigen Schweisses hervortritt. Auf diese Weise hört die Entledigung des Blutes vom Kohlenstoff in der Leber, dem Hauptorgane des

Unterleibes auf; die natürliche Ausdünstung, die von der Wirkung der Capillargefäße abhängt, ist auf der ganzen Oberfläche des Körpers zurückgehalten, weil diese Gefäße mit dem verdickten Blute überfüllt sind, weshalb sie nur das Serum ausschwitzen. Nach allem diesem bleiben dem Kranken nur die einzigen Lungen, um das venöse Blut ins arterielle zu verwandeln. Kann man sich nun wundern, wenn bei solchen Unordnungen die Cholera am Ende einen so schnellen Verlauf nimmt? Das Erbrechen kommt, meiner Meinung nach, von dem in den Gefäßen des Hirnschädels angesammelten verdickten Blute und dem davon herrührenden Drucke auf das Gehirn. Die Krämpfe hängen, wahrscheinlich, von eben so einem Drucke auf jene Nerven ab, die in den Muskeln vertheilt sind, welche von den Krämpfen zusammengezogen werden.

Die gewöhnlichen *entfernten Ursachen* der Cholera sind: Erkältung oder Fehler in der Kost. Tägliche Beobachtungen beweisen, das einige Speisen zur gewöhnlichen Zeit ganz unschädlich, während der Epidemie aber Störungen in der Verdauung verursachen und die Cholera nach sich ziehen. Es ist unmöglich fest zu setzen, welche Speise schädlich oder unschädlich ist, weil Menschen, die an eine gewisse Kost gewöhnt sind, solche ohne Gefahr so-

gar in der Epidemie genießen können, während sie bei andern die Krankheit erzeugt. Diejenigen, die gut verdauen und an Roggenbrodt, Kwas, Sauerkraut, Milchspeisen u. s. w. gewöhnt sind, genießen alle diese Dinge ohne Gefahr, wogegen solche Speisen bei Menschen, die an dergleichen nicht gewöhnt sind, oder bei denen, die an einer schwachen Verdauung leiden, während der Epidemie, den Durchfall hervorbringen. Es giebt Lebensmittel, die zur Zeit der herrschenden Krankheit allen, im allgemeinen, schädlich sind und zwar: Schweinefleisch, Gans, nicht frische Fische, besonders gesalzene, nicht gut ausgebackenes und warmes Brodt, nicht ganz gegohrenes Getränk, unreife Früchte, Z. B. unreife Äpfel, Preiselbeeren, Vogelbeeren u. s. w., oder saftige Beeren, die schon längst gepflückt, zerdrückt und schon in Gährung übergegangen sind, als Johannisbeeren, Stachelbeeren u. s. w.; rohe Rüben, Kohlrabi, Rettig, Kohlstrünke. Für Personen, die schlecht verdauen, ist die Liste der Verbotenen Speisen noch weit grösser, wobei man jedoch nicht sagen kann, dass die unten benannten, alle ohne Ausnahme, schädlich wären, besonders wenn sie in kleiner Quantität genossen werden.

Die Erfahrung lehrt, dass für Schwachverdauende alle Arten geräucherten und fetten Fleisches schäd-

lich sind, als: Schinken, Enten u. s. w. von Fischen: Stockfisch, Lachs, Stör, Sterlet, Forellen, gedarter Storrüken (Balik), alle Arten geräucherter Fische, Kaviar, Krebse u. s. w.; die Vegetabilien: Kohl, Ruben, Kohlruben, Erdbeeren, Trüfeln, frische und gesalzene Pilze, Gurken, Melonen, Wassermelonen; von andern Dingen: Sauermilch, Käse, saure Schmant, Vinaigrette u. s. w.; fettes Backwerk, scharfe Käse etc.; von Getränken: frischer Kwas, oder solches Bier und Meth, und alle brausende Flüssigkeiten, wie auch Früchte, wenn sie sogar reif sind, und Kompote. Viele werden nun sagen: es wäre ja alles verboten! — Keinesweges, da es noch zur Genüge viele andere Lebensmittel giebt, wie: Rindfleisch, Kalbfleisch, besonders gebraten, Hühner, Kuchlein und verschiedenes Wild; frische weisse Fische als: Sänder, Barsche, Kaulbarsche; ferner, mehlichte und weisse Graupen, Reiss, Perigraupen, gute Kartoffeln, nicht fettes Backwerk und nicht in Gährung übergehende Conserven. Zur Zeit der Epidemie kann jede Überladung des Magens schädlich sein.

Die zurückgehaltene natürliche Ausdünstung kann, während der Epidemie, den Durchfall verursachen, wenn auch der Magen nicht überladen worden ist. Viele sind von der Cholera befallen worden, weil

sie sich auf dem Heimweg aus der Badstube erkaltet hatten. Übermässig warme Kleidung kann Fussgängern, die zum Schwitzen geneigt sind, schaden, besonders bei kaltem Nordwinde; zu leichte Kleidung hingegen, welche Brust und Unterleib nicht genug warm hält, und die natürliche Verrichtung der Haut nicht unterstützt, kann auch Ursache von gestörter Verdauung und dem daraus folgendem Durchfall werden.

Die Behandlung der Cholera.

Die Vorbauungskur. Während der Epidemie muss die grösste Aufmerksamkeit darauf gerichtet sein, die Funktionen der Haut und der Verdauungsorgane in natürlichem Zustande zu erhalten. Da der Antheil, den die Leber in der ursprünglichen Entwicklung der Cholera nimmt, bekannt ist, so müssen wir streng darauf sehen, dass keine Verstopfung statt finde. Allein auch hier hängt viel von der Gewohnheit ab; einige sind schon von ihrer Kindheit auf dazu angehalten worden, nur über einen oder zwei Tage einen natürlichen Stuhlgang zu haben, was ihnen, so zu sagen, zur Natur geworden ist; andere umgekehrt, gehen auch in ihrem gesunden Zustande, in 24 Stunden, drei oder vier mal zu Stuhl. In dieser Hinsicht müsste man den mässigen Abfuhrungsmitteln, die der natürlichen Verrichtung behülflich wären, den Vorzug geben. Rheum, Aloës und Mittelsalze, besonders das Glaubersalz, können in kleinen Gaben täglich den an Verstopfung Leiden-

den gereicht werden. Der Gebrauch des Kochsalzes, in den Speisen, in gehöriger Menge, dient auch zur Beförderung der guten Verdauung. Ein Esslöffel Kochsalz in einem Glase Wasser aufgelöst, mit Zusatz von zwei oder drei Theelöffelchen Rum oder Brandtwein, ist als ein einfaches Hausmittel bei dem Erscheinen der ersten Zufälle der Krankheit anwendbar.

Die an aufregende Mittel gewöhnt sind, mögen dabei bleiben, ohne solche jedoch in grosser Menge zu gebrauchen. Wein, schwarzer Pfeffer, Zimmt, Muskatnuss, Senf und andere bei Tische gebräuchliche Gewürze sind nützlich. Ich kenne Personen, die nach dem Essen drei mal täglich zu acht oder zehn Gran Kampfer nehmen, und sich dabei sehr gut befinden. Während der Epidemie muss man aber alle Arten von Speisen und Getränken vermeiden, die der Erfahrung nach als schädlich anerkannt worden sind, sonst bleiben alle Hilfsmittel ohne Nutzen.

Die Unterstützung der natürlichen Ausdünstung der Haut, hat grossen Einfluss auf die Erhaltung der Gesundheit. Wer leicht schwitzt erkältet sich auch leichter, und wenn jemand nach irgend einer Bewegung in der freien Luft in Schweiss gerathen ist, so muss er durchaus, besonders zur Winterzeit,

einen warmen Ort aufsuchen, und sich nicht im Freien abkühlen. Viele Krankheiten kommen im Winter überhaupt daher, dass Leute, die in warmen Zimmern arbeiten, sich oft mit Schweiss bedeckt daraus in schlecht eingerichtete Abtritte begeben, wo es überall zieht. Solche Orte müssen nach Möglichkeit verbessert werden, und die Leute sollten sich auch hüten im Schweisse nicht hinauszugehen. Wer an eine Bauchbinde, flannelne oder andere warme Unterkleider gewöhnt ist, darf diese während der Epidemie nicht ablegen; wer aber daran nicht gewöhnt ist und keine Kälte empfindet, bedarf keiner solchen warmmachender Kleider, die mehr Anlass zur Ausdünstung, folglich auch zur Erkältung geben. Überhaupt muss man sich nach der Jahreszeit und dem Wetter kleiden, so dass man weder zu warm noch zu kalt hat. Wer es verspürt, dass er sich am Tage erkältet hat, muss sich beim Schlafengehen warm zudecken, damit die Thätigkeit der Haargefässe der Haut wieder hergestellt werde.

Die Behandlung der Krankheit. Diese muss sich vorzüglich darauf gründen, der nächsten Ursache der Krankheit entgegen zu wirken, um wo möglich, durch Entfernung dieser Ursache, die Gesundheit herzustellen. Bei der Untersuchung der Ursachen überzeugten wir uns, dass die Schwärze und Dicke des

Blutes, in der Cholera, theils von der geschwächten Thätigkeit der Leber, und theils von der im Magen und Darmkanal stattfindenden Gährung herrührt. Durch diese Verdickung des Blutes wird der Kreislauf und die Ausdünstung gehemmt. In Rücksicht dieser zusammengesetzten nächsten Ursache, gründe ich die Cur der Cholera auf folgende Indicationen:

1). Auf Herstellung der normalen Thätigkeit der Leber, und Aufhebung der Gährung im Magen und Darmkanal.

2). Auf Verdünnung des Blutes, damit der Kreislauf desselben in den Haargefässen hergestellt, und auf solche Weise die allgemeine Ausdünstung hervorgebracht werde.

Abführungsmittel überhaupt, und besonders die so genannten Galleireibenden (Cholagoga), indem sie die Absonderung der Galle vermehren, erhöhen die Thätigkeit der Leber und befördern in mässigen Gaben die regelmässige Verdauung; so ist es Z. B. bekannt, dass das Rheum in grossen Dosen abführt und in kleinen den Durchfall zurückhält, d. h. die Regelmässigkeit der Verdauung herstellt. Auf ähnliche Weise wirken einige Salze Z. B. Kochsalz, Glaubersalz u. a. Aber das wirksamste Galleireibende Mittel ist Calomel. Wir sehen täglich in der Behandlung schnellverlaufender Krankheiten, wie

nach seinem Gebrauch gallige Ausleerungen folgen; dies ist besonders bei dem Gebrauch des Calomel in der Dysenterie zu bemerken. Manche flüchtige und aromatische Mittel tragen ebenfalls zur Beförderung der regelmässigen Verrichtung der Leber bei. Hierher gehören Kampfer, Pfeffermünzöl, Hoffmannstropfen, Wein, geistige Getränke, schwarzer Pfeffer und andere Gewürze und reizende Mittel. In den letzten zwei Epidemien, so wie auch in der jetzigen gebrauchte ich mit dem besten Erfolg, im Anfange der Krankheit eine Mischung von Salmiak mit Kampfer, nach der unten angegebenen Formel (*). Ich rühme dies Mittel nicht als Specificum, indem ich weiss, dass Rheum mit Kampfer, Kochsalz mit Pfeffermünzthee, Menthatropfen, Liquor ammoniae anisatus, oder sogar ein Glass Punsch, oft wirksam waren; allein überzeugt durch viele Erfahrungen von der schnellen und sichern Wirkung dieses Mittels im Anfange der Krankheit, finde ich es nicht für überflüssig, die Bereitung desselben hier anzuführen, und um so mehr, da es in keinem Falle, so gar in grossen Gaben, Schaden zufügen kann.

(*) Rp. Camphorae gr. xij, Mariat. Ammon. dr. ij, Mucilag. gum. arab. unc. j, Aq. commun. lib. j, M. f. l. a. mistura, D. S. Alle Stunden ein Spitzglass.

Nicht selten geschieht es, dass Kranke in der ersten Periode des Übels an Leibesverstopfung, begleitet von Kollern im Unterleibe, Schmerzen in den Hypochondrien und Unbehaglichkeit im ganzen Körper leiden. In solchen Fällen gab ich gewöhnlich ein Laxirpulver aus 20 oder 30 Gran Rheum mit 5-6 Gran Calomel; jedoch können auch andere Abführungsmittel, als: Aloë, Senesblätter oder Bitterwasser mit Nutzen angewendet werden.

In der zweiten Periode der Krankheit, beim Erscheinen des weiss aussehenden Durchfalls und Erbrechens, müssen wirksamere Mittel gebraucht werden; sind die Zufälle zu einer hohen Stufe gestiegen, so gebe ich einem Erwachsenen, anfangs alle Stunden oder sogar alle halbe Stunden, Calomel und Kampfer zu zwei Gran, mit zehn Gran arabischem Gummi, auf eine Gabe; Leuten von starker Leibesbeschaffenheit zu drei, und Kindern zu einem Gran von jedem Mittel. Die ersten Gaben werden gewöhnlich ausgebrochen, aber ein Theil davon bleibt doch im Magen zurück, wodurch bei der gleichzeitigen Anwendung der unten erwähnten äussern Mittel das Erbrechen und Laxiren seltener werden, und endlich ganz aufhören. Nach dem Aufhören des Erbrechens kann man noch ein oder zwei Pulver geben. Manchen wird es vielleicht sonderbar vor-

kommen, dass ich gleich nach Salmiak Calomel gebe, da nach chemischen Combinationen Calomel mit Chlorsalzen nicht vereinigt werden darf, allein der chemische Prozess, der im Magen vor sich gehet, ist von einer andern Art, als der im Glase. Geben wir doch in hitzigen Krankheiten Calomel, ohne Suppe zu verbieten, worin sich Kochsalz befindet.

In der vergangenen Epidemie verwarfen die Ärzte das Blutlassen, indem sie glaubten die Cholera rühre ursprünglich aus Störung in den Nerven her. Die Krankheit von ihrer ersten Entwicklung verfolgend, habe ich nie Zufälle bemerkt, welche offenbar auf eine ursprüngliche Affection der Nerven deuten, daher gebrauchte ich damals, wie jetzt, allgemeine und örtliche Blutaussäuerungen, und sah nie schlimme Folgen darnach, wenn sie nur zeitlich genug vorgenommen wurden. Es traf sich, dass ich zu Kranken eine oder zwei Stunden vor ihrem Tode gerufen wurde, wo der Puls nicht mehr zu fühlen und der Körper schon blau und mit kaltem klebrigen Schweiß bedeckt war. In solchem Falle hatte das Blutlassen keinen Erfolg mehr, und es floss dann kaum eine halbe Tasse Blut heraus. An dem Tode solcher Kranken war das Blutlassen gewiss nicht Schuld, sondern die allgemeine Stockung des

Blutes in allen seinen Gefässen, und dass dessen Übergang aus den Arterien in die Venen nicht mehr Statt finden konnte. Manche werden vielleicht hier einwenden: Wozu Blutaussäuerungen in der Cholera, hat sie denn einen entzündlichen Charakter? — Das bei einem Cholera-kranken ausgelassene Blut hat keine entzündlichen Eigenschaften, und fast nie bemerkt man die bekannte Entzündungshaut, allein, da während des Blutlassens selbst das Blut schwarz und dick ist, so erkläre ich mir den Nutzen davon auf folgende Weise: wir sehen, dass in der Reihenfolge der sich entwickelnden Cholera, die Menge des venösen Blutes sich allmählig so vergrößert, dass wenn der Verlauf der Krankheit nicht unterbrochen wird, das arterielle Blut im Organismus endlich ganz verschwindet, und die Leber, als das Hauptorgan, welches das Blut vom Kohlenstoff befreit, um es dünner zu machen, ist nicht im Stande eine solche Menge zu bearbeiten. Ich glaube daher dass, durch Verminderung der Masse des venösen Blutes, wir die Wirksamkeit der Leber befördern, und auf diese Art die normale Thätigkeit dieses Organs wieder herstellen.

Menschen, die an allgemeine oder örtliche Blutentleerungen gewöhnt sind, eben so wie Vollblütige, müssen schlechterdings beim Erscheinen der ersten

Cholerazufälle sich Blutlassen, oder Blutigel oder Schröpfköpfe ans rechte Hypochondrium oder die Herzgrube ansetzen. In gewöhnlichen Fällen, wenn die Krankheit noch in der ersten, oder im Anfange der zweiten Periode ist, kann allgemeine und örtliche Blutentleerung entbehrt werden; wenn aber die Cholera in vollkommener Entwicklung da steht und dem Leben Gefahr droht, dann muss man ohne Verzug zu allgemeinen und örtlichen Blutaussäuerungen schreiten.

Wir sehen, dass in der Cholera der Lebensprozess allmählig geschwächt wird und endlich ganz aufhört, weil das Blut durch seine Verdickung die Fähigkeit verliert sich im Kreislaufe zu bewegen, und zuletzt im ganzen Blutsystem gerinnt. Die chemischen Analysen haben uns bewiesen, dass diese Verdickung des Blutes in der Cholera, in Folge des Verlustes der auflöslichen Salze, welche im Blute des gesunden Menschen enthalten sind, statt finde, und dass diese Salze durch die cholertischen Ausleerungen mit den Serum zugleich ausgeführt werden. Durch die Analyse des Blutes des gesunden Menschen, die von berühmten Chemikern vorgenommen wurde, wissen wir, dass sich im Blute mehr Kochsalz, als anderer Salze befindet (*). Dr.

(*) Nach Berzelius, Marcet und Lecanu enthalten 100 Theile

Stevens machte zuerst die Bemerkung, dass schwarzes Blut durch Zusatz von Kochsalz röther wird, und wenn man eine Auflösung dieses Salzes zu dem aus der Vene fliessendem Blute mischt, so bleibt das Blut roth und flüssig. Dr. Wakefield gab in der Cholera Kochsalz als Arzneimittel, und mit ziemlich gutem Resultate. Da aber diese Krankheit zu Ende ihrer Entwicklung einen sehr raschen Verlauf hat, so können wir nicht hoffen, dass das Kochsalz, als ein das Blut flüssigmachendes inneres Mittel, rasch und zeitig genug die äussersten Enden der Arterien und Venen, so wie auch die Capillargefässe der Haut, die zu sehr von den Verdauungsorganen entfernt sind, erreichen werde. Um daher so schnell als möglich das in den Haargefässen der Haut gerinnende Blut flüssig zu machen, müssen wir uns bestreben, dass die Salzauflösung unmittelbar auf die Haut wirke. Zur Erreichung dieses Zweckes muss man den Kranken in Leintücher, die mit Salzwasser nass gemacht worden

Blut 0, 6 Kochsalz, von allen übrigen Salzen aber nur 0, 2 oder 0, 4. Nach dieser Berechnung enthält das Blut eines gesunden Menschen, von mittlerem Wuchse, beiläufig 15 Zoltnik Kochsalz, wenn man seine ganze Blutmasse zu 28 oder 30 Pfund annimmt.

sind, einhüllen, und dabei ihn durch künstliche Wärme zu erwärmen suchen. Durch die Wärme öffnen sich die Poren der Haut, und die Salzauflösung wird von der ganzen Oberfläche des Körpers eingesogen. Dadurch wird das gerinnende Blut in den Haargefässen flüssiger, die blau erscheinenden Theile beginnen sich zu röthen, im Körper entwickelt sich Wärme, die Wallungen des Blutes werden stärker, der Puls voller und schneller, ein reichlicher Schweiss bricht auf dem ganzen Körper aus, und fliesst in grossen Tropfen von dem gerötheten Gesichte herunter.

Man wird mir einwenden: wozu kann das Flüssigmachen des Blutes in den feinen Gefässen der Haut dienen, wenn es sich in den andern Arterien und Venen, die entfernt von der Oberfläche des Körpers, in den Eingeweiden liegen, seiner Verdickung wegen, nicht mehr bewegen kann? — Ist das Blut schon so verdickt, dass der Puls nicht mehr schlägt und der Kranke mit kaltem und klebrigem Schweisse bedeckt ist, woraus zu ersehen ist, dass das Serum des zersezten Blutes auf die Oberfläche des Körpers hervortritt, so hilft dieses Mittel auch nicht mehr; hat sich aber die Krankheit noch nicht bis zu dieser Stufe erhoben, und ist der Schlag des Pulses noch fühlbar, wenn auch die

Hände und Füsse des Kranken blau sind, so können wir hoffen, durch die Hervorbringung eines verstärkten Kreislaufes des Blutes auf der Oberfläche des Körpers, und durch die auf derselben erregten Wallungen im Blute, die Stockungen in den inneren Theilen aufzuheben; und wenn wir noch zu gleicher Zeit durch Blutlassen die Menge des venösen Blutes vermindern, so können wir um so mehr hoffen, die Stockungen des Blutes in den inneren Theilen in Bewegung zu bringen, und den Kranken auf diese Weise noch zu retten. Ich gebrauchte oft dieses Mittel, in ähnlichen Fällen, mit Erfolg.

Obgleich eine reine Auflösung des Kochsalzes eine solche Veränderung im Zustande des Kranken hervorbringt, so gebe ich doch einer Salzauflösung, nach Art des Raspalschen Wassers (*Eau sédative*), mit Zusatz von Salmiakgeist und Kampferspiritus den Vorzug, indem ich mich oft überzeugte, dass durch Zusatz dieser beiden Mittel, die Salzauflösung das Blut flüssiger macht, und die Ausdünstung befördert.

Am Krankenbette verfährt man folgendermassen: In einen Fayancebecken, oder eine andere irdene Schüssel, schütet man $1\frac{1}{2}$ Theetassen Kochsalz, und gießt ungefähr drei Bouteillen heisses Wasser darauf. Zu dieser heissen Auflösung

setzt man drei Unzen Salmiakgeist und eben so viel Kampferspiritus zu, die beide früher zusammen- gemischt werden. In diese Auflösung taucht man zwei zusammengelegte Leintücher ein, umwickelt damit den Körper des Kranken, und legt ihn so ins Bett, dass nur der Kopf frei bleibt, der übrige Körper aber, besonders die Füße, müssen sorg- fältig in diese nassen Leintücher eingehüllt werden; dann legt man noch eine Decke auf, und dar- auf Säcke mit heissem Hafer (*), endlich noch eine Decke oder ein warmes Kleidungsstück. Auf den entblösten Kopf legt man ein, in den Rest der kaltgewordenen Auflösung, getauchtes Hand- tuch, welches man von Zeit zu Zeit benezt, und Acht giebt, dass von der Flüssigkeit nichts in die Augen dringe. Auf diese Weise bleibt der Kranke anderthalb oder zwei Stunden eingewickelt, und so bald sein Gesicht sich röthet und ein reichlicher Schweiss sich zeigt, hören gewöhnlich alle Zufälle auf, und der Kranke fühlt sich wohl.

Wenn jedoch nach Verlauf dieser Zeit, bei dem Gebrauche oben erwähnter Mittel, das Erbrechen

(*) Ein Tschetwerik Hafer wird mit Wasser angefeuch- tet, und in einem Kessel, oder in Pfannen, auf einem Herde oder im Ofen heiss gemacht.

und der Durchfall fortauern, oder wenn der Kranke einen Schmerz in der Herzgrube empfindet, so be- freie man ihn von den nassen Leintüchern, ziehe ihm reine und trockene Wäsche an, und lasse ihm ohne Verzug zur Ader. Sollte aber der Kranke von schwacher Leibesbeschaffenheit sein, so setze man ihm Blutigel oder Schröpfköpfe unter der Herzgrube und dem rechten Hypochondrium an.

In leichteren Fällen der Krankheit ist manchmal ein Fussbad mit Salz (*) zur Hervorbringung einer starken Ausdünstung hinreichend.

Nach dem Aufhören der Cholerazufälle bleibt manchmal ein Kollern im Unterleibe zurück. In sol- chen Fällen lasse ich die Salmiak-Kampfer Mixtur fortsetzen; ist diese aber dem Kranken zuwider, so gebe ich eine schwache Auflösung von Glaubersalz oder irgend einem andern abführenden Salze mit Pfeffermünz- oder einem andern aromatischen Was- ser gemengt. Sollte in den folgenden Tagen keine

(*) Ein Suppenteller Kochsalz wird in heissem Wasser in einem kleinen Zuber von gehöriger Höhe aufgelöst. Die Füsse müssen bis zu den Knien im Wasser sein, und da- rin eine Stunde, oder wenigstens eine halbe Stunde ge- halten werden.

Leibesöffnung statt finden, so nehme der Kranke eine mässige Gabe Rhabarber (Tinct. Rhei Dareli), des Wiener Tränkchens oder eines abführenden Salzes. Bei noch zurückbleibendem Schmerz in der Herzgrube, gebrauchte ich mit Nutzem Einreibungen aus Merkurial- und Nervensalbe (ung. mercuriale cum ung. nervino), Morgens und Abends, im Verlaufe eines oder zweier Tage. Es geschieht auch in seltenen Fällen, dass sich, nach überstandener Cholera, Fieberbewegungen alle Tage zu einer gewissen Stunde einstellen. In solchen Fällen wendete ich schwefelsaures oder valeriansaures Chinin, mit Zusatz von etwas Rheum, mit Erfolg an.

Es ist viel über den Typhus, der sich in Folge der Cholera bildet, geschrieben worden. Diesen Übergang der Krankheit schreibe ich dem Gebrauche des Opiums während derselben zu. Was mich betrifft, so habe ich keine Gelegenheit gehabt den typhösen Zustand bei Cholerakranken, die ich behandelte, zu sehen; wahrscheinlich weil ich gar kein Opium während der Behandlung gebrauchte. Ich füge hier keine Übersicht der von mir behandelten Cholerakranken hinzu, denn die Krankheit ist in der ersten Periode zu unbedeutend, und wird von vielen für gar nichts gehalten, und anderseits kann ich, aller Gerechtigkeit nach, jene Kranken

nicht als von mir behandelte zählen, zu deuten ich in der letzten Periode, zwei oder drei Stunden vor ihrem Tode, gerufen wurde.

Die hier beschriebene Behandlungsart wendete ich in der vergangenen Epidemie an, und befolge sie auch jetzt, weil ich sie in allen Fällen wirksam finde, wo noch Hoffnung da ist, den Kranken zu retten. Allein das, so zu sagen, todt Blut bei einem Sterbenden wieder zu beleben, kann weder durch diese, noch durch irgend eine andere Heilmethode, je erreicht werden. Ist also der Puls gar nicht mehr zu fühlen, der Kranke blau, mit einem kalten, klebrigen Scheweisse bedeckt, und der Athem auch kalt, so sind alle Bestrebungen vergebens. Die Vernachlässigung der Krankheit bis zu einem solchen Grade, rührt grösstentheils von der Unwissenheit der Kranken selbst her.

Es ist tröstlich während der Epidemie zu folgenden drei Wahrheiten, die durch tägliche Beobachtungen sich bestätigen, Vertrauen zu haben: 1) Dass Menschen, die vorsichtig im Essen sind, auf Ordnung in den Verrichtungen der Verdauungsorgane sehen, und sich vor Erkältung hüten, von der Cholera nicht befallen werden; 2) Dass die Krankheit keinen schnellen Verlauf hat, und dass immer einige Tage vor ihrer

Entwicklung vergehen, wodurch sie noch bei Zeiten erkannt werden kann, und 3) Dass sie fast immer heilbar ist, wenn sie noch keine hohe Stufe erreicht hat.

From the PHILOSOPHICAL MAGAZINE for June 1854.

ON
A NEW AND SIMPLE METHOD
OF
DETERMINING THE AMOUNT OF UREA
IN THE
URINARY SECRETION.

BY
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UREA has long been regarded with much interest by scientific men, on account of its physiological and chemical relations. It represents one of the last stages of a series of metamorphoses or changes which nitrogenous matter undergoes in the animal economy, and is the form under which the detritus of pre-existing nitrogenous tissues which have become effete, principally pass from the system. This interesting organic base, urea, is not only formed during the exercise of the vital functions in man and some of the higher animals, but is also produced during the chemical decomposition of a number of substances containing nitrogen; and the chemist can now obtain it in any quantity by artificial means, and thus imitate one of the most important results of the chemistry of life.

In reference to medicine, urea is not without some practical interest, as it is well known that during various diseased conditions of the system the quantity of urea eliminated from the blood by the action of the kidneys and excreted in the urine is occasionally subject to great variation, and some ready means of ascertaining its quantity in that secretion might frequently aid the physician in forming his diagnosis of certain diseases.

Different means of effecting this object have from time to time been proposed; but all the methods hitherto recommended, with the exception, perhaps, of Baron Liebig's recent one, require for

their execution much time and trouble, and in some cases complicated and expensive apparatus; and though capable in experienced hands of yielding tolerably accurate results, would in the case of those not much practised in chemical manipulation, and perhaps unaware of the many sources of error to be guarded against, give anything but correct results, and are therefore inapplicable to the greater number of those desirous of a quick and easy method of determining the quantity of urea in urine.

The method I propose is one of extreme simplicity, and can be performed by almost any one in a very few minutes, and is capable of yielding results sufficiently accurate for all practical purposes. It is founded on the fact I have recently observed, that urea is very readily decomposed by the chlorides, or rather hypochlorites of soda, potash or lime; and its constituent nitrogen is evolved in the gaseous state, and from the quantity of gas evolved I estimate the amount of urea present.

After trying different means of carrying out that fact with a view of making it available to determine the quantity of urea in urine, I found that the following very simple one seemed to answer the purpose completely. I take a strong glass tube, about 12 or 14 inches long, closed at one end, and its open extremity ground smooth, and having the bore not larger than the thumb can conveniently cover. This I fill more than a third full of mercury, and afterwards pour in carefully a measured quantity of urine to be examined, which may be from a quarter of a drachm to a drachm or upwards, according to the capacity of the tube; then holding the tube in one hand near its open extremity, and having the thumb in readiness to cover the aperture, I quickly fill it completely full with a solution of the hypochlorite of soda (taking care not to overflow the tube), and then instantly cover the opening tightly with the thumb, and having rapidly inverted the tube once or twice to mix the urine with the hypochlorite, I finally open the tube under a saturated solution of common salt in water, contained in a steady cup or small mortar. The mercury then flows out and the solution of salt takes its place, and the mixture of urine and hypochlorite being lighter than the solution of salt, will remain in the upper part of the tube, and will therefore be prevented from descending and mixing with the fluid in the cup. A rapid disengagement of minute globules of gas soon takes place in the mixture in the upper part of the tube, and the gas is there retained and collected. The tube is then left in the upright position till there is no further appearance of minute globules of gas being formed, the time being dependent on the strength of the hypochlorite and the quantity of urea present; but the decomposition is generally completed in from three to four hours; it may, however, be left

much longer, even for a day if convenient, and having set the experiment going, it requires no further attention; and when the decomposition is completed, it is only necessary to measure the quantity of gas produced by transferring it into a graduated tube or measure.

I have generally used a graduated tube in the first instance, as it saves the trouble of transferring the gas and incurring the risk of losing some of it in the process. That which I would recommend as being convenient for this purpose is a stout tube having a bore of half an inch in diameter, and capable of holding from 2 to 3 cubic inches. A tube having this bore and about 14 inches in length, will hold $2\frac{1}{2}$ cubic inches, which will be quite large enough. Each cubic inch of it should be divided into tenths and hundredths of a part of a cubic inch.

It is scarcely necessary to remark, that in cases where great accuracy is required, due attention must be paid to the temperature and atmospheric pressure, and certain corrections made if these should deviate from the usual standards of comparison at the time of reading off the volume of the gas; but in most cases sufficiently near approximations to accuracy may be obtained without reference to those particulars. From a number of experiments, I have ascertained that the quantity of gas evolved from different amounts of urea, treated in the way I have just described, very closely approximates to the quantity of nitrogen gas which should be furnished from the urea by calculation. This will be seen from the following, taken from many experiments.

The fifth part of a grain of urea should furnish by calculation 0.3098 parts of a cubic inch of nitrogen gas at 60° F. and 30" bar.; the same quantity of urea treated as described furnished in one experiment 0.3001, and in another 0.3069 parts of a cubic inch of gas at the same temperature and pressure; which shows that the calculated quantity of nitrogen differs from the amount of nitrogen gas obtained by only a few thousandths of a part of a cubic inch. I may observe that I was obliged to operate on such small quantities of urea, on account of the graduated tube I had at the time being only of one cubic inch capacity.

Seeing, then, that the quantity of gas evolved agrees so very closely with the calculated amount of nitrogen present in a certain quantity of urea, I take the calculated amount as being the more correct; and knowing the relation that exists between a certain quantity of urea and nitrogen, I can from the quantity of gas evolved in any case easily calculate the amount of urea present by the simple process of rule of three. Thus the fifth part of a grain, or 0.2 of a grain of urea, gives by calculation 0.3098 parts of a cubic inch of nitrogen gas. Then 0.3098 : the

volume of gas found : : 0.2 : to the required quantity of urea ; or multiplying the first and third terms by 5, we have 1.549 cubic inch of gas representing one grain of urea, which is a simpler proportion. Using these data, I made several comparative experiments on different samples of urine with my method and that of Baron Liebig's, which I believe is considered one of the most accurate of the methods of determining urea in urine at present known. The following are the results of three comparative experiments on different samples of urine, using the same with each method.

Amount of urea in grains and parts of a grain in one fluid ounce of urine.

	Liebig's.	New method.
1st experiment . .	3.680	3.712
2nd experiment . .	5.328	5.472
3rd experiment . .	4.976	4.976

In the first and second experiments the quantity of common salt present in the urine was taken into account, as it is found to increase to a slight degree the apparent quantity of urea in the urine by Liebig's method. In the third this was not taken into consideration, and the quantity of urea was compared with the mean of two results obtained by my method. These experiments show how very closely the results obtained by the new method agree with those by Baron Liebig's, and therefore show the correctness of the former method.

I prefer the hypochlorite of soda to that of potash as a decomposing agent, because the soda salt is an article of our pharmacopœia under the name "Sodæ chlorinatæ liquor," and therefore can be so easily procured ; whereas the potash salt, not being used in medicine, would require to be specially made for the purpose. As to the hypochlorite of lime, I do not think it so effectual, and it has the disadvantage of soiling the sides of the graduated tube by the carbonate of lime formed in the reaction.

In reference to the quantity of hypochlorite of soda to be employed, it should always be used in excess ; and I think that about five or six times the volume of the urine employed would be found generally to be quite sufficient, and ensure there being an excess of the hypochlorite. The amount required may be easily determined also by direct experiment, by adding to a certain quantity of urine to be examined in a glass a measured quantity of the hypochlorite, and leaving it for a short time till the evolution of gas is nearly over ; then if, on the addition of more of the hypochlorite, the effervescence is renewed, it shows that there was not enough of the decomposing liquor first employed, and more must be added from time to time till no further

evolution of gas is produced ; and the quantity of hypochlorite used to arrive at this point indicates the amount necessary. I found by experiment that one grain of urea requires somewhere about half a fluid ounce of the ordinary sodæ chlorinatæ liquor for its complete decomposition. The amount of mercury employed requires some little attention. It should, as a general rule, be never less than the volume of gas produced ; for if the volume of gas evolved is more than that of the mercury used, it will be more than that of the solution of salt, and therefore some of the mixture of urine and hypochlorite will be forced out of the tube before it is completely decomposed, and consequently some of the gas will be lost ; so that if this occurs, we must repeat the experiment, using either a larger quantity of mercury if our tube will allow, or diminishing the quantity of urine employed.

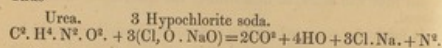
It might be supposed on first sight that this method would be liable to the following source of error, viz. that some of the gas would be evolved and lost during the pouring in of the hypochlorite ; but this is not the case, as several seconds elapse before there is any apparent reaction or evolution of gas on mixing the hypochlorite with the urine, and there is therefore full time to perform the experiment without any loss of the gas. I have also ascertained that the acid reaction of the urine does not affect my method. I should observe that this new method, like all the others known, is not perfectly free from some slight sources of error ; the principal one being, that ammonia, if it exists in the urine, gives rise to nitrogen gas, and therefore increases the apparent amount of urea ; but the same objection holds equally in Liebig's and Ragsky's methods, which are perhaps the two most accurate at present known. Uric acid also is similarly affected by the hypochlorite ; but it and ammonia ordinarily occur in such small proportion in urine, that the error produced from these substances would be but trifling, and is partly corrected by taking the calculated quantity of nitrogen, which is, as I have shown, something more than that obtained from a certain quantity of urea by direct experiment.

In cases where ammonia or uric acid occurs in more than ordinary quantity, these substances must be separated by the usual means employed before having recourse to my method.

I should think that gently heating the urine with a certain quantity of baryta water as long as the odour of ammonia is disengaged, and then filtering the solution, as recommended by Liebig, for the separation of ammonia before applying his method (see the Quarterly Journal of the Chemical Society, vol. vi. p. 30), would effect the object very easily, and separate not only the ammonia, but also the greater part, if not all of the uric acid present.

There is one other source of error which may arise, and which can be easily avoided. It is the following: that if a solution of the hypochlorite of soda alone, or standing over mercury, be exposed to the light for several days, it will very gradually evolve a minute quantity of oxygen, which shows that in determining urea we should not allow the experiment to go on for too long a time; but if left for a day, or even two, it will scarcely make any appreciable effect on the quantity of gas evolved in testing for urea.

The reaction which appears to take place in the process seems to be the following. The hypochlorite of soda acting on the urea gives rise to the formation of carbonic acid, water, and chloride of sodium, together with the evolution of nitrogen gas. Thus



The nitrogen is evolved and the carbonic acid is absorbed by some of the hypochlorite of soda in excess, for I find that this salt absorbs carbonic acid very quickly without evolving any other gas; and I failed in several experiments to detect the smallest portion of carbonic acid in the gas produced by acting on urea, though I have always noticed the presence of a very minute quantity of oxygen in the nitrogen gas. These appear from my experiments to be the changes produced; but this part of the subject I have not as yet minutely examined, and my experiments have hitherto been made on healthy urine; I have, however, ascertained that several of the substances found in urine during disease, as for example, sugar, albumen, bile, and excess of urinary colouring matter, produce scarcely any effect on the results obtained by this new method of determining the quantity of urea in the urinary secretion.

REMARKS
ON THE
CLASSIFICATION AND NOMENCLATURE
OF
CONTINUED FEVERS.

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REMARKS
ON THE
CLASSIFICATION AND NOMENCLATURE
OF
CONTINUED FEVERS.

ALL the divisions of Continued Fever, as well as the numerous designations which have been applied to these, have, for the most part, been founded upon their symptoms, or upon their supposed anatomical origin. But the more one studies the subject, the more he will be convinced, that in the case of such diseases as idiopathic fevers, the most philosophic classification must be one which is based on their etiology. Fevers dependent upon the most different causes may occasionally resemble each other closely in their symptoms, so as to render their diagnosis difficult. Take, for example, the so-called Relapsing Fever; when the relapse and other characteristic symptoms are absent, it may be difficult to distinguish between it and a case of Simple Fever or Febricula. Surely, however, we are not to conclude, that the former, which is a contagious disease, prevailing in widely spread epidemics, and dependent upon a specific poison, is one and the same affection with the latter, which is constantly occurring in the sporadic form, and arises from non-specific causes, such as exposure to the sun's rays and fatigue. In like manner, with regard to typhus and typhoid fever, in their symptoms, they may sometimes be closely assimilated; in their causes they are widely different. Now we know, that very similar symptoms may be produced in the animal body by two very different vegetable or mineral poisons; so much so, that it may be impossible to determine from the symptoms alone what the poison is. No one would be presumptuous enough to conclude, that the poisons, under such circumstances, were identical, without subjecting them to analysis; and the same principles should guide us in the study of fevers, which only differ in the nature of the poisons being more obscure.

Even those febrile conditions of the system which are the result of some local inflammation, may approximate idiopathic fevers very closely in their general characters; and, indeed, the resemblance between typhus and typhoid fever is never so great, as what we not unfrequently observe between the general symptoms of typhus fever and asthenic pneumonia. If there be no cutaneous eruption,

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I know of no symptoms by which we can distinguish a case of primary asthenic pneumonia from one of typhus, in which the pneumonia is a secondary complication; and yet there is this important practical difference, that the one is contagious, the other not so. If typhus was not prevalent, or the patient had not been exposed to its contagion, we should no doubt set down such a case as one of primary pneumonia; but we should be led to such a conclusion, solely from the certainty with which we can detect the presence of the local lesion. But in reference to typhus and typhoid fever, we must recollect that both are idiopathic fevers, independent of any local lesion; and that, although a specific lesion is developed in the course of the latter, the symptoms which indicate its existence, even when it is most extensive, may be very obscure. It is not to be wondered at, then, that they who have not had much experience in both fevers should have some difficulty in distinguishing them; but it is scarcely legitimate to infer, that the two fevers are identical.

One reason why many still refuse to admit the plurality of species of Continued Fever, is their neglect of the circumstances under which fevers originate. In an essay, which I lately had the honour of submitting to the Royal Medical and Chirurgical Society,¹ I endeavoured to prove that the class of Continued Fevers comprises three, or in all probability four, distinct species, originating from widely different causes.

First, there is *Typhus*, the grand predisposing cause to which is destitution; while the exciting cause, or specific poison, is generated by overcrowding of human beings, with deficient ventilation, and afterwards is propagated by contagion. Hence it is, that epidemics of typhus occur during seasons of famine, and in besieged cities; and hence it is, that we find it limited to the most overcrowded localities of large towns, and seldom meet with it in country districts, or in the upper classes of society.

Secondly, there is the "*Relapsing Fever*," about which there may still be some doubts as to its specific distinction from typhus. There can be no question that it differs widely from that disease, both in its symptoms and mortality; and also, that a previous attack of the one confers no immunity from a subsequent one of the other. On the other hand, relapsing fever is found to prevail, as epidemics, at the same times, and under the same circumstances, as typhus. Researches are still wanting as to the distinctive etiology of these two fevers; but I have grounds for believing, that it will yet be shown that relapsing fever is produced by famine alone; typhus, by destitution and overcrowding combined; in other words, that destitution and starvation are the predisposing causes of typhus, the exciting causes of relapsing fever.

Thirdly, there is *Typhoid* or *Enteric Fever*, which is less conta-

¹ This Essay will appear in the 41st volume of the "Transactions."

gious than either typhus or relapsing fever, and which is quite independent of the causes which give rise to these, being generated by the putrid emanations from decaying organic (animal) matter. The grounds for this opinion may be briefly summed up as follows:—

1. Previous attacks of either typhus or relapsing fever confer no immunity from subsequent attacks of typhoid fever (and *vice versa*.)

2. There is no authenticated proof that the poison of typhus has ever generated typhoid fever (nor *vice versa*.)

3. Typhoid fever does not prevail in wide-spread epidemics. It is essentially an endemic disease; or when it does become epidemic, such epidemics are always of the most limited and circumscribed character.

4. Typhoid fever is always most prevalent in autumn, or after a long continuance of hot weather. A hot autumn after a wet summer appears to afford the most favourable conditions for its development.

5. Typhoid fever is not, like typhus and relapsing, limited to the poor, but is met with among poor and rich alike.

6. Typhoid fever is not confined to overcrowded localities, but appears alike in the most dense, and in the least populous districts of large towns, and even in isolated houses in the country.

7. There is evidence, of the most conclusive nature, that typhoid fever may result from the emanations from (animal) organic matter, in a peculiar state of decomposition.¹ In every instance where "Fever" has been described as originating from such a cause, the fever has been typhoid. The reason why this cause is not more generally recognised, is the want of attention to the distinctions between the different fevers. Those who deny that "Fever" can be the result of putrid emanations, adduce thousands of cases of typhus and relapsing fever as negative evidence, in the same way as there are not wanting a few who bring forward typhoid cases to prove that fever cannot be the result of destitution and overcrowding.

Fourthly, there is *Simple Fever*, or *Febricula*, which is non-contagious, and arises from such non-specific causes as exposure to the sun's rays, fatigue, surfeit, etc. In its simplest form, this fever may terminate in 24 or 30 hours, as in the *Ephemeral* or *Diary Fever* of systematic writers; or it may be prolonged to eight or ten days, as in the *Ardent* or *Sun Fever* of tropical climates.

A faulty nomenclature must be assigned as another reason, why the various fevers are not more generally recognised as distinct diseases. The term *Typhus*, though derived from a single symptom, and so far objectionable, is one which is sanctioned by great antiquity and

¹ In addition to the evidence upon this point which will be found in my essay in the *Medico-Chirurgical Transactions*, I may allude to the circumstance, that Dr Barker of Bedford has recently succeeded in producing in animals symptoms very similar to those of Typhoid fever, by making them inhale the noxious principles arising from cess-pools.—*The Influence of Sewer Emanations*, by T. Herbert Barker, M.D. Lond., 1859.

a Hippocratic origin. The designation, Relapsing Fever, is no doubt inappropriate; for, in the first place, in a large proportion of the cases there is no relapse; and, secondly, it is hardly accurate to call that a relapse, which is a constituent part of the ordinary course of the disease; yet the name is distinctive enough, and, in the present state of our knowledge, it might be imprudent to alter it. The same remark, however, does not apply to the appellation Typhoid, to which I beg more particularly to draw attention. It is one which is not only faulty, but tends to create confusion. It is faulty; for, first, it literally means "like Typhus," and, consequently, it is at variance with all precedent in the scientific nomenclature of natural objects, whenever it is desired to confer designations on distinct genera or species; secondly, because the same word is constantly employed, in an adjective sense, to indicate a group of symptoms, which may come on in the course of any disease; and, thirdly, a large proportion of the cases to which it is applied exhibit no symptoms of a typhoid character, or resembling typhus. For the same reasons, it greatly tends to create confusion; and, indeed, I have good reasons for believing, that this name has done as much as anything else to make the public and the great body of the profession consider, that the affection is merely a variety of typhus. At the same time, none of the numerous synonyms is, in my opinion, more appropriate. For example, it would not be desirable to have a name derived from the abdominal lesion, tending, as such would do, to revive in the minds of many the exploded doctrines of Broussais. A suitable distinctive name for the disease remains a desideratum; and after having devoted much thought and attention to the question, I ventured, in a foot-note to my essay, already referred to, to propose one, derived from what I believe to be the cause of the fever. In that essay, I collected what, in my opinion, is conclusive evidence that typhoid fever is produced by the putrid emanations from decaying (animal) organic matter; and I therefore suggested for it the appellation of *Pythogenic Fever*,—*πυθωγενής*, from *πύθω* (*pythō*, putresco) and *γεννάω*. In repeating this suggestion in a more prominent manner, I do so with considerable diffidence, and am actuated solely by the crying necessity there exists for adopting a more appropriate designation for the disease in question. Should a better one be proposed, I shall gladly accept it; but, in the meantime, I would recommend the subject to the earnest consideration of the profession.

The whole class of Continued Fevers may be said to occupy an intermediate position, between the Eruptive Fevers (Variola, Scarlatina, and Rubella) on the one hand, and the Malarious Fevers (Remittent and Intermittent) on the other. As to the causes which originate the eminently contagious poisons of the eruptive fevers, we as yet know little or nothing; and if we have almost succeeded in eradicating one of these, our preventive measures have no reference whatever to the causes which generate it. The malarious fevers are non-contagious; and although we know little of the in-

trinsic nature of malaria, we have long known the causes and circumstances which give rise to these, and the prophylactic measures by which they may be in a great measure averted. Hence it is that agues, which in former times were so prevalent and so fatal in many parts of Britain, are now rarely met with. The continued fevers resemble the eruptive in being contagious, though to a less degree, while they are also assimilated to the malarious fevers, inasmuch as we know the circumstances under which they are developed, and the means by which they may, to a great extent, be prevented. I am fully aware, that the doctrines here enunciated are at variance with the deeply-rooted convictions of a large body of the profession, who, while they admit that the various conditions specified in this paper may favour the propagation of the specific poison of fever already existing, yet deny that the poison of a contagious disease can be by any such means generated *de novo*. But if this view be correct, how comes it that the same conditions only give rise to one form of disease? If, for example, overcrowding only acts by favouring the propagation of typhus (that it does thus act I am far from denying), how is it that, in temperate climates, it is always typhus which appears as an epidemic in a besieged city, or in an overcrowded prison, and not diseases which are notoriously more contagious, such as variola and scarlatina? In fact, with regard to typhus and pythogenic fever, the matter resolves itself into this: if certain conditions are present, we can, with almost certainty, predict the result.

Pythogenic fever may be said to form the connecting link between the continued and the remittent fevers. It is but sparingly contagious. There is also much that is remittent in the history of the course of the disease,—so much so, that many of the designations which have been bestowed upon it have reference to this character. There can be little doubt, I think, that the *Hemitriticus* or *Febris Semitertiana* of old writers, which was considered a composite disease made up of a tertian and quotidian intermittent fever, was pythogenic fever; while it is now generally admitted, that the so-called "Infantile Remittent Fever" is of the same nature. Again, there are not a few well-authenticated instances of pythogenic and malarious fevers prevailing together, and apparently generated under similar circumstances. Such is the case, Dr Mercer Adam tells us, every summer in the foul-smelling city of Amsterdam. A most remarkable instance of this intimate relation between the two fevers will be found in an account of the diseases observed in the commune of Guernange, in the Duchy of Lorraine, and presented by M. Anclon to the French Academy of Sciences in 1845.¹ There are also good grounds for believing, that careful dissections will show that many of the so-called remittent fevers of tropical

¹ See description of the *Febris Semitertiana* given by Hoffmann, *Op. Om.* 1740, lib. ii., cap. 5, p. 40.

² *Compt. Rend.*, vol. xxi., p. 158.

climates are typhoid or pythogenic. Already Drs Scriven¹ and Ewart,² of the Bengal Medical Service, have proved, by *post-mortem* examinations, the existence of pythogenic fever in India and in Burmah. Dr Scriven, who studied the characters of the disease under Dr Jenner at University College, has given figures of the intestinal lesion, and has written me that he has met with several other cases of the same fever since the date of his papers. It will be important to ascertain what are the precise circumstances under which such cases are observed in India, and whether they are not different from what are known to develop the more prevalent malarious fevers. With regard to these last, the common opinion at present is, that, although the putrefaction of vegetable matter is a frequent concomitant, it is not an essential part of the process of the development of malaria. This opinion is founded on the facts recorded by Chisholm³ and Fergusson;⁴ but both the facts and the opinion require reconsideration.⁵ Without entering into this question at present, I would merely record my opinion, that it is highly probable that vegetable putrefaction is the source of malarious fevers, while the putrefaction of animal matter begets pythogenic fever.

All the continued fevers which have been described by authors under so many different names, may be referred to one or other of the four species already spoken of. The following synonyms I have arranged under each of the fevers to which I believe they belong, after having referred myself to the works of most of the authors quoted. I have likewise given briefly the leading distinctive characters of each fever.

I.—TYPHUS FEVER.

Characters.—A disease generated by contagion, or by overcrowding of human beings, with deficient ventilation, and prevailing in an epidemic form in periods, or under circumstances, of famine and destitution. Its symptoms are: more or less sudden invasion marked by rigors or chilliness; a small, weak, usually frequent pulse; dry, brown tongue; in most cases, constipation; skin warm and dry; a morbilliform rash, appearing between the fifth and eighth days, frequently accompanied by true petechie, and lasting until death or recovery; great and early prostration; delirium coming on early, and for the most part low and wandering; contracted pupils; duration of the fever usually about 14 days, seldom or never more than 21. In the dead body, no specific lesion, but great congestion of all the internal organs.

Synonyms.

Τύφος; Typhus; Hippoc.⁶ Febris typhodes? (*Prosp. Alpin.*, 1611; *Recalchus*, 1639;

¹ *Med. Times and Gaz.*, Jan. 20, 1854, p. 79; and *Ind. Ann. of Med. Science*, No. viii., 1857.

² *Ind. Ann. of Med. Sc.*, No. vii., 1856.

³ *Edinburgh Medical and Surgical Journal*, vol. vi.

⁴ On Marsh Poison. *Edinburgh Philosophical Transactions*, vol. ix.

⁵ See my remarks on the Origin of Marsh Poison, in my paper on "The Diseases of Burmah," *Edinburgh Medical and Surgical Journal*, vol. lxxxii., p. 79.

⁶ Probably a different disease.

Juncker, 1718); Typhus (*Callen*, 1769); Enecia Typhus (*Mason Good*, 1817); Typhus and True Typhus (*modern English writers*).

Febris pestilens (*Galen*? *Frascatorius*, 1546; *Forestus*, 1581; *Riverius*, 1623; *Willis*, 1659; *Sydenham*, 1663); Febris epidemica (*J. Barrois*, 1625); Pestilential Fever (*Grant*, 1775; *Stoker*, 1826); La constitution épidémique (*Bonlaac*, 1810); Epidemic Fever *pro parte* (*recent writers*).

One of the morbi contagiosi (*of Frascatorius*, 1546); Febris contagiosa (*Ogterus*, 1578); Infectious Fever (*Lind*, 1753); Der ansteckende Typhus (*J. V. Hilsenbrand*, 1810); Typhus contagiosus (*J. C. Goss*, 1811); Das ansteckende Nervenfeber (*Horn*, 1814); Contagious Fever (*Botman*, 1818); Tifo contagioso (*Rossi*, 1819); Contagious Typhus (*English writers*).

Febris putrida et maligna, Synochus putris et S. cum putredine (*old authors*);¹

Febris maligna pestilens (*Riverius*, 1623; *Sennertus*, 1641; *Willis*, 1659);

Malignant Fever (*Langrish*, 1735; *Fordyce*, 1791); Febris continua putrida (*Boerhaave*, 1738; *Wintringham*, 1752); Putrid Malignant Fever (*Huxham*,

1759); Febris exanthematica, maligna et venenosa, et pernicio (*J. F. Bianchini*, 1750); Febris maligna (*Le Roy*, 1771); Putrid Fever (*Macbride*, 1772); Febris continua putrida (*Selle*, 1770); Das Faulfeber (*Hecker*,

1800); Febris putrida (*Ital.*); Fièvres putrides et malignes, *pro parte* (*Fr.*); Typhoid Fever, with putro-adyamic character (*Copland*, 1844).

Febris contagiosa in carceribus genita (*Huxham*, 1742); Jail Fever (*Pringle*,

1760; *Hogham*, 1782; *John Howard*, 1784); Typhus Carcerum (*Sauvages*, 1764); Febris carceraria (*Barrois*, 1785); Jail Distemper (*J. C.*

Smyth, 1795); Maladie des Prisons (*French*).

Malignant Fever of the Hospital (*Pringle*, 1752); Febris nosocomialis (*Barrois*,

1785); Fièvre des Hôpitaux (*French*).

Pestis bellica and Typhus bellicus (*var.*); Morbus Castrensis vel Morbus Hungaricus, *pro parte* (*many old authors*); Morbus qui ex castris in Bavariam penetravit (*Rhumelius*, 1624); Febris Castrensis (*Willis*, 1659; *Haller*,

1742); Febris militaris (*Petri*, 1665); Typhus Castrensis (*Sauvages*, 1768); Camp Fever (*Grant*, 1775); Die Kriegspäst (*Hufeland*, 1814); Typhus des camps et des armées (*Louis*, 1829).

Febris pestilentialis nautica (*Huxham*, 1752); Ship Fever (*Lind*, 1763; *Grant*,

1775); Febris nautica (*Barrois*, 1785).

Febris pestilens quam Cuticulas vel Puncticulas vocant (*Frascatorius*, 1546; *Forest*,

1591); Tabardillo et Puntos? (*De Torres*, 1574); Febris purpurea epidemica? (*Theracius*, 1578); Febris petechialis (*Salvus Dierius*, 1584; *Sennertus*,

1641; *Selle*, 1770; *Barrois*, 1785); Febris peticularis (*Robertus*, 1591); Febris purpurata? (*Riverius*, 1623); Febris pulicaris seu puncticularis (*Pet. A. Castro*, 1650); Morbus puncticularis (*Donkers*, 1686);

Febris petechialis vera (*Hoffmann*, 1700); Das Fleckenfeber (*Ettmüller*,

1726; *Reuss*, 1814); Spotted Fever (*Strother*, 1728); Morbus cum petechiis (*Strack*, 1736); Febris petechiale (*Rossi*, 1802; *Rasori*, 1812);

Morbo petechiale (*Acord*, 1811; *Palloni*, 1817); Typhus exanthematicus and Das exanthematische Nervenfeber (*of German writers*); Petechial

Fever (*Poebles*, 1835); Petechial Typhus (*auct. var.*).

Febris asthenica? (*var.*); Febris atacta *pro parte* (*Selle*, 1770); Fièvre ataxique *pro parte* (*Pinel*, 1798); Fièvre adynamique (*Pinel*, 1798; *Roux*, 1813);

Adynamic Fever (*Stoker*, 1826; *Burne*, 1826); Brain Fever? (*auct. var.*)

II.—RELAPSING FEVER.

Characters.—A contagious disease, which is apparently generated by destitution, and which is only met with in the epidemic form during seasons of

¹ Previous to the time of Huxham and Pringle, the terms putrid and malignant were frequently applied to all fevers, except the Simple or Febricula.

scarcity and famine. Its symptoms are: a very abrupt invasion marked by rigors or chilliness; quick, full, and often incompressible pulse; white tongue; tenderness at the epigastrium; vomiting, and often jaundice; enlarged liver and spleen; constipation; skin very hot and dry; no characteristic eruption; high-coloured urine; severe headache, and pains in the back and limbs; restlessness, and rarely subacute delirium; an abrupt cessation of all these symptoms about the fifth or seventh day;—after a complete apyretic interval (during which the patient may get up and walk about), an abrupt relapse on the fourteenth day from the first commencement, running a similar course to the first attack, and terminating on or about the third day of the relapse—mortality small, but occasionally death from sudden syncope;—after death, no specific lesion, but usually enlargement of liver and spleen.

Synonyms.

A five days' Fever with Relapses (*Ratty*, 1770); Short Fever, Five days' Fever, Seven days' Fever (*var.*, 1843); Relapsing Fever (*Paterson*, *Steele*, etc., 1847; *Jenner*, 1849).
Fever of the New Constitution (*O'Brien*, 1828).
The Epidemic Fever (*auct. var.*); Epidemic Fever of Edinr., 1817 (*Walsb*, 1819); Epid. Fev. of Ireland *pro parte* (*Barker* and *Cheyne*, 1821); Scotch Epidemic of 1843 (*Wardell*, *R. Cornack*, *Allison*, *Jackson*, *Henderson*, *Craigie*, etc.); the Silesian Fever of 1847 (*Brit. and For. Med. Ch. Rev.*, July 1851).
Epidemic Remittent Fever (*Mackenzie*, 1843); Gastric Fever with Remittent type (*Craigie*, 1845); Gastro-hepatic Fever (*Ritchie*, 1855); has also been designated—Mild Yellow Fever, Bilious Relapsing Fever, Bilious Remittent Fever, Remitting Icteric Fever.
Irish Famine Fever (*Stoker*, 1826; and *Dublin Journal*, 1849); Die Hungerpest (*Gravell's Notizen*, 1848).
Dynamic or Inflammatory Fever (*Stoker*, 1826); Synocha (*Christison*, 1840 and 1853).
Relapsing Fever in all probability constituted one of the varieties of the Inflammatory Fever or Synocha, of the writers of last century; in more recent times, on the other hand, it has not infrequently been considered a variety of Typhus.

III.—PYTHOGENIC FEVER.

Characters.—An endemic, slightly contagious disease, generated by putrefying organic (animal) matter. Its symptoms are: a commencement often insidious, or marked by slight rigors, a sensation of chilliness, or profuse diarrhoea; pulse usually frequent and soft, but variable in the same patient; tongue red and fissured, in a few days becoming dry and brownish; in most cases, but not invariably, increased splenic dulness, tympanites, abdominal tenderness, gurgling in the iliac fossae, and diarrhoea, with or without melæna; urine copious and pale; skin warm, often with irregular sweats; an eruption of rose-coloured papules, first appearing between the seventh and fourteenth days, and coming out in successive crops, each of which lasts two or three days; very rarely petechiae; frequently epistaxis; prostration coming on late, and often slight; delirium active or often absent; dilated pupils; the disease often protracted to the thirtieth day, and occasionally, though rarely, followed by a relapse of all the symptoms, including the eruption; after death, ulceration of the solitary and aggregated glands of the ileum, and enlargement of mesenteric glands.

Synonyms.

Typhus mitior and Synochus *pro parte* (*Cullen*, 1769); Abdominal Typhus (*Auterieth*, 1822, and *German writers generally*); Fever with affection of the abdomen (*Allison*, 1827); Fever with ulceration of the intestines (*Bright*, 1829); Synochus and Typhus with abdominal affection (*Southwood Smith*, 1830); Fièvre Typhoïde (*Louis*, 1829; *Chomel*, 1834); Typhoid

Fever (*Stewart*, 1840; *Bartlett*, 1847; *Jenner*, 1849); Mild Typhoid Fever (*Copland*, 1844); Enteric Typhus (*Christison*, 1850).
Illeceus, hæmorrhæicus! (*Hippoc.*); Hemitriteus! Tritophyas! and Triphodes! (*auctor. antiq. var.*); Febris semitritiana seu composita (*Galen*? *Foretus*, 1591; *Spigelius*, 1624); Tritophya typhodes (*Mangetus*, 1695); Frigeraria! (*Sagar*, 1776).
Infantile Remittent Fever (*var.*); Febris hectica verminosa (*Vander Bosch*, 1700); Febris verminosa (*Selle*, 1770); Infantile Gastric Remittent Fever (*Loock*, 1840); Infantile Icteric Fever and Worm Fever (*auct. var.*).
Febris non-pestilens! (*Foretus*, 1591); Morbus Hungaricus and Febris Hungarica *pro parte* (*Auct. var. et Sennertus*, 1641); Endemic Fever (*var.*); Autumnal or Fall Fever (*Austin*, *Flinn*, 1852; and in *America generally*).
Febris putrida (*Riccius*, 1623); Febris putrida quæ vulgo lenta appellatur (*Willis*, 1659); Febris putrida nervosa? (*Wintringham*, 1752); Febris putrida aut biliosa (*Tissot*, 1758); Febris a putredine orta (*A. Trullianus*, quoted by *Dursertius* as *Syn. for his Feb. gastric. ac.* 1785).
Febris lenta (*Foretus*, 1591; *Willis*, 1659; *Linnaeus*, 1763; *Fogel*, 1764); Febris hectica sive lues neurodes (*Willis*, 1667); Slow Fever (*Strother*, 1716; *Langrish*, 1735); Nervous Fever (*Gilchrist*, 1734); Slow Nervous Fever (*Hucham*, 1739); Febris chronica! (*Juncker*, 1736); Febricula, or Little Fever, commonly called the Nervous or Hysterical Fever, the Fever on the Spirits, Vapours, etc. (*Manningham*, 1746); Febris astacta *pro parte* (*Selle*, 1770); Febris lenta nervosa maligna (*Burnerius*, 1785); Irregular low nervous Fever (*Fordyce*, 1791); Fièvre ataxique *pro parte*, and F. adéno-méningée (*Pinel*, 1798); Nervenfieber! (*Blaschke*, 1814); Fièvre nerveuse (*Fr.*); Common Continued Fever (*Armstrong*, 1816); Low Fever (*auct. var.*).
Febris petechizans vel Spuria (*Hoffmann*, 1690).
Febris biliosa (*Galen*? *River*, 1623; *Stahl*, 1700; *Juncker*, 1736); Bilious Fever (*Pringle*, 1750; *Ratty*, 1770); Febris biliosa putrida (*Selle*, 1770); Febrile biliosa (*Benelli*, 1775); Synochus bilious (*Sawage*, 1763); Febris cholericæ! (*Fogel*, 1764); Bilio-gastric Fever (*Copland*, 1844); Gastro-bilious, and Bilious Continued Fever (*modern writers*).
Febris catarrhalis? (*Bockel*, 1580; *Craus*, 1670); Febris colliquativa? (*J. R. Fortis*, 1668); Febris Stercoralis? (*Quesnay*, 1753); Morbus mucosus (*Roderer* and *Wagler*, 1762); Febrile glutinosa gastrica (*Sarcone*, 1765); Febris mucosa (*Selle*, 1770); Febris pituitosa (*Selle*, 1770; *Stroch*, 1789); Febris colliquativa primaria seu essentialis (*Burnerius*, 1785); Morbus biliosus mucosus (*Knaus*, 1786); Febris pituitosa nervosa (*Jacobi*, 1793); Schleimfieber? (*Cous*, 1795); Fièvre muqueuse (*Fr.*); Mucous or Pituitous Fever (*Copland*, 1844).
Febris gastrica (*Ballonius*, 1734); Febris acuta stomachica aut intestinalis (*Heister*, 1736); Febris gastrica acuta (*Burner*, 1783); Fièvre méningo-gastrique (*Pinel*, 1798); Gastrisches Fieber (*Richter*, 1813); Fièvre gastrique (*Dict. des Sc. Méd.*, 1816); Epidemic Gastric Fever (*Cheyne*, 1833); Gastric Fever (*Craigie*, 1837).
Febris mesenterica maligna (*Bagliet*, 1696; *Hoffmann*, 1728); Febris intestinalis et mesenterica (*Riödin*, 1705); Febris mesenterica acuta (*Burchard*, quoted by *Burnerius*, 1785); Fièvre entero-mesenterique (*Petit* and *Sorbo*, 1813); Enterite Fever (*Mills*, 1813); Gastro-enterite (*Bronson*, 1816); Entero-mesenteric Fever (*Abercrombie*, 1820); Dothinerite (*Brémond*, 1825; *Christison*, 1840); Enterite Folliculeuse (*Forget*, 1841); Mucos-enteritis and Gastro-enteric Fever (*var.*). Enteric Fever (*Wood*, 1848; *Aitken*, 1858).
Night-soil Fever (*Brown*, 1855).

¹ Clinical Lectures, not published.

Many of the cases described by Cullen and his successors, under the designation *Enteritis erysipelatosa*, were probably examples of this fever.—(See description of it in *Alison's Path. and Pract. of Med.*, p. 323.)

IV.—SIMPLE FEVER OR FEBRICULA.

Characters.—A sporadic, non-contagious disease, arising from exposure to the sun, fatigue, surfeit, inebriety, etc. Its symptoms are: frequent, full, and firm pulse; white tongue; great thirst; constipation; high-coloured urine; very hot and dry skin; no eruption; severe headache, and sometimes acute delirium; the fever subsiding in from one to ten days, with copious perspirations, herpetic eruptions, etc.; in the dead body, congestion of all the internal organs.

Synonyms.

Kaisos (Hippoc.); *Causus* sive *Febris ardens* (Galen, Willis, 1659; Boerhaave, 1738); *Synochus caustoides* (Forestus, 1591; Mangetus, 1695); *La Calentura* (Piquer, 1751); *Causos* (Vogel, 1764); *Endemical Causus* (Moseley, 1789); *Enecia Cauma* (Mason Good, 1817); *Ardent Fever* (Burnett, 1812; Ronald Martin, 1841; Copland, 1844); *Ardent Continued Fever* (Morehead, 1856).
Σύνωχος? (Greeks); *Synocha* vel *Synochus simplex* (Rivierius, 1623; Hoffmann, 1700; Juncker, 1736; Burserius, 1785); *Synocha* (Linnaeus, 1763; Sauvages, 1768; Cullen, 1769); *La Fievre Synoque* (Davy, 1847); *Synochische* (Germ.).
Synochus imputris? (Galen); *Febris continua non putrida* (Lemnius, 1569; Boerhaave, 1738); *Synocha sine putredine* (Sennertus, 1641); *Synocha non putris* (Bellini, 1732); *Febris non putrida* (Quarin, 1781).
Σύνωχος; *Φεσματώδης*? (Gr.); *Febris Sanguinea*? (Avicenna); *Synocha sanguinea*? (Sennertus, 1641); *Feb. acuta sanguinea* (Hoffmann, 1700); *Febris venosa* (Ballonius, 1734); *Acute Continual Fever* (Langrish, 1735); *Simple Inflammatory Fever* (Huxham, 1739; Fordyce, 1791); *Febris acuta simplex* (Storck, 1741); *Synocha plethorica* and *Ephemera plethorica* (Sauvages, 1763); *Febris continua inflammatoria simplex* (Selle, 1770); *Febris acuta* (Plouquet, 1791); *Entzündungsieber* and *Entzündliche Fieber* (Reil, 1794, etc.); *La Fievre angioténique* (Pinel, 1798); *La Fievre angioténique pure et simple* (Bouillaud, 1826); *Fievre inflammatoire* (French); *Febbre inflammatoria* (Ital.).
Febris depuratoria? (Quemoy, 1753); *Judicatoria*? (Sagar, 1776).
Febris septentrionalis (Platerius, 1556; Sprengel, 1814); *Ephemera plurium dierum* (Sennertus, 1641; Juncker, 1736); *Synocha septimo die soluta* (Hoffmann, 1700); *Febris continua*? (Stahl, 1700); *Febris continua simplex* (Lieutaud, 1776); *Simple Continued Fever* (modern writers).
Febris ephemera (Rivierius, 1623; Sennertus, 1641; Sauvages, 1768); *Diary Fever* (Strother, 1728); *Ephemera simplex* (Boerhaave, 1738); *Febris diaris* (Juncker, 1736; Linnaeus, 1763); *Fievre ephémère* (Davy, 1847); *Feblicula* (var., and Juncker, 1849, not the *Feblicula* of Manningham); *Das entzigtze Fieber* (Germ.); *Efinero* (Ital.); *Efinera* (Span.).
Ephemera a frigore and *E. a calore* (Sauvages, 1768); *Sun Fever* (Scriven, 1857).

FEVER IN AGRICULTURAL DISTRICTS;

BEING A

REPORT

ON

CASES OF FEVER

OCCURRING IN THE PARISH OF GREAT HORWOOD,
IN THE COUNTY OF BUCKINGHAM.

BY

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OXFORD AND LONDON:

JOHN HENRY AND JAMES PARKER.

1858.

Oxford, July 10, 1858.

SIR,

1. By request of Mr. Willis, Clerk to the Winslow Board of Guardians, I have inspected the parish of Great Horwood, in company with the Union medical officer, Mr. Wynter.

2. The purpose of my visit was to gather the particulars of a Fever which has been Endemic in the parish since Michaelmas, 1857. Accordingly, my attention has been especially directed, 1stly, to the Nature, 2ndly, the Cause, 3rdly, the Treatment, and 4thly, to the Mode of Prevention of the disease.

3. In reporting to you the conclusions at which I have arrived, I take leave to do so at the greater length, because the danger is not only considerable, but deeply rooted in this district; and because, did I answer you briefly, it might be supposed that I thought lightly of the case, because the mortality is numerically small. How grave the evil is may best be appreciated by a comparison, even if in some respects it be inapt. An Endemic affecting a similar proportion of the population of London would attack, in nine months, more than a quarter of a million of persons of all ages, above fifty thousand would die of the fever alone, while the deaths during the period from other causes would not be diminished. Further, you will perceive that the remarks which will be made on the population and its circumstances are applicable to many districts in this and other counties; and that the instance of Great Horwood is therefore instructive to many as an example of

the risk in which some people live, who are apparently leading healthy out-of-door lives.

4. The number of cases which have occurred in the parish up to the present date, July 9, whether under the care of the Union officer, Mr. Wynter, or under that of Mr. Newham of Winslow, are 125^a. The deaths have been eighteen. The population was computed in 1851 at 704^b: therefore out of every six persons one has already fallen ill: and of those who have had it one of every seven has died. Not only the numbers of cases of disease from this cause alone, but its hitherto unabated character, and the simultaneous high rate of mortality from other causes, constitute the ground for anxiety.

To you, who have instituted this inquiry, I need not comment on the loss of time, inconvenience, suffering, distress, depression, and anxiety which such a visitation entails on a whole population.

Description of the District.

5. The part of the parish of Great Horwood in which the village is situate, occupies a ridge of the Oolite which runs down from Whaddon Chase in a westerly direction, is bounded on either side by a moderate valley, and terminates by a gentle fall a mile to the south-west of the township.

In each valley a small rivulet runs, generally from east to west; the two rivulets unite at the base of the slope just named, and the joint stream enters the Ouse between Padbury and Buckingham.

There is, therefore, a natural fall to the North, the

^a In this estimate Mr. Wynter does not include mild cases, which might be termed *Febricula*.

^b Singleborough is excluded: there is at present one case in that distant hamlet, whose population was 130 in 1851.

South, and the West. The parish is pleasantly but not excessively wooded, grows splendid corn, and has meadows of rich pasture on undulating slopes.

6. The population is wholly agricultural: the males are generally well employed in field-work. Some females earn, by close industry, from 2d. to 5d. a-day at lace-making. Time was, when a girl of sixteen could obtain from this work 16s. weekly. Rent is paid by the labourers from 9d. to 1s. 6d. weekly. Most of the cottages have some sort of garden. A pig is generally kept. Many are the possessors of a small copyhold property, by which they can barely live; while all the poorer parishioners have allotments on an enclosed common, a mile along a good road from the mass of their dwellings.

There is no large resident landed proprietor. The parish has, within the last few years, greatly improved. Industry and neatness are the rule in the parish rather than the exception; and although, as will be presently seen, there is yet much to be done in reconstructing on a better plan the poorer class of tenements, yet steady progress has been making for some time past by the whole population.

Such is the district and such the people in question. The relation of these facts and circumstances to the prevailing disease will soon appear.

General Character of Fevers.

7. As this paper may fall into the hands of some who are but imperfectly acquainted with the general laws of Fevers, it may be proper to state them in a simple and untechnical manner.

Several of the early symptoms of Fever are common to many disorders; such as disorders of digestion, inju-

ries, &c. But FEVERS, specially so called, as distinguished from "being feverish," imply some causes, or cause, independent of the person affected.

These causes are now known to be for the most part Destitution, Mental depression, Putrescent organic remains, Over-crowding, Deficient ventilation, Contagion. These appear to act with the more intensity, in proportion as they combine together.

Of FEVER there are probably three varieties. Their mortality, causes, and communicability by contagion vary. There is also great difference in different individuals in their susceptibility to the infection, and in the duration of the disease; in the way in which it lays hold of, and in the mode by which it destroys their vital powers; and so also, and necessarily, a great difference in the way in which persons are to be treated.

This difference is caused, not only by the nature of the Fever itself, and, probably, by general laws regulating the course of all disease, from generation to generation, but by the constitution of the person, by his past mode of life, by his present habits of mind and of body, and by the circumstances of his dwelling. Death may be caused rapidly by, as it were, the primary shock to the whole constitution; or more slowly, by either, gradual "wear and tear" or chemical destruction, which cannot be resisted, or, by the establishment of particular injuries in some vital organ, or organs.

8. Recovery may be brought about at various periods from the moment when the poison is received, either by the spontaneous cessation of the chemical changes; or by supplying by art the great waste of the body; by maintaining the general powers of the circulation and of the nervous system; by aiding the repair of injury to organs; by hindering and preventing that injury; and

by placing the sick in the circumstances most favourable for promoting these remedial processes.

Therefore, the duty of the Physician, wherever Fever is prevalent, is, above all, and first of all, if possible to ascertain what originated the disease; 2ndly, by what means it was propagated; 3rdly, in what way to remove the cause, and check the propagation; 4thly, how to bring the existing cases to a happy termination.

Local Characters of the Fever.

9. The disease which has occurred in Horwood would have been called some years since "Common Continued Fever running into Typhus;" it is now designated by the more distinctive name "Typhoid." This is not an occasion to discuss the merits or the nature of the implied difference: it is sufficient for me to state that in well-marked cases "Typhoid Fever" is more fatal than "Typhus." The name unhappily leads non-medical persons to suppose it to be otherwise, and the distinction is specially here mentioned to guard against the error.

On the 29th of June, and again on the 2nd of July, I examined with Mr. Wynter 11 cases. They were fair examples of the early stages of the disease, of its middle, and of approaching convalescence. None were of a severe type; and all may, if no graver complications occur, recover. The ages of the sufferers ranged from 3 to 64. Three were above the age of 40; four were below 20. Five were males, employed in labour.

I am informed by Mr. Wynter and by Mr. Newham, to both of whom I am indebted for the most unreserved communications, that in those cases which have proved fatal they have either been rapidly so, or, which has been more frequent, the disease has come on insidiously, and in the later stages only given evidence of the probable

fatal termination. This termination has, as is very usual in this form of Fever, been preceded by hæmorrhage more or less severe. Death has taken place in the second or the third week, or at a much later period. Partial convalescence sometimes lingers on for many weeks.

With respect to the fatal cases recorded by Mr. Wynter, it is notable that the mortality has been rather less among the parish patients than among those who were in somewhat better circumstances. Many of the lighter cases, indeed, have, under his proper management, stopped in the first week.

In the spring of this year the disease, which began at Michaelmas last year, appeared to abate, to be renewed in May and June.

Presuming that Medical details will here be out of place, I proceed to state what I could learn as specially notable concerning the mode of origin and spread of the disease.

Course of the Disease.

10. As far as can be traced now, either with the assistance of Mr. Wynter or of Mr. Newham, it would appear that the first case appearing about Michaelmas in last year occurred in one of the best houses in the place. Popular opinion traces its origin to Buckingham. The person in question had been there, but without known intercourse with any Fever patient: I understand that there was much Fever in Buckingham at that time.

11. The second case was in the family of a servant who lived in the house where the first case originated. It was not in the person of that servant: the servant had but little to do with the first case; and her sister who was taken ill had not, I am told, been away from

home. The servant eventually returned to her own home, and did not fall ill for a fortnight: all the children, five in number, had the fever, including, last of all, the servant in question: the parents both escaped. The rooms were generally close and too small, but there is no specially marked unhealthy condition in the cottage.

12. It is quite otherwise with the house where the next set of cases occurred. That house was one of the worst in the parish; it was an underlet tenement, and in some respects in a shocking condition. Some of the family slept in an upper room, large enough, indeed, but dirty. The floor is old and bad; the ceiling below is not plastered: the area beneath this floor was nearly a foot deep in black, stagnant water, into which from without filth oozed. All who slept in the room over this swamp were taken ill, and three of the family died*.

13. There was a family connexion between these cases and the neighbours of the next fatal set, which occurred in quite another part of the parish, and a wholly different kind of tenement. In two adjoining rooms of a *Row of new houses* at Nash-End three men died. Other cases have occurred in the same buildings. They will be again mentioned.

14. Shortly after another group of cases sprung up independently of neighbourhood, in another kind of houses,—*old cottages*, not in bad repair, at Singleborough-End. Three are now ill there; one has died.

15. Though I do not venture to ask you to draw

* I desire to record that, when these circumstances were made known to the Warden and Fellows of New College, they immediately took steps to take the property into their own hands, and intend, as I understand, the destruction of the present, and the erection of a new and superior tenement.

absolute conclusions from so slender data, yet it will seem to you most probable that some common cause existed in all these tenements, situated widely apart, to produce the first cases of each, and that either the same cause operating on the other inmates, or else contact with the first case, produced the subsequent illnesses. You will have noticed that autumn, winter, spring, and summer have not seen the entire cessation of the disease.

16. The causes of disease that are common to all these houses after the first, are offensive exhalations of some kind adjoining the houses, coupled with deficient ventilation in all the rooms, and more particularly in the bedrooms: in some of these good health is impossible.

It would be useless that I should extract detailed statements and measurements from my note-book, when you have the originals for examination. There are, doubtless, in Horwood many sanitary errors, but scarce any that may not be found in hundreds of other rural parishes.

In most agricultural districts there are many points connected with the older dwellings, which are either remediable, or irremediably bad. Some have thatch, where they should have slate; half-brick-nogging, when there should be walls of the thickness of a brick and a-half; and porous, absorbent tiles, instead of hard flagstones, or wooden flooring. They have rooms 6 feet high below, or even between the rafters, when they should be 7 feet 6 inches, or 8 feet, as a minimum, below them; the door and the windows are not seldom on one side of the house, whereas there should be a back door and windows at the back for through-draught; they have one casement of the window to open, when they both should be hinged; have a pantry without a window, when it should always have free access to the outer air. Some

have a small ladder to the bedroom, instead of convenient stairs; have one bedroom window a fourth of the proper size *on the floor* below the wall-plate, or on the floor, dormerwise, instead of two against the ceiling; and allow in some instances but half of the cubic air space for two adults, which should be allowed to one. Every such fact may be seen at Wigwell, and many of the individual blemishes may be noticed in other parts of this and most agricultural districts, which have not been thoroughly taken in hand by influential landlords.

Nor is this all: the exterior of many of the dwellings is no less unsatisfactory than the interior. In some instances, even if the dwellings were better than they are, the gardens might cause disease. Into one an open privy overflows: into another the refuse of the house is thrown near the door, and forms a putrescent heap round which the children play; from it the father, as he returns home weary from a breezy field, inhales foulness which blasts the energy that the hours of night should give.

In Horwood, as elsewhere, these heaps are treasured up to be carried away to the allotments.

17. The evils which are here alluded to, and which might be much enlarged upon, were pointed out long ago of Buckinghamshire houses in the Sanitary Reports of 1842:—

“Those who are ignorant of the habits and dwellings of our rural populations, would be surprised to find how little the labouring classes interest themselves in improving their cottages. If a drain be out of repair, it is usually left untouched until the owner of the property becomes aware of the defect; and if the privy becomes full, it is left uncleared until the increase of the soil renders the accommodation inaccessible. To the state of the

privies in the cottage-gardens is attributable more illness than to any other cause. The construction of these conveniences is usually of the rudest character, being nothing more than a hole dug in the ground. In the course of time the soil rises in these holes to the level of the surface-mould, when a natural drain is formed beneath the cottage-floor, which is usually lower than the surface. In a row of cottages on a dry heath in Buckinghamshire, I was surprised to find Fever prevailing amongst the inmates, who were agricultural labourers possessing unusual advantages. The cottages had been constructed with much attention to the comforts of the inmates, yet by the neglect to empty the privies, the vegetable mould beneath the floors had become thoroughly saturated by the drainage; thus producing frequent and serious indispositions amongst the inmates. At Long Crendon, near Thame, the state of these conveniences is as injurious to the health of the inmates of the adjoining cottages as the heaps of decaying animal and vegetable matter at the cottage-doors. Fever is usually endemic in this parish, and during the past year the mortality has been alarming. Mr. Caporn, Relieving Officer of the Thame Union, has taken some pains to point out to the inmates the cause of this mortality, but has not succeeded in obtaining the attention due to the importance of the subject."

"Of one parish it is said, 'There is also a large drain blocked up, the contents of which are in a state of decomposition. These are probably the causes of the prevailing infectious epidemics, and I think require immediate attention.' I could mention other instances of Fever occurring under similar circumstances, but I know of none in which the consequences have been so serious as in the parishes above mentioned. At Long Crendon the severity of the Epidemic has been particularly alarming during the past winter; few families have escaped its effects; and many have lost more than one member."

Again, in the neighbourhood of Stratford-on-Avon:—

"As almost all the cottages in which there has been Fever are thatched, and the thatch in many of them is in a very rotten and insufficient condition, it is not improbable that slow decomposition in the thatch, from the unusual quantity of rain which has

fallen, may have been going on, and contributed to the production and continuance of Fever. It has been observed by others, I believe, that it is more difficult to get rid of Fever in thatched, than in slated cottages."

18. It should not be supposed that in every house all or any of these circumstances are to be noticed, and that they are therefore to be assumed to be the actual cause of disease.

The following history gives probably an example of the two simplest modes of origin of Fever; the one by poisoning through malarious exhalations from the soil, the other by infection from a person already ill. A young man dug the foundations of a wall at Horwood, on a spot where refuse and ordure had been accumulated: he went home to Winslow, and had Fever. Two brothers and a sister have since fallen ill in the same house: all are recovering.

Compare the following. The cottage which I should designate as the most dirty, worst kept, and, on the whole, as far as I know, most unsatisfactory cottage in Horwood, has not had a single death. One inmate had severe Typhus twelve years ago, and more than one minor attack since. Three slight cases have occurred in the house this spring; all the inmates are always unhealthy; but in this endemic though no one has been very ill, no one has been well in it.

19. In truth, where a combination of circumstances is required to produce the disease, people seem sometimes to escape because they are worse off than their neighbours: just as in the thickest of the fight a man escapes, while one only chance shot kills his comrade. A parish house, near the village, which seems positively dangerous, is two feet below the road, and has the floor three inches below the door-sill, is yet airy, because it is large and

out of repair: the bed-room looks miserable, and has no ceiling at the collar; but if it were tiled, from the reasons given, would be healthy enough. I have seen in Oxfordshire and Bucks many such houses, and have heard the people say "they prefer them to (what they call) the better sort: to be sure they look slovenly, but you see, Sir, we have room to move and fresh air."

20. Again, a positive good is neutralized by one carelessness. It has been already said that some of the cottages have no back door: it is almost impossible that a labourer's dwelling should be healthy when this is the case. This fact alone condemns the construction of the new buildings at Nash-End, and renders them dangerous. But on the other hand, a back door may lead to concealed and confined filth, unless there be adequate space behind. Lately, I asked during a sanitary enquiry in a house to which there was a back door, where the privy was placed; the answer was, "Mine ain't a privy, it's a tub!" The tub was among other articles of domestic use at the back door. The object was to collect the manure for the garden.

Or compare these two instances: in one charmingly kept cottage, with a capital garden, the whole of one side of the garden is occupied by a privy, its overflow, a general swamp nine inches deep, for the pig to play in, and his sty; and in the tenement next to it, there is a similar arrangement: there has been scarcely any illness in this house. The pig's swamp is open, (i.e. has no roof,) and the bedroom window is *not* over it. But in another cottage, where the bedrooms are larger, the window is over the roof of a reeking pig muck, and the strong man that slept by the window fell ill and died, and his widow is yet struggling between life and following him.

21. There is no reason for supposing that the aliment

is inferior, or the general condition of the labourers at all worse in Horwood, than in agricultural districts generally. On the contrary. The allotments which were apportioned to them when the common was enclosed have greatly helped them in providing the house. But I fear also that the muck has been treasured up, and more pigs kept close to the cottages, for the purpose chiefly, if not wholly, of supplying manure. The water is obtained chiefly from wells, and is not complained of. That obtained from the rivulets is not fit for drinking, and is not used for it. The south-eastern rivulet, as it flows at the back of Wigwell, is often impregnated with ooziings from cesspools, and from surface refuse.

Summary of Evils.

22. It remains only now to sum up into a few words the conditions which appear to have favoured the continuance of the disease during the last nine months. Reverting to the modes of origin of Fever as stated above, (§ 7.) viz., Destitution, Bodily and mental depression, Contagion, Over-crowded dwellings, Putrescent animal and vegetable matter, and an insufficient supply of fresh air, or, as it is called, bad ventilation, we must attribute in various degrees the persistence of the affection to the last four, and especially to bad ventilation. The evil is most grievous in the sleeping rooms. "We can do no more," said one admirable woman, "than keep clean that which we have. We cannot get our landlord to give us more air, or make the windows we have to open. 'Women,' he said, 'are best shut up.'" Some of the small cottages at Wigwell are models of personal cleanliness and of neatness on the part of the inmates. The fault is not in them, but in their tenements. So offensive do the bed-rooms of some become

in the rooms where the windows are near the floor, that one said to me, "I often awake in the night stifled, and me and my husband go and sit at the window."

Too much stress cannot be laid on this. It is an evil the inmates cannot, with their present education and notions, rectify. It would seem very easy, by borrowing a tool or two, to make, at the cost of a little labour and wit, an additional window in a roof; but "it is the landlord's place" to do it. He does not know of the necessity, or *has not the means*, or doubts the soundness of what he considers sanitary innovations. And as for the father, he comes home tired. From day to day the old small casements remain; no better are substituted, and none are added.

To this I must only append the oft-repeated caution against accumulations of muck, filth, and piggeries close to human dwellings. The great difficulty of the case must be admitted. Custom, convenience, poverty, want of time, family cares, and close living, combine against the formation of the energetic moral habit which leads to the most healthful state of body and mind. With many, therefore, the difficulties of living are barely surmounted; complete mastery of their circumstances is seldom attained.

Remedies proposed.

23. From the statement of the causes which has been made, the remedies become self-evident.

In all cases of Endemic disease, there are two main objects:—1st, to treat the sick, and, 2ndly, to check the formation of new cases of illness.

1. Under the first head, the treatment of the sick, you have no great difficulty. With an attentive and zealous medical officer, who has not spared himself, the poor have

the advice they stand in need of. But for placing them favourably for following our advice, two things are necessary; 1st, a Superintending nurse or nurses, as the numbers may require; 2ndly, either the removal of the sick person to an airy apartment, or of the rest of the family to another house.

You have no difficulty about a nurse, as an old Crimean nurse is already gone to you, and more can be supplied from hence, or from elsewhere, should you need them: and for the second recommendation, a barn might be borrowed; at this time of the year it would furnish all the comfort you need. At all events, it is certain that as many of the roof attics at Nash-End, Singleborough-End, and Wigwell now are, a due supply of air, without an alteration in the windows, is impossible.

The ministrations of the esteemed Rector, to whose devotedness the parish is largely indebted, will supply his flock with confidence and cheerfulness; these will conduce to that vigorous state which is calculated as well to resist the infection as to battle with the disease.

2. With respect to the secondary measures, those for checking the disease, they also have been implied, and may be briefly summed up.

The removal of sewage from an agricultural village is attended with particular financial difficulty. Your legal adviser will, however, best explain the methods and the powers by which funds may be raised under the existing Health Acts. Horwood has remarkable facilities for systematic drainage. These I have pointed out above (§ 5). The cost, therefore, would be less, and the result much better than in many villages.

If this is not to be done, the next step is to take measures for a methodical removal of the "soil," and for

a cleanly manner of keeping it till it shall be removed. The Chinese are great adepts at ensuring the removal of human ordure; and it was carried to some perfection in France^a. I have no doubt that by energy and attention on the part of an inspector, or with a small subscription, measures might be taken to ensure the regular cartage of all refuse that can be useful in agriculture to the allotments or grounds where it is needed, with advantage to the cultivator as well as the inmates. In truth, as is well known, in some places these heaps are purchased. Any way, the object to be aimed at is a system of complete removal at short intervals.

When the exterior of the labourers' dwellings has been so provided for, the next desideratum is a dwelling which will be healthy, if kept with care. A neat cottage has been built near Wigwell, and is beautifully kept. But it is nearly on the model of the old traditional kind; being so, clearly, from want of knowledge, and not of intention. There is no back door: the rooms are somewhat higher, but the bedroom windows are far too small, ill placed, and insufficient, and the ceiling under the collar should be pierced with a ventilating trap-door.

A model Cottage is required.

It is of the utmost consequence, in a sanitary point of view, that in every district there should exist a good cottage built at the *cheapest rate*, to serve as a model to small capitalists and tradesmen who are willing to invest in this kind of property, and as a pattern to the labourers of the kind of cottage they ought to have. I take leave, therefore, to add, in a few propositions, the essentials for a healthy cottage. The grounds of the

^a See Appendix to Memoir on the Cholera at Oxford. (London and Oxford: Parker. 1856.)

several recommendations are generally very obvious, and I should fear to trespass on your patience if I were to do more than enumerate them.

1. Detached or double cottages are preferable to rows. Double cottages are perhaps the best of the three.

2. The angles, not the sides of the building, should be placed to the cardinal points of the compass. They should stand *in* their garden-plot; that is, a little way back from their front boundary.

3. The garden should, if practicable, contain from a sixth to a quarter of an acre. The privy should be at the extreme end, with the pigstye, if there be one. But as it is very doubtful whether a pig is a real advantage to a small family living on small means, and as it is certain that a pigstye is in itself an evil, pigs are to be tolerated, not to be encouraged.

4. A drain from the back door to the privy should carry surface drainage and house slops to the privy; and thence, wherever practicable, another drain should lead to a tank for distribution by the farmers to meadow or other land. The cesspool should be a closed one, if a cesspool is necessarily employed, and agreements should be made for periodically removing its contents.

5. Tiles or slates, not thatch, are to be used.

6. A back as well as a front door: windows on both sides of the tenement, if in a Row: or on three sides, if in a double cottage. A window in the pantry. Both sashes to open, where sashes are used; both casements, where there are double casements.

7. The day-room to have in its chimney an Arnott's valve or the better arrangement of an air-tube or separate flue. The floor to be made with a boarded centre, and hard flags or tile border, set in concrete. The floors to

be raised six inches at least above the outer road. The back room to have a fireplace, with boiler attached, and a well-trapped sink.

8. Three bedrooms: for parents, boys, and girls. Height of rooms to be eight feet: the top of the windows to reach the ceiling. A fireplace to be in one room at least, a ventilating tube in the other two.

9. The front room and bedrooms to be approached from a porch: the staircase may be open to the room, or closed.

10. Where rows of houses exist, a washhouse in common to all the houses should be contrived in the garden. It may be next a row of wood-houses and tool-houses, and have a common pump adjoining. Its drain to go to the privy*.

11. The privy in every case to be removed as far as possible from the water supply.

* It would be needless to go into further detail: ample information may be obtained, as well as specifications of small dwellings, from the Sanitary Report, 1842; from the admirable volume, "The Dwellings of the Labouring Classes," by Roberts; and from the Journals of the Royal Agricultural Society, and also of the Bath and West of England Agricultural Society, for the year 1856 (Ridgway, London); in these valuable periodicals, copious details are given in a prize essay by Mr. Isaacs of Bath.

There is reason to believe, as I have said, that New College will shortly erect some excellent cottages in Great Horwood; some model cottages have been long desired in Oxford; see my "Memoir on Cholera in Oxford." On almost every gentleman's estate there are examples: on Sir H. Verney's, or, further off, at Windsor, on the Bedford estates, the Pusey estate, Sir Robert Throckmorton's, Lord Radnor's, &c.; but I do not insist on these, because my object is to aid small capitalists and small builders in the way of making the cottages which they build as free from fault as possible, and at the same time of making them serve as investments as well (if this word may be used) as they do now.

Summary of Remedies.

24. In conclusion, the following recapitulation and summary is added of remedies suited to your present emergency.

I. IMMEDIATE MEASURES.

a. *Treatment of Sick.*

1. Removal of early cases to more airy apartments: a barn may easily be made more comfortable than some of the rooms; and more healthy to the wife or attendants.

2. When the removal of sick cannot be effected, removal of the other inmates, except those required for nursing.

3. Immediate washing of linen, by a paid washer-woman, the clothes being boiled in a copper.

4. A competent superintendent nurse to go round to the cases daily to see that the advice of the Medical Officer is carried out, and to report to him where, why, and in what there is difficulty in obeying him.

b. *Management of Dwellings, and Inspection of Nuisances.*

1. To recommend to every house where the disease has existed systematic cleaning and lime-washing. When the inhabitants have not the means, to do this for them without charge; urging upon them the burning of accumulated rags and useless stores, which will in some houses be found.

2. To remedy, as far as possible, every privy in a dirty condition, by burying its soil with lime; and when it adjoins the houses, to remove it to a greater distance: to clear and trap existing drains, especially at the cottage doors.

3. To examine the ground near the bedroom windows, and to remove any putrescent matter.

4. To open small dormers near the ceiling of such

bedrooms as are in the roof, in close and small cottages; and to open trap-doors in the bedroom ceilings which are applied at the level of the collar in the same cottages.

II. PROSPECTIVE OR PREVENTIVE MEASURES.

a. Drainage.

To consider seriously, the feasibility of a systematic drainage of the parish in connection with the two natural streams, and the formation of sewage-tanks on those streams for the adjoining lands, such tanks to be placed at a distance from houses: to drain thereby or fill up stagnant ponds and ditches near houses.

b. Houses.

1. To improve permanently the ventilation of the existing rooms, by opening and enlarging casements, adding Arnott's valves, or ventilating tubes; to remove pig-styes and privies as far as possible, especially from the bedrooms; to emend the privy arrangements, where possible, on Isaacs' method. (See "Royal Agricultural Journal," 1856.)

2. To promote the erection of cottages of greater height, and constructed on the principles above described (§ 23).

It is not consistent with observed facts here or elsewhere to conclude that Fever necessarily accompanies foul drains, or even over-crowded, unclean, and unventilated dwellings; but for the practical purpose of caution, and for the inculcation of a just dread of these conditions, it is equally cogent if we are obliged to believe that Fever may originate at any time in such circumstances; that they are exceedingly favourable to its development; that where Fever has once entered, its ra-

vages may continue for many months; and, which is most alarming, that, when it has entered a district, the danger is no longer confined to the unclean and the careless. In the great Epidemics of Scotland and Ireland this has been abundantly and repeatedly proved.

It is my duty further to remind you that there is no doubt that the disease may be carried, and is habitually carried, from place to place by persons moving to and fro; by residents; by vagrants; and that when so imported, it establishes a fresh centre of disease. This has actually happened, as I have before stated, at Winslow. That centre, then, begins afresh. If it happen to light on a dwelling not favourable for further development, it is well. If it light on a house where all is ready for the formation of the poison, the whole family may be consumed, and a newer and more potent ferment is established for that neighbourhood.

They who know and believe these things are separated by a wide gulf from those who do not know or who do not believe them. They who do not know or believe, or who, knowing and believing, persuade themselves that themselves will be exempt, turn aside from the Physician who strives to PREVENT, as from a Visionary, and so wait the time when the only thing left is to cure what might have never occurred, and is then perhaps incurable.

I have the honour to be, Sir,

Your obedient Servant,

HENRY W. ACLAND.

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Chairman of the Winslow Board of Guardians.

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ON

THE SANITARY CONDITION

Of the Parish of St. Mary, Islington,

DURING THE YEAR

1858.

BY

EDWARD BALLARD, M.D., (LOND.)

LICENTIATE OF THE ROYAL COLLEGE OF PHYSICIANS;

FELLOW OF THE ROYAL MEDICAL AND CHIRURGICAL SOCIETY OF LONDON, ETC.;

MEDICAL OFFICER OF HEALTH.

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TO THE VESTRY OF ST. MARY, ISLINGTON.

GENTLEMEN,

1. My Third Annual Report, that for 1858, which I have now the honor to submit, will consist of the usual summary of the Sanitary condition of the parish during the year, and of the work that has been accomplished in the removal of nuisances and of various other conditions calculated to affect injuriously the health of the inhabitants of the district. Latterly the public ear has unfortunately become familiar with the name of a disease to which it had hitherto been unaccustomed, and with the appearance of the term "Diphtheria" in my monthly reports, the unusual prevalence of sore throats has excited much inquiry into the nature of that malady and the circumstances under which it becomes developed. It was clearly my duty to study the disease and to avail myself of all the advantages for this purpose that my position afforded me. As soon therefore as it became apparent that it was making way as an epidemic in Islington, I set myself to inquire into the particulars of each fatal case, both at the house where it occurred and from the medical gentlemen who attended it. I desire thus early to make my public acknowledgment of the services which these gentlemen have rendered me in replying immediately and most courteously to the questions I addressed to them. Several of the cases came once or oftener under my own observation. It has appeared to me desirable to present you with a somewhat detailed account of the disease as it appeared in Islington, although in order to do so it becomes necessary to abridge other portions of my report.

MORTALITY.

2. Following my usual practice, I have thrown the particulars of the mortality that has occurred in the parish into the form of Tables, which will be found at the end of the Report, and which are comparable with those of former years. Excluding persons from other parishes who have died in the Fever and Small-pox Hospitals, 2,642 individuals died in Islington between the 1st day of January and the 31st of December, 1858. We may calculate that, in addition to these, about 149 others fell victims to disease or accident in the various general and special Hospitals, Lunatic Asylums and Lying-In Hospitals of the Metropolis, which receive patients from all parts of town indiscriminately. Adding these, our mortality for the year will be raised to 2,791, an excess of 235 above that of last year. An unfavourable circumstance attending this comparison is the fact that whereas the births in 1857 exceeded the deaths by 2,035, they exceeded them in 1858 by a much smaller number, namely 1,771. The registration of births, however, might have been unusually imperfect.

3. The proportion of the entire corrected mortality which must be estimated to belong to the West Sub-district is 1401 and to the East 1390. With the exception of about 30 deaths, the whole excess of mortality of the year occurred in the East Sub-district.

4. The weekly deaths varied from 31 (in the week ending August 21st) to 92 (in the week that ended on December 4th). The mean weekly mortality of the first quarter of the year was 55, of the second 44, of the third 43, and of the fourth quarter 60.

5. Of the 2,642 persons of whose deaths I possess particulars, 1,280 were male and 1,362 females.

CAUSES OF DEATH.

The causes of death of 2,642 persons are detailed in Table 1. As usual the diseases of the zymotic class stand at the head of the list as occasioning the largest number, and next to them stand the pulmonary and tubercular diseases.

a. Zymotic Diseases.*

6. Of the 2,642 deaths 721 arose from diseases of this class. This is a per centage of 27.2. In 1856 the per centage was 23.6, and in 1857 it was 22.4. It is in the Eastern half of the parish that the excessive zymotic mortality has prevailed. The difference in this respect between the two sub-districts is too great to be readily explicable on any other hypothesis than the really greater prevalence or greater fatality of these diseases in this sub-district. The per centage in the West Sub-district was 23.8 and in the East 30.6.

7. The greatest weekly zymotic mortality, viz. 24, occurred (Table II.) in the week which ended November 27, at the period therefore of the most unseasonable cold (See Monthly Report, No. XX.) In the following week it reached 22. It is remarkable on a survey of the year how little effect the excessively hot weather in June had upon the mortality. In the week ending June 19th, the hottest week of the year, the total mortality did not exceed 45, and the zymotic mortality was only 8. It is true that the zymotic deaths became more frequent in the following weeks, but they never attained a high number.

8. The most fatal diseases of this class were Hooping Cough which occasioned 159 deaths, Scarlet Fever which occasioned 116 deaths, Measles 98, Diarrhoea 90, Fever 65, and Diphtheria and epidemic sore throat 59. All of these, with the exception of Measles, carried off by far the largest number of persons in the East Sub-district.

Hooping Cough has been unusually prevalent throughout the year, although subsiding a little in the summer months. The largest monthly mortality from Hooping Cough, viz. 27, occurred in the five weeks which succeeded the very cold weather in November.

Scarlet Fever, which only occasioned 13 deaths during the first half of the year, began to present an epidemic character about the end of July, and between this time and the close of the year carried off 103 persons, of whom all but five were under 10 years of age. Its progress week by week is shown in Table II. The mortality suddenly subsided in Islington after the cold weather of the last week in November.

The deaths from Measles were slightly in excess of previous years in the 1st, 2nd, and 4th quarters.

Considering the excessive heat of the summer and the prolonged dry weather, it is highly worthy of remark that Islington, in common with the rest of the Metropolis, enjoyed a comparative immunity from the bowel complaints which we annually look for at that season. When Cholera approached this island so closely as it did in 1857, and when an outbreak to a limited extent actually took place in a metropolitan suburb, we certainly did look with anxiety to the progress of events in the succeeding summer. The result in Islington was that only 59 deaths from Diarrhoea occurred in the quarter against 99 in 1857 and 78 in 1856.

The deaths from Fever have also been reduced in number, since they only amounted to 65. In 1857 they were 84, and in the previous year 74. Another important and interesting fact is that true Typhus has been almost completely absent from London.

* "This class comprises diseases which have been observed to be epidemic, endemic, or contagious. The blood is probably, in the greater number of them, the primary seat of disease; and they may be considered, by hypothesis, the result of specific poisons of organic origin, either derived from without or generated within the body."—*Statistical Nomenclature*, 1848, p. 14.

The form of fever which has been met with is "Typhoid," a distinct disease characterized by certain specific changes in the glands of the intestines. This form is less infectious than true Typhus. Out of all the cases of continued Fever admitted into the London Fever Hospital only 15 were Typhus while 180 were Typhoid. From the week ending May 1st to the end of the year, although 132 cases of Typhoid entered the Hospital, only a single case of Typhus was received, and that not strongly marked. Recent researches appear to shew that Typhus mainly originates in overcrowding and destitution, while Typhoid is connected more or less closely with the emanations from decomposing organic matter and organic impurities in the water that is drunk. The very existence of this disease may indicate to us that although much has been done to purify the atmosphere about our habitations and to improve the condition of our drinking water, there still remains much to be effected in the same direction.

I reserve my remarks upon Diphtheria to the close of the Report.

b. Tubercular Diseases.

9. Tubercular diseases, including Pulmonary Consumption, carried off during the year 408 persons, or 15.4 out of every hundred that died from all causes. This proportion is unusually low; and there can be no fallacy about it, inasmuch as the actual number is less than in previous years. The reduction that took place, however, was only observed in the West Sub-district.

10. Consumption alone occasioned 316 of these deaths, or 11.9 per cent. In the East Sub-district, the mortality from Phtisis was higher than in either 1856 or 1857; the improvement observed having reference only to the West Sub-district.

c. Diseases of the Organs of Respiration.

11. The mortality from acute and chronic pulmonary affections (exclusive of Consumption) was unusually high, especially in the first and fourth quarters of the year. It will be recollected that the close of 1857 was marked by a slight epidemic of influenza, which had not entirely subsided when the new year commenced; and the deaths in the fourth quarter were concentrated almost into the last six weeks of the year, commencing with the cold weather at the end of November. Of the 494 deaths from these diseases, 238 were of males and 256 of females, constituting together 18.6 per cent. of the deaths from all causes. In the West Sub-district there were 264 deaths, or 20.1 per cent. of all deaths; and in the East Sub-district 230, or 17.2 per cent. of all deaths: in each case the mortality was higher than prevailed in either of the two previous years.

AGE AT DEATH.

12. Table I. shows the age at death in 2,641 instances. It will be observed that of this number 1,238, or 46.8 per cent. of all deaths, occurred under five years of age. Of these children, 648, or more than half, died during the first year of their existence, and 276 before they had completed their second year. The diseases which have been most fatal to children may be thus arranged:—

	Deaths.		Deaths.
Diseases of the organs of respiration of an acute character	261	Atrophy	74
Hooping-cough	149	Debility from premature birth...	47
Convulsions, and other cerebral affections.....	105	Hydrocephalus	43
Measles	92	Violent causes	40
Scarlet fever	82	Epidemic sore-throat	33
Diarrhoea	80	(Diphtheria).....	30
		Teething	27
		Tubercular	27

13. In order to remove the causes of a high mortality, it is not only necessary to know what are the diseases or classes of diseases that occasion it, but also where, in what parts of a district, and among what ranks of our population the fatality exists. Much of the time of an officer of health is necessarily devoted to this inquiry; and in each of my reports I have endeavoured to represent as clearly as I could, in various tables, the results that I have been able to arrive at—not only with respect to the mortality generally, but to that of infants in particular, and of those classes of disease which occupy the most prominent places in the returns. With a view to indicate the relation borne by the mortality under these more important conditions to the rank in life of the individuals, I have this year a contribution of a different kind to offer. From the street-list that I keep, I have taken out the names of 109 streets and places, the houses of which, I know, are occupied almost uniformly by single families in easy circumstances, and counted up the deaths in them during the three years (1856-7-8) from all causes, from zymotic and tubercular diseases, and also of children under five years of age. I have formed another list of 150 streets, &c., the houses in which I know to be either let to two or more families, or occupied by those of labouring men. I have compared the totals of deaths in these two distinct classes of our community, and here is the result:—

	No. of Streets, &c.	Total Deaths in 3 years.	Deaths from Zymotic Diseases.	Per Ct. of all Deaths.	Deaths from Tubercular Diseases.	Per Ct. of all Deaths.	Deaths under 5 years.	Per Ct. of all Deaths.
CLASS I. Occupied by the wealthier part of our population . .	109	784	183	18.1	107	14.5	216	29.4
CLASS II. Occupied by the less wealthy, or labouring part of our population	150	2237	687	50.7	333	14.8	1333	69.8

14. These numbers will almost speak for themselves. Of course they do not indicate the absolute liability of the two classes to suffer from these forms of mortality, but only of their liability to them relatively to the mortality from other diseases. The former could only be settled by a comparison with the number of persons of each class living—a datum which I do not possess. I may state that I selected the streets for comparison as fairly as I could, according to my knowledge of the nature of their occupancy, taking into account none but those respecting which I was certain.

SICKNESS.

The Table of Sickness (Table IV.) that is given, as it presented itself at the several Institutions from which I receive returns, and among the poor who are attended by the Parochial Surgeons is constructed in such a manner as to show a total only in respect of those cases which can with strictness be said to belong to our Parish, the sickness in the two Special Hospitals being placed in columns apart from the rest. The only comparison with the returns of the previous year that I can institute concerns the sickness at the Workhouse, the Infant Poor-house and the out-patient paupers at large; selecting as in Table V. some of the more important forms of disease. This Table renders apparent the greater prevalence during the year of small pox, chicken pox, measles, scarlet fever, hooping cough, erysipelas, thrush, ague, and acute diseases of the organs of respiration. On the other hand diarrhoea was reduced to nearly half the number of cases met with in 1857, and there was a very much smaller number of continued fever. Of the 353 cases of small pox that entered the Hospital on Highgate Hill 32 were from the Parish of Islington. There has been during the year an increase of this disease throughout the Metropolis.

I am informed by Dr. Scurrah that not a single case of true Typhus has been received during the year into the London Fever Hospital from any part of this Parish.

SANITARY IMPROVEMENTS DURING THE YEAR.

15. I have thrown into a tabular form (Table VII.) a summary of the house improvements effected under the orders of the Sanitary Committee and to the satisfaction of the Inspectors, from the period of my last Report to the 25th of March, 1859. Nearly all these works were completed without the necessity of appealing to the authority of the Police Magistrate. In addition, however, to the work thus specified, I have to mention the abatement of overcrowding in several instances which came under my notice, and concerning which I certified to the Sanitary Committee that the amount of space allotted to each inhabitant was below that requisite for the maintenance of health. At one house in the West Sub-district, where a murder and suicide occurred during the hot weather of June, the nuisance that arose was so intolerable that the inspector was compelled to destroy the beds and bedding and to superintend the cleansing and disinfecting of the house. It is unnecessary to specify also the removal of a variety of noxious accumulations under the superintendence of the Sanitary Inspectors, of which complaint has been made from time to time at the offices.

16. The Slaughter-houses throughout the parish were visited in the month of September, and, with but few exceptions, were found to be in a satisfactory condition. The butchers, who kill upon their own premises, are now generally aware of what is required of them for the preservation of cleanliness and the avoidance of nuisances in their trade.

17. One of the most important works of the year has been the improvement of the Vaults beneath the Parish Church. I mentioned in my last report that they had been visited by Mr. Grainger from the Burial Acts Office, under the orders of the Home Secretary. After several conferences between myself and that gentleman, and full canvassing of the various plans that might be adopted, it was finally arranged that the coffins should be placed in a single, and in some places in a double layer upon the floor of the vaults; that they should then be covered with a layer of gravel to the depth of eight inches, and that over this should be placed a layer of concrete four inches in thickness. The private vaults were to be subjected to the same treatment as the public ones. This plan was submitted to the Vicar and Churchwardens, and having received their ready acquiescence, was made the subject of an order in council on the 26th of February. A public notice to that effect was given and opportunity afforded for the removal of any remains prior to the 26th of March. The arrangement, however, was so completely consonant with the feelings of the friends of those who were there deposited, that no notice of removal was received, and the work was accordingly proceeded with by Mr. Pelling. Mr. Churchwarden Jenkin, who manifested great interest in the proceeding, and myself visited the vaults daily and sometimes twice a day, and although from the decayed state of the coffins and the leakage from them of putrid fluids, the workmen were exposed to unquestionable danger on several occasions, the precautions adopted and the free use of Macdougall's disinfecting powder enabled the whole to be completed without a single casualty. In some respects this was an experiment; it was this first work of the kind instituted in the Metropolis under new powers vested in the Privy Council, and we were all quite in the dark as to the manner in which the suggested improvement would be received by the public. It was so successful as to constitute a precedent for similar improvements which have since been carried out in other Metropolitan Churches.

18. An order in Council for the levelling and planting of Jones's Burial Ground, in Church Street, has proved abortive from the absence of any power to enforce

obedience. Rubbish of all kinds has been carted into it during the whole year, and matters still more objectionable have on various occasions been deposited there. It is perfectly clear that the law must be amended before these disgraceful proceedings can be brought to an end.

TRADE NUISANCES.

19. But little has been done during the year in the matter of nuisances arising out of trades. In one or two instances where complaints have been made, I have communicated either personally or by letter with the parties complained of, and the cause of complaint has been done away. For the suppression of the nuisances proceeding from the group of manufactories at Belle Isle, the sitting magistrate at the Clerkenwell Police Court has repeatedly recommended indictment at the sessions; for, notwithstanding that some means of arresting the offensive emanations have been generally adopted, and as a consequence the nuisances are not so intolerable as they were three years ago, there still remains such an amount as to inconvenience the inhabitants of the surrounding district. This recommendation met with the concurrence of the Vestry. The selection of a case for trial arose in the ordinary course of events out of the first official complaint of a Belle Isle nuisance made at the office subsequently to the resolution of the Vestry. On this complaint an information was first laid at the Police Court against Mr. Schweizer, the varnish manufacturer, under the provisions of the Parish Act. A difficulty having arisen as to the applicability of this statute, the magistrate again suggested the propriety of an indictment; and as the counsel retained by the parish on that occasion advised the same course, the recommendation to indict was adopted by the Vestry. The burden of providing evidence such as will satisfy a jury now lies, where it always ought to have lain, with the complainants. It should suffice them in such a case as this, that the Vestry is prepared to advocate their cause as that of the parish generally, if they will themselves take the trouble to ascertain unquestionably, and to the satisfaction of your legal advisers, the particular establishment which occasions their discomfort.

EPIDEMIC SORE-THROAT AND DIPHTHERIA.

20. In order to be in a position to present as complete a history as possible of this epidemic malady as it has occurred in Islington, I have tabulated all the cases which proved fatal in the parish from January, 1858, up to the close of the first quarter of 1859. The Table which I have thus formed relates to 80 cases.

21. Assuming the presence of the peculiar exudation upon the interior of the throat as the characteristic sign by which true diphtheria is to be distinguished from the other forms which epidemic sore-throat has presented, I have taken the pains to ascertain by careful inquiry in how many of the 80 fatal cases this symptom was present. I obtained the information I desired in 67 instances. In the remaining 13 I failed to do so: most of them were the earlier cases in 1858. I propose, then, to divide the 80 cases into three classes:—Class 1. Those in which I have obtained satisfactory evidence of the presence of the exudation, or in which I saw it myself.—Class 2. Those which were certified as deaths from Diphtheria by the medical attendant, but in which I have obtained no particulars of the appearances in the throat, and Class 3. Those in which I am assured by the medical attendant that the exudation was absent. These last cases were returned as deaths from "Tonsillitis," "Quinsey," "Gangrenous Quinsey," "Inflammatory Sore-throat," "Gangrenous Sore-throat," and

in one instance as "Diphtheria." With respect to Class 2, I may say, that whereas out of 58 deaths returned as from Diphtheria there were only 2 in which the peculiar exudation and other symptoms of the disease were ascertained, by inquiry, to have been absent; it is probable that of the 13 not inquired into the majority, if not all of the cases, were really instances of the true disease. At all events, I can not agree with those who hold that the term "Diphtheria" has been loosely applied by the medical practitioners in this parish to sore-throats of all kinds. The numbers of each Class stand thus:—

	Males.	Females.	Total.
Class 1	31	25	56
Class 2	4	9	13
Class 3	6	5	11
Total	41	39	80

By far the greater number of deaths from epidemic sore-throat have been of that class which presented the characteristic marks of true Diphtheria. Of these the male deaths have somewhat exceeded those of females, (see also Sect. 24), the proportion however being brought nearly the same if the cases of Class 2 are included.

22. Age.—The ages of the 80 persons varied from 16 days to 61 years. There were 5 infants who died under 1 year; they were aged respectively 16 days, 5 weeks, 8 months, 9 months, and 9 months. It is remarkable that all these were male children. There were 6 persons who died at ages above 20 years, and these were aged respectively 26, 29, 37, 49 and 61 years; all but one of these were females. The deaths were distributed among the ages thus:—

Ages.	Under 1 year.	1—	2—	3—	4—	5—	10—	20 and upwards.
Class 1	4	9	7	10	6	14	2	4
Class 2	—	2	2	1	1	4	2	1
Class 3	1	1	2	2	—	1	3	1
Total	5	12	11	13	7	19	7	6

Of course, unless numbers such as these are compared with the numbers living at each age, no accurate conclusion can be drawn as to liability of the different ages to fatal attacks of this epidemic. Apart from this comparison, however, the following facts appear deducible.—1st. That the disease has been comparatively rarely fatal to infants in their first year of life.—2nd. That it has been chiefly fatal to children under 10 years of age.—3rd. That up to 10 years of age the greater number of fatal cases were in males; the numbers being 39 males and 25 females: that at ages above 10 years the preponderance was disproportionately on the side of the females, the numbers being 2 males and 11 females.

23. *Social Position.*—Dividing the cases into two groups, according to the plan adopted at p. 6 of this report, I find the following result:—

	Class 1.	Class 2.	Class 3.	Total.	Mean Mortality of these years in streets, &c., where those deaths occurred.	Per centage of Deaths from Epidemic Sore-throat.
Superior Group	36	7	7	50	142	35.2
Inferior Group	20	6	4	30	124	24.2

Here, then, we meet with the very reverse of that which obtains in the instance of other zymotic maladies, (p. 6). The numbers favour an idea which has been floating in the minds of many medical men that I have conversed with, viz., that epidemic sore throat and diphtheria attack the middle and comfortable classes of our population in preference to those who occupy the lower walks in life. The above proportion is preserved when merely the true and reputed deaths from diphtheria are compared with the mean mortality of the streets in which they occurred, the numbers then being for the superior group 32.8 per cent., and for the inferior 21.6 per cent. Taking the ascertained deaths from diphtheria alone, viz., Class 1, the proportion for the superior group is 32.1 per cent., and of the inferior 23.5. The fatality of epidemic sore throat appears to have been half again as great in the middle as in the lower ranks of society, where a comparison is made with that arising out of diseases of all other kinds. The absolute frequency of the disease in the two groups is another question for the solution of which I do not possess the necessary data.

24. The following Table represents the frequency with which the disease in the 80 cases is stated to have occurred *primarily or secondarily to other maladies*:—

	PRIMARY.	SECONDARY TO				
		Scarlatina.	Measles.	Whooping-cough.	Croup.	Influenza.
Class 1	47	5	2	1	—	1
Class 2	9	1	1	1	1	—
Class 3	10	—	—	1	—	—
Total	66	6	3	3	1	1

It is to be observed, however, that in the certificates of death furnished to the registrars by medical men, the secondary disease of which a patient died is not constantly entered. Of the primary cases in Class 1, 29 were males and 18 females.

25. *Duration of the Disease.*—In only 2 cases did I fail to ascertain this point, which is represented in the following table:—

	DAYS.									
	1	2	3	4	5	6	7	8 to 14	15 to 21	22 to 28
Class 1	—	1	7	4	6	5	5	18	6	4
Class 2	1	1	1	1	2	1	2	3	—	—
Class 3	—	—	—	3	—	1	1	4	1	—
Total	1	2	8	8	8	7	8	25	7	4

The sex scarcely affected the duration of the fatal cases, and the age of the patient appeared to have equally little influence over it, except in the instance of the 4 infants under 1 year, included in Class 1. The mean duration in these four cases was 4 days; at each of the other ages the mean duration before death was from 9 to 11 days.

26. *Mode of Death.*—In 59 of the cases inquired into, the mode of death was stated by the medical attendant as follows:—

	Class 1.	Class 2.	Class 3.	Total.
From extension of the disease to the larynx, oedema of the glottis, or suffocation	22	—	5	27
Sudden	4	—	1	5
From exhaustion	20	1	2	23
" disease of the brain	1	—	—	1
" convulsions	—	1	1	2
" suppression of urine and dropsy	1	—	—	1
Total	48	2	9	59

These numbers will also assist in determining the periods of the disease at which these several modes of death are imminent. The following table embraces 58 cases:—

	1st Week.				2nd Week.				3rd Week.	4th Week.
	Laryngeal Complications.	Sudden.	Exhaustion.	Brain Disease.	Laryngeal Complications.	Exhaustion.	Convulsions.	Suppressed Urine.	Laryngeal Complications.	Exhaustion.
Class 1	15	2	7	1	7	7	—	1	1	2
Class 2	—	—	—	1	—	—	—	—	—	—
Class 3	3	1	—	—	1	1	1	—	—	1
Total	18	3	7	1	8	8	1	1	1	3

The danger from which a fatal result is mainly to be apprehended in the course of the first week of the disease, is extension of the latter to the upper part of the air passages, and consequent suffocation. The sudden deaths in this week are probably due to the same cause giving rise to spasmodic closure of the glottis. As the malady advances into the second week the chances of death from this cause are only equal to those from the general prostration of the vital powers. In the third and fourth weeks, the latter is the condition chiefly to be dreaded; the sudden deaths at this time are probably due to syncope or fainting.

The following table is constructed to show the mode of death of 59 patients at various ages.

	AGES.									
	Under 5 years.				5 to 9 years.			10 to 19 years.		
	Laryngeal Complication.	Sudden.	Exhaustion.	Convulsions.	Laryngeal Complication.	Exhaustion.	Suppressed Urine.	Laryngeal Complication.	Sudden.	Exhaustion.
Class 1	14	4	12	—	7	4	1	—	—	2
Class 2	—	—	—	1	—	1	—	—	—	—
Class 3	3	—	1	1	—	—	—	1	1	1
Total	17	4	13	2	7	5	1	1	1	3

27. *Relation of Diphtheria to Scarlet Fever.*—This is clearly not the proper place for the discussion of theoretical questions, and I shall therefore address myself merely to the practical inquiry whether scarlet fever and diphtheria are or are not mutually protective against each other, as they would be expected to be, if merely phases, as some suppose, of the same disease. The following facts may be regarded as a contribution to the solution of this question; and in themselves point to a reply in the negative.

1.^o In 7 cases of class I the diphtheritic sore throat was stated to have been associated with recent scarlet fever. In only 2 of these cases did the rash appear in the course of the throat affection. Out of the remaining 5 cases in which the rash appeared first, and the diphtheritic sore throat secondarily to it, the rash had in 4 cases left the skin before the exudation upon the mucous membrane made its appearance.

2.^o In 47 cases I was able to obtain satisfactory information as to the patient having suffered from scarlet fever at more distant periods. The following represents the proportion of those who had and had not thus suffered.

	Had Scarletina previously.	Not had Scarletina previously.	Total.
Class 1	9	29	38
Class 2	1	2	3
Class 3	2	4	6
Total,	12	35	47

This proportion of 1 to 3 does not I imagine differ greatly from that which would be found to exist among persons selected in any other manner from the same ages, and the same station in life. In the 12 patients who had had scarlatina, the occurrence of the disease was dated back to periods varying from 6 months to 6 or 6 years. Now it may positively be asserted that of the cases of scarlet fever that are met with in practice, second attacks of the disease do not constitute one-third or any proportion approaching this. A second attack of scarlet fever is indeed so uncommon, that well authenticated cases are among the rarest occurrences in medical experience.

3.^o Neither is the occurrence of an attack of diphtheria a protection against scarlet fever. The following instance is an illustration. Three children of a family in the West Sub-district, were attacked with Diphtheria in August, 1858, two of them died, and the third, aged 3 years, recovered. I satisfied myself on visiting these patients, that they were cases of true diphtheria. In January, 1859, the child that recovered was attacked by scarlet fever, after playing upon a carpet brought from a house where this disease had been fatal. There was the usual throat affection of scarlet fever, but no diphtheritic exudation, and the child died.

I may mention in passing, that among all the cases fatal, or otherwise, that I have investigated, I have not met with any instance in which diphtheria has been apparently communicated from a patient with scarlatina, except where the latter disease has been communicated also. Neither have I at present met with more than one instance in which cases of diphtheria and scarlet fever occurred in the same family about the same time, in such a manner as to favour the idea that they were varieties of the same disease.

28. *Relation of Diphtheria to other forms of Sore Throat.*—The prevalence of other and non-diphtheritic forms of sore throat during the past year has been matter of general remark. Congestive and Inflammatory sore throats have appeared to bear the same relation to diphtheria as diarrhoea bears to cholera in epidemic seasons. With respect to the latter diseases, the general opinion in the profession is, that they arise from the operation of the same morbid poison. I think it may be maintained that diphtheria and the other varieties of sore throat I have mentioned are similarly related. As is any case of diarrhoea in an epidemic season, it is impossible to predicate that it will not pass into cholera if neglected, so in the cases of ordinary sore throat which have lately presented themselves, no one would be bold enough to assert, that any one might not before long exhibit the characteristic symptoms of true diphtheria. A fact upon which I would especially rely in the support of this view, is the occurrence of ordinary sore throats equally with diphtheritic sore throats among the number of families where deaths from diphtheria had taken place. Out of 47 families in which a death from diphtheria had occurred, (Class 1.)

Some other members of the family suffered from diphtheria in		9	Families
other forms of sore throat in		15	"
Some members suffered from diphtheria and other forms of sore throat in		8	"
Neither diphtheria nor sore throat occurred in other members of the family		15	"
Total		47	

The severity of these cases of sore throat varied considerably. In some it was a slight attack, congestive in character and lasting but a few days; in others there was so much febrile disturbance as to demand medical assistance; in others again there was ulceration, supuration of the tonsils or mortification. In some families the earlier cases were sore throat, in others the earlier cases were diphtheritic.

29. *Is Diphtheria Contagious?*—This again is a practical question to which you may fairly look to me for a reply. The following considerations may prove of value in assisting to determine this important point. Their tendency is towards the affirmative.

- 1.^o There is a *prima facie* case made out in favour of the affirmative, by the facts mentioned in the former section. Infectious diseases habitually spread in families they invade. Out of 47 families in which fatal diphtheria occurred, there were only 15 in which the other members all remained healthy. Of course it may be argued in opposition that all the members of a family are equally exposed to the operation of local causes of disease.
- 2.^o As a rule, it spread in the houses it invaded, chiefly among those members of the several families who were most closely in communication.
- 3.^o In no case, where separation from the sick person has been effected early in the disease, have I noticed that it has spread to the separated individuals. In one case where communication had been allowed for 3 days before separation, a child was seized with diphtheria on the 6th day from removal from home.
- 4.^o The following special cases may be mentioned of communication from one house to another, viz. :—

On May 1st and 2nd, Jane —, aged 10 years, was on a visit to a cousin, who on the 2nd manifested decided symptoms of diphtheria, but recovered; another person in the house, a servant, subsequently dying of the disease. On the 3rd having returned home to Islington, Jane was attacked with diphtheria, and died in 7 days. Her mother, and her sister aged 14 years, were both taken ill on the 11th. In each of these instances there was sloughing of the tissues, and the mother expectorated a complete cast of windpipe. Both died. Other members of the family had slight sore throat.

On September 18th, George —, aged 2 years, was attacked with what was believed to be diphtheria. There was much swelling behind and below the angles of the jaw, with loss of voice and great fetor of the breath. He died on the 24th. His brother Edward —, aged 9 months, was attacked on the 22nd, *i. e.*, on the 4th day of George's illness. The exudation was present, there was much swelling of the throat with fetor of the breath, and bloody discharge from the nose. He died on the 24th also. Beside other members of the family, and other persons in the house, who had been in communication with these children and consequently suffered from sore throat, the child's grand-mother had sore throat on the 24th, 25th, and 26th. She laid out the children, and on the 24th returned to her own home, at another part of the parish. A child who lived with her, aged 16 months, cousin of those who died, was seized with diphtheritic sore throat and fever on the 29th, and died on October 19th.

In the middle of June a boy brought home diphtheria from school and shortly after his recovery a sister, aged 2 years, was attacked with the disease, in a marked form, and died in 11 days. Almost every other member of the family suffered either from diphtheria or some other form of sore throat.

A little girl, aged 6 years, went to a day school where a case of diphtheria occurred; she was attacked with the disease in a characteristic form and died in 22 days. Her father and mother both suffered from a slight attack of sore throat. Another child in the family, who was kept apart from her, and subsequently sent out of the house, escaped.

In view, therefore, of those facts alone which I have ascertained in your own district, I may well repeat the admonition in No. 14 of my Monthly Reports, and impress upon all in whose families this disease may appear, the importance of at once interdicting any but the most necessary intercourse with the sick individual, and especially any communication with the younger persons in the house.

30. *Local Sources of Miasma, &c.* at the houses invaded. The following table exhibits the results of inquiries instituted at 57 houses where fatal cases occurred:—

	Number of Houses.	Houses Drains defective, or Offensive Smells on or near the Premises.	Overcrowding, or Defective Ventilation.	Foul Drinking Water and other Nuisances and Noxious Accumulations of various kinds.	Nothing Amis.
Class 1	46	19	4	6	20
Class 2	6	3	—	—	3
Class 3	5	2	—	2	2
Total	57	24	4	8	25

In more than half the houses examined there was some defect or other in the sanitary arrangements or surrounding conditions of the patient. In the greater number of houses thus deficient the fault was discovered in the state of the drainage.

31. The full discussion which I have given to these 80 cases enables me to dismiss in a very few words the subject of epidemic sore throat, so far as it relates to the year 1858. The total number of primary cases was 59, of which 37 occurred in the East Sub-district. Their distribution over the various periods of the year may be seen in Table II. The frequency of the deaths from this disease, relatively to those from other diseases in the several Wards of the Parish may be gathered from Table III.

I have the honour to be, Gentlemen,

Your very obedient Servant,

EDWARD BALLARD, M.D., L.R.C.P.

Medical Officer of Health.

42, Myddelton Square,
April 4th, 1859.

TABLE I.
THS WHICH HAVE OCCURRED IN ISLINGTON AND ITS SUB-DISTRICTS DURING THE YEAR 1881
Distinguishing the Ages and Causes of Deaths—(Deaths of Strangers in Hospitals excluded.)

West. S.E.	East. S.E.	Cause of Death.	All Ages.	AGES.												
				Under 1	1-2	2-3	3-4	4-5	5-10	10-15	15-20	20-25	25-30	30-35	35-40	40-45
1	2	V. Pericarditis.....	3	..	..	..	..	..	..	..	..	..	..	..	..	..
1	2	Aneurism.....	3	..	..	..	..	..	..	..	..	..	..	..	..	..
50	55	Disease of Heart, &c.	103	..	..	..	..	..	..	..	..	..	..	..	..	..
18	4	VI. Laryngitis.....	14	6	6	..	..	..	..	..	..	..	..	..	..	..
17	109	Bronchitis.....	126	88	35	16	5	5	149	8	1	5	6	19	19	59
3	..	Pleurisy.....	3	..	..	..	..	..	..	..	..	..	..	..	..	..
39	75	Paramonia.....	113	51	29	9	8	2	..	..	..	..	..	..	..	..
10	19	Asthma.....	29	..	..	..	..	..	..	..	..	..	..	..	..	..
5	4	Disease of Lungs.....	9	..	..	..	..	..	..	..	..	..	..	..	..	..
18	12	VII. Teething.....	30	18	9	3	..	..	..	..	..	..	..	..	..	..
4	4	Quinsy.....	8	..	..	..	..	..	..	..	..	..	..	..	..	..
3	1	Gastritis.....	4	..	..	..	..	..	..	..	..	..	..	..	..	..
7	9	Esteritis.....	16	5	..	1	1	1	8	1	1	1	2	1	2	..
6	5	Peritonitis.....	11	1	..	..	..	..	..	..	..	..	..	..	..	..
..	3	Amnesia.....	3	..	..	..	..	..	..	..	..	..	..	..	..	..
1	1	Hernia (Rupture).....	2	..	..	..	..	..	..	..	..	..	..	..	..	..
3	4	Hemorrhage.....	7	..	..	..	..	..	..	..	..	..	..	..	..	..
1	1	Intussusception.....	2	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Stricture of Intestinal.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..
6	3	Cancer.....	9	..	..	..	..	..	..	..	..	..	..	..	..	..
1	..	Disease of Stomach, &c.	1	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Inflammation of Liver.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..
2	3	(Hepatitis).....	5	..	..	..	..	..	..	..	..	..	..	..	..	..
18	13	Jaundice.....	31	5	..	..	..	..	..	..	..	..	..	..	..	..
..	1	Disease of Liver.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Disease of Spleen.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..
2	1	VIII. Nephritis.....	3	..	..	..	..	..	..	..	..	..	..	..	..	..
4	4	Nephro (Bright's) Disease.....	8	..	..	..	..	..	..	..	..	..	..	..	..	..
1	..	Ischuria.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Diabetes.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..
3	..	Stone.....	3	..	..	..	..	..	..	..	..	..	..	..	..	..
..	1	Cystitis.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..
5	9	Stricture of Urethra.....	13	..	..	..	..	..	..	..	..	..	..	..	..	..
1	..	Disease of Bladder.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..
1	..	IX. Parametritis.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Ovarian Dropsy.....	..	..	..	..	..	..	..	..	..	..	..	..	..	..
3	1	Childbirth (Puerperal).....	4	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Dia. of Uterus, &c.	3	..	..	..	..	..	..	..	..	..	..	..	..	..
3	..	X. Rheumatism.....	3	..	..	..	..	..	..	..	..	..	..	..	..	..
4	4	Arthritis.....	8	1	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Dia. of Joints, &c.	1	..	..	..	..	..	..	..	..	..	..	..	..	..
3	..	XI. Carbuncle.....	3	..	..	..	..	..	..	..	..	..	..	..	..	..
1	..	Phlegmon.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..
1	1	Disease of Skin, &c.	2	..	..	..	..	..	..	..	..	..	..	..	..	..
3	3	XII. Cyanosis.....	6	..	..	..	..	..	..	..	..	..	..	..	..	..
1	3	Spina Bifida.....	4	..	..	..	..	..	..	..	..	..	..	..	..	..
3	3	Other Malformations.....	6	..	..	..	..	..	..	..	..	..	..	..	..	..
2	..	XIII. Intemperance.....	2	..	..	..	..	..	..	..	..	..	..	..	..	..
..	1	Privation of Food.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..
11	2	Want of Breast Milk.....	13	1	..	..	..	..	..	..	..	..	..	..	..	..
1	..	Cold.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Fatigue.....	..	..	..	..	..	..	..	..	..	..	..	..	..	..
2	3	Burns and Scalds.....	5	1	1	3	..	..	..	..	..	..	..	..	..	..
..	..	Hanging.....	2	..	..	..	..	..	..	..	..	..	..	..	..	..
6	5	Suffocation.....	11	9	..	..	..	..	..	..	..	..	..	..	..	..
1	6	Drowning.....	7	..	..	..	..	..	..	..	..	..	..	..	..	..
7	4	Fracture of Cranium.....	11	..	..	..	..	..	..	..	..	..	..	..	..	..
2	2	Wounds.....	4	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Other Violence.....	10	8	..	..	..	..	..	..	..	..	..	..	..	..

* Age of one, unspecified.

TABLE I.
DISTINGUISHING THE AGES AND CAUSES OF DEATH (Deaths of Strangers in Hospitals excluded.)

West. S.E.	East. S.E.	Cause of Death.	All Ages.	AGES.																				
				Under 1	1-2	2-3	3-4	4-5	5-10	10-15	15-20	20-25	25-30	30-35	35-40	40-45	45-50	50-55	55-60	60-65	65-70	70-75	75-80	80-85
2	1	V. Pericarditis.....	3	..	..	..	..	..	..	..	1	1	..	1	..	..	..	..	..	..	..	..	..	..
1	2	Aneurism.....	3	..	..	..	..	..	..	..	..	..	..	..	1	1	..	..	..	..	..	..	..	..
50	55	Disease of Heart, &c.	103	..	..	..	..	..	..	..	..	..	..	..	3	5	20	1	19	36	8	1	..	..
19	4	VI. Laryngitis.....	14	6	6	..	..	..	19	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..
127	195	Bronchitis.....	313	88	35	16	5	5	149	8	1	5	6	10	19	19	36	59	24	6	..	..	..	
3	..	Pleurisy.....	3	..	..	..	..	..	..	..	..	..	..	..	1	..	2	..	..	..	..	..	..	
39	75	Paramonia.....	113	51	29	9	8	2	..	..	1	7	3	3	2	2	..	..	..	..	..	..	..	
10	19	Asthma.....	29	..	..	..	..	..	..	..	..	..	..	..	..	1	3	6	7	3	..	..	..	
5	4	Disease of Lungs.....	9	..	..	..	..	..	..	..	..	..	..	..	1	2	2	1	3	..	..	..	..	
18	12	VII. Teething.....	30	18	9	3	..	..	39	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
4	4	Quinsy.....	8	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
3	1	Gastritis.....	4	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
7	9	Esteritis.....	16	5	..	1	1	1	8	1	1	1	2	1	2	..	2	..	..	..	..	..	..	
6	5	Peritonitis.....	11	1	..	..	..	..	..	..	..	..	..	..	2	2	3	..	..	..	..	..	..	
..	3	Amnesia.....	3	..	..	..	..	..	..	..	..	..	..	..	..	..	1	1	..	..	..	..	..	
1	1	Ulceration of Intest.	4	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	1	..	..	..	..	
3	4	Hernia (Rupture).....	7	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
1	1	Hemorrhage.....	2	..	..	..	..	..	..	..	..	..	..	..	1	1	..	..	..	..	..	..	..	
..	..	Intussusception.....	2	1	..	1	..	..	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
..	..	Stricture of Intestinal.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
6	3	Cancer.....	9	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	..	
1	..	Disease of Stomach, &c.	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	
..	..	Inflammation of Liver.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	
2	3	(Hepatitis).....	5	..	..	..	..	..	..	..	..	..	..	..	1	..	1	2	1	..	..	..	..	
18	13	Jaundice.....	31	5	..	..	..	..	5	..	..	..	1	..	..	..	1	..	..	..	1	..	..	
..	1	Disease of Liver.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
..	..	Disease of Spleen.....	1	..	..	..	..	..	..	..	..	..	1	..	..	..	..	..	..	..	..	..	..	
2	1	VIII. Nephritis.....	3	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	2	..	..	
4	4	Nephro (Bright's) Disease.....	8	..	..	..	..	..	..	..	..	..	..	..	1	..	1	2	..	..	..	..	..	
1	..	Ischuria.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
..	..	Diabetes.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	..	..	
3	..	Stone.....	3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
..	1	Cystitis.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
5	9	Stricture of Urethra.....	13	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	..	..	..	
1	..	Disease of Bladder &c.	1	..	..	..	..	..	..	..	..	..	..	..	..	..	1	2	..	..	..	..	..	
1	..	IX. Parametritis.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
..	..	Ovarian Dropsy.....	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
3	1	Childbirth (Puerperal).....	4	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
..	..	Dia. of Uterus, &c.	3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2	1	..	..	..	
3	..	X. Rheumatism.....	3	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	..	..	..	
4	4	Arthritis.....	8	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
..	..	Dia. of Joints, &c.	1	..	..	..	..	..	..	..	1	2	1	4	..	..	..	..	..	..	..	..	..	
3	..	XI. Carbuncle.....	3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
1	..	Phlegmon.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
1	1	Disease of Skin, &c.	2	..	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	
3	2	XII. Cynoma.....	5	5	..	..	..	..	5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
3	3	Scars, Etc., &c.	5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
..	..	Other Malformations.....	5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
2	..	XIII. Intemperance.....	2	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	1	..	
11	2	Intoxication of Food.....	13	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
11	2	Want of Breast Milk.....	13	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
..	..	Neglect.....	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
1	..	Cold.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
1	..	Poison.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
..	..	Burns and Scalds.....	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
3	3	Hanging.....	3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
1	6	Suffocation.....	11	9	..	1	1	11	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
7	2	Fractures & Contus.	9	..	..	..	..	..	..	..	..	..	..	..	1	1	1	1	..	..	2	..	..	
..	..	Wounds.....	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	
8	2	Other Violence.....	10	8	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	

DEATHS OCCURRING IN EACH WEEK AND QUARTER, "DISTINGUISHING CERTAIN IMPORTANT DISEASES AND AGES."
[Deaths of Non-Residents in Hospitals, and of Hospital Patients, not included.]

WEEK ENDING	Jan. Jan.	Feb. Feb.	Mar. Mar.	Apr. Apr.	May May	Jun. Jun.	Jul. Jul.	Aug. Aug.	Sep. Sep.	Oct. Oct.	Nov. Nov.	Dec. Dec.	1st Quarter	2nd Quarter	3rd Quarter	4th Quarter
Total	58	53	53	54	47	42	41	39	44	57	729	53	47	48	43	57
Under 5 years	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Zymotic diseases	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Measles	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Scarlatina	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Epidemic Sore Throat	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Diphtheria	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Whooping Cough	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Diarrhoea	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Cholera	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Continued Fever	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Phthisis	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Consumption	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Organ	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Mean Weekly Temperature	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5

WEEK ENDING	Jan. Jan.	Feb. Feb.	Mar. Mar.	Apr. Apr.	May May	Jun. Jun.	Jul. Jul.	Aug. Aug.	Sep. Sep.	Oct. Oct.	Nov. Nov.	Dec. Dec.	1st Quarter	2nd Quarter	3rd Quarter	4th Quarter
Total	58	53	53	54	47	42	41	39	44	57	729	53	47	48	43	57
Under 5 years	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Zymotic diseases	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Measles	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Scarlatina	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Epidemic Sore Throat	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Diphtheria	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Whooping Cough	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Diarrhoea	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Cholera	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Continued Fever	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Phthisis	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Consumption	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Organ	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
Mean Weekly Temperature	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5	53.5

TABLE III.

Showing the Number of Deaths registered in each Ward, distinguishing those from certain important Diseases and Classes of Disease, and the proportion borne by those Deaths to the total mortality of the Ward.
[Omitting Deaths in the Workhouse and Infant Poor-house, and such Deaths as could not be correctly referred to the Ward in which they occurred.]

	Ward 1	Ward 2	Ward 3	Ward 4	Ward 5	Ward 6	Ward 7, Ward 8
	Per cent. of all deaths.	Per cent. of all deaths.	Per cent. of all deaths.	Per cent. of all deaths.	Per cent. of all deaths.	Per cent. of all deaths.	Per cent. of all deaths.
All Causes.	153	211	284	558	205	228	276
Zymotic diseases . .	32	20.9	69	27.1	91	32.5	131
Tubercular diseases .	24	15.0	38	18.8	35	16.6	44
Deaths under 5 yrs.	52	33.5	105	47.5	139	67.3	120
Phthisis	20	13.0	29	13.0	29	13.0	32
Dts. of Org. of Res.	20	13.0	29	13.0	29	13.0	32
Small Pox	3	1.9	1	0.6	1	0.7	1
Measles	1	0.6	1	0.6	1	0.6	1
Scarlatina	5	3.2	5	2.3	5	2.3	5
Epi. Sore Throat . .	2	1.3	4	1.8	9	3.7	11
Whooping Cough . .	11	7.1	21	9.5	20	9.3	11
Diarrhoea & Cholera .	3	1.9	7	3.1	14	6.3	9
Fever	1	0.6	8	3.6	12	5.8	4

TABLE IV.

New Cases of Disease coming under Treatment during the Year ending Saturday the 1st day of January, 1889, at the following Institutions:—

	Workhouse.	Infant Poor House.	Out Patient Dispensary.	Holloway Dispensary.	Pertham Hall Dispensary.	Task.	Locksley Hospital.	Small Pox Hospital.
Small Pox	..	..	37	10	..	42	2	228*
Chicken Pox	..	..	45	19	..	58	3	1
Measles	..	..	340	88	..	422	1	6
Scarlatina	..	..	14	10	..	24	1	..
Whooping Cough	..	..	240	150	..	390	4	..
Cramp	..	..	8	1	..	9	..	..
Diarrhoea	43	1	604	321	..	1031	..	..
Dysentery	..	..	18	10	..	28	..	..
Cholera	..	..	3	..	..	3	..	..
Ague	..	..	24	23	..	47	10	..
Continued Fever	..	..	67	103	..	170	..	..
Purpura	..	..	1	3	..	4	..	..
Rheumatic Fever	..	..	43	24	..	67	..	..
Erysipelas	..	..	92	24	..	116	..	..
Diphtheria	..	..	6	3	..	9	..	..
Pneumonia	144	1	2140	782	..	3410	25	..
Phthisis	..	..	40	45	..	85	..	..
Fluorid	..	..	8	1	..	9	..	..
Idiomatic Tremor	..	..	..	..	..	14	..	..
Insanity	10	..	37	1	..	55	..	..
Carbuncle	..	..	15	2	..	17	..	..
All other Diseases	272	165	8725	2670	..	10,372	112	9
Total	805	121	9564	3325	911	15,829	321	358

* Thirty-two of these from Islington.

TABLE V.

COMPARATIVE TABLE OF SICKNESS AMONG PAUPER PATIENTS DURING
1857 & 1858.

	Total.		Per cent. of all Cases.	
	1857.	1858.	1857.	1858.
New Cases	9432	10591		
Small Pox.....	23	37	0.24	0.34
Chicken Pox.....	35	45	0.37	0.42
Measles.....	138	348	1.46	3.28
Scarlatina.....	61	154	0.64	1.45
Whooping Cough	189	247	2.0	2.33
Croup	5	8	..	..
Thrush	10	39	..	..
Diarrhoea	1078	648	11.4	6.1
Dysentery.....	6	18	..	..
Cholera.....	2	3	..	..
Purpura, Scurvy	2	3	..	..
Ague.....	9	26	..	..
Remittent Fever	2	0	..	..
Infantile Fever.....	27	18	..	..
Continued Fever	118	69	1.25	0.65
Puerperal Fever	0	1	..	..
Rheumatic Fever.....	99	55	1.49	0.51
Erysipelas.....	54	88	0.57	0.83
Syphilis	70	55	0.74	0.51
Noma (Canker)	5	4	..	..
Total of Zymotic Diseases	1933	1866	20.49	17.6
Diseases of Respiratory Organs	1997	2353	21.17	22.2
Phthisis	103	120	1.09	1.13
Carbuncles and Boils	44	67	0.46	0.63
Chronic and Local Rheumatism	299	375	3.17	3.54
Other Diseases.....	4668	5368	49.49	50.0
Accidents and Injuries.....	388	442	4.11	4.17

TABLE VI.

LIST OF STREETS, ETC. in which the Zymotic Mortality has exceeded one-fourth of the total mortality of the Street; while the mortality from Tubercular Diseases has exceeded one-tenth, or the Infant mortality (under 5 years of age) one-half. Streets where only one death has occurred omitted.

Wherever the Population is stated, it is that ascertained at the time the Street was inspected.

The * against any Street indicates that its Death-rate exceeded 20 per 1000 of those living.

	Sum. Popu- lation, 1857-8.	Total Mortality, 1857-8.	Zymotic Mortality, 1857-8.	Tubercular Diseases, 1857-8.	Infant Mortality, under 5 yrs.		Sum. Popu- lation, 1857-8.	Total Mortality, 1857-8.	Zymotic Mortality, 1857-8.	Tubercular Diseases, 1857-8.	Infant Mortality, under 5 yrs.
Ward 1.											
Archway Place	6	1	1	0	0	Salisbury Terrace	2	1	1	0	0
Bowman's Place North	2	1	1	0	0	Stanley Road	2	1	1	0	0
Bramwell Road	2	1	1	0	0	*Victoria Place, Horseay Road ..	67	21	3	1	0
Crown Terrace	2	1	1	0	0						
John Street	117	21	3	1	1	Ward 4.					
Junction Villas	2	1	1	0	0	Allison Street	271	10	3	1	0
John Street	2	1	1	0	0	*Bath Place	261	10	3	1	0
Midford Road	2	1	1	0	0	*Bemerton Street	261	10	3	1	0
Regina Road	2	1	1	0	0	*Bemerton Terrace	159	10	3	1	0
Upper Horseay Road	2	1	1	0	0	Blackfield Street	159	10	3	1	0
						Bryon Place	2	1	1	0	0
Ward 2.											
Arthur Terrace	3	1	1	0	0	Rockingham Street	2	1	1	0	0
Brundell Street	2	1	1	0	0	Caledonia Crescent	2	1	1	0	0
Devonshire Street	2	1	1	0	0	Caledonia Place	2	1	1	0	0
Edward Terrace	2	1	1	0	0	Caledonia Street	2	1	1	0	0
Frederick Place	2	1	1	0	0	*Casal Terrace	159	10	3	1	0
Frederick Street	2	1	1	0	0	Clayton Street	2	1	1	0	0
George Street	7	3	1	1	1	Doddington Place	2	1	1	0	0
George's Grove	4	2	1	1	1	*Edward Square	431	17	6	1	0
Holmesworth Street	101	12	3	1	1	*Edward Street	186	10	3	1	0
Little Hyde Street	165	12	3	1	1	Edward Terrace	424	17	6	1	0
Loraine Place	2	1	1	0	0	*Freeling Street	173	12	4	1	0
Palmer Street	2	1	1	0	0	Gordon Terrace	2	1	1	0	0
Pocock's Fields, (Cottages)	2	1	1	0	0	Hastings Street	2	1	1	0	0
Stocks-Outlet Ground	2	1	1	0	0	St. James's Vale, & Sutton's Gdn ..	123	10	3	1	0
Sutton Street	2	1	1	0	0	*Lavinia Grove	2	1	1	0	0
*Upper Victoria Road	222	10	3	1	1	London Street	2	1	1	0	0
St. James's Road	2	1	1	0	0	Loam Street	2	1	1	0	0
Wellington Terrace	2	1	1	0	0	Lyon Street	2	1	1	0	0
Whim Street	2	1	1	0	0	Marilla Street	2	1	1	0	0
						Mexican Terrace	2	1	1	0	0
Ward 3.											
*Albany Place, Horseay Road	208	10	3	1	1	Penelope Cottages	2	1	1	0	0
*Arundel Road	83	10	3	1	1	Penelope Street	2	1	1	0	0
*Aston Buildings	63	10	3	1	1	Princes Terrace	2	1	1	0	0
Blackstock Lane	2	1	1	0	0	Randall's Tile Kiln	2	1	1	0	0
*Brand Street	2	1	1	0	0	Randall's Cottages	2	1	1	0	0
*Brunswick Grove	65	10	3	1	1	Rivers Terrace	2	1	1	0	0
Canterbury Road	2	1	1	0	0	St. James's Terrace	2	1	1	0	0
Canterbury Terrace	2	1	1	0	0	Silbury Terrace	159	10	3	1	0
College Terrace, Highbury Vale ..	2	1	1	0	0	Stammore Street	2	1	1	0	0
*Cranby Terrace	2	1	1	0	0	Sutherland Terrace	2	1	1	0	0
*Hawthorn Street	159	10	3	1	1	Thornhill Bridge Place	2	1	1	0	0
Heron Place	2	1	1	0	0	Thornhill Street	2	1	1	0	0
Kelso Terrace	2	1	1	0	0	Twyford Street	2	1	1	0	0
*King's Road, Bulls Pond	4	1	1	0	0	Upper Binglefield Street	2	1	1	0	0
*Lindfield Terrace	99	10	3	1	1	*Upper Beaumont Street	281	10	3	1	0
Myrtle Street	242	10	3	1	1	Upper Freeling Street	2	1	1	0	0
Newington Crescent	2	1	1	0	0	Upper Winchester Street	2	1	1	0	0
Northampton Park	2	1	1	0	0	William Street North	2	1	1	0	0
Prince Edward Street	128	10	3	1	1						
Railway Terrace	2	1	1	0	0	Ward 5.					
Railway Terrace	128	10	3	1	1	Albert Street	2	1	1	0	0
						Alison Grove, West	2	1	1	0	0
						Barnaby Grove	2	1	1	0	0

TABLE VI. (Continued)

	Sum. Popu- lation, 1867-74.	Total Mortality.	Spontaneous Mortality.	Tabular Mortality.	Mortality under 5 yrs.		Sum. Popu- lation, 1867-74.	Total Mortality.	Spontaneous Mortality.	Tabular Mortality.	Mortality under 5 yrs.
Ward 5, continued.											
Coakley Street	..	4	..	3	1	Rutherford Street	..	..	2	..	2
Lawrence Place	..	..	..	..	..	St. Mary's Road	..	..	..	..	..
Marriott Terrace	..	..	..	..	..	Spencer Street	..	..	..	..	..
St. James Terrace	..	..	..	..	..	Spencer Terrace	..	..	..	..	..
Union Street	..	..	..	..	..	Strahan Place	..	..	..	..	..
Union Street	..	..	..	..	..	Sunset Terrace	..	..	..	..	..
Waverley Place	165	19	4	..	..	Victoria Road	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Willon Square	..	..	..	..	..
Waverley Terrace	..	..	..	..	..						
Waverley Terrace	..	..	..	..	..	Ward 6.					
Waverley Terrace	..	..	..	..	..	Adelaide Square	..	..	9	..	1
Waverley Terrace	..	..	..	..	..	Alfred Mews	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Alfred Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Angel Terrace	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Angels' Gardens	255	19	..	..	..
Waverley Terrace	..	..	..	..	..	Arington Square	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Baldwin Terrace	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Berby's Buildings	78	19	4	..	..
Waverley Terrace	..	..	..	..	..	Britannia Row	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Carlton Crescent	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Carlton Place	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	City Gardens	61	25	24	..	..
Waverley Terrace	..	..	..	..	..	Elmer Walk and Place	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Dally Terrace	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Dane Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Dunoon Terrace	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Elliot's Place and Gardens	165	23	6	..	..
Waverley Terrace	..	..	..	..	..	Frederick's Place	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Progn Lane	132	13	3	..	..
Waverley Terrace	..	..	..	..	..	Griffin Street	226	13	..	..	..
Waverley Terrace	..	..	..	..	..	Grove Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Hall Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Hall Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	James Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	King Street, South side	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	King Street Terrace, South side	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Nelson Place	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Nelson Place	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Newall Street	201	28	10	4	..
Waverley Terrace	..	..	..	..	..	Norfolk Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Norfolk Street	215	14	..	..	..
Waverley Terrace	..	..	..	..	..	Paradise Place	232	17	4	..	..
Waverley Terrace	..	..	..	..	..	Paradise Place	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Pierpoint Row	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Poplar Street	330	19	13	..	..
Waverley Terrace	..	..	..	..	..	Poplar Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Rhodes Terrace	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Rhodes Terrace	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Russell Place	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Russell Place	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Rydon Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	St. John Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	St. John Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	St. Peter's Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	St. Peter's Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	St. Thomas Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Sidney Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Sidney Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Sidney Grove	224	19	4	..	..
Waverley Terrace	..	..	..	..	..	South Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	South Street	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Swan Yard	66	47	6	..	..
Waverley Terrace	..	..	..	..	..	Swan Yard	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Victoria Place	161	15	9	..	..
Waverley Terrace	..	..	..	..	..	Victoria Place	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Ward's Terrace	233	33	6	..	..
Waverley Terrace	..	..	..	..	..	Ward's Terrace	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Ward's Terrace	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Ward's Terrace	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Ward's Terrace	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Ward's Terrace	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Ward's Terrace	..	..	..	..	..
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Waverley Terrace	..	..	..	..	..	Ward's Terrace	..	..	..	..	..
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Waverley Terrace	..	..	..	..	..	Ward's Terrace	..	..	..	..	..
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Waverley Terrace	..	..	..	..	..	Ward's Terrace	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Ward's Terrace	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Ward's Terrace	..	..	..	..	..
Waverley Terrace	..	..	..	..	..	Ward's Terrace	..	..	..	..	..
Waverley Terrace											

TABLE VII

HOUSES INSPECTED AND SANITARY IMPROVEMENTS EFFECTED.

From January 1st, 1858, to March 25th, 1859.

	West Sub-District.	East Sub-District.	Whole Parish.
Number of Houses Inspected	1168	1036	2204
DRAINS { Constructed	97	256	353
Improved or repaired	152	327	479
Traps fixed	827	805	1632
CRESSPOOLS { Abolished	330	316	646
Cleaned or disinfected	2	..	2
PRIVES AND WATER CLOSETS.. { Pan, trap, and water supply furnished..	220	271	491
Pan and trap only furnished.....	129	95	224
Water supply furnished.....	108	244	352
DUST-BINS { Constructed	250	514	764
Repaired and covers adapted.....	340	217	557
SURFACE DRAINS AND PAVEMENTS OF YARDS { Constructed	98	149	247
Relaid.....	563	167	730
GENERAL WATER SUPPLY { New receptacle provided	171	186	357
Receptacle repaired and cleansed	72	83	155
Water supply provided	26	22	48
OTHER IMPROVEMENTS { Houses generally repaired	100	98	198
„ cleansed or limewashed	123	297	420
„ ventilated	28	93	121

REPORT ON THE SANITARY CONDITION

Of the Parish of St. Mary, Islington,

DURING THE YEAR

1859.

BY

EDWARD BALLARD, M.D., (Lond.)

MEMBER OF THE ROYAL COLLEGE OF PHYSICIANS;

FELLOW OF THE ROYAL MEDICAL AND CHIRURGICAL SOCIETY OF LONDON, ETC.

MEDICAL OFFICER OF HEALTH.

LONDON:

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1860.

REPORT
ON THE SANITARY CONDITION

OF THE PARISH OF ST. MARY, ISLINGTON.

PRESENTED TO THE VESTRY.

1859.

EDWARD BURNARD, M.D., F.R.S.

Medical Officer of Health.

PRINTED BY J. H. COOPER, 15, N. B. ROAD, ISLINGTON.

TO THE VESTRY OF ST. MARY, ISLINGTON.

GENTLEMEN,

1. In estimating with accuracy the Sanitary condition of a district, and especially in attempting to compare it with that of any other district, it is not enough to enumerate the cases of sickness, or the deaths which take place in the course of nature, or as the result of disease. It is true that such an enumeration is one element, and an essential element in the problem, but there is a variety of other considerations which must be taken into account. Among the most important of these are the number of persons inhabiting the district during the period embraced in the inquiry, the relative proportion of the sexes, of the young, middle-aged, and old, the occupations in which they are engaged, their wealth or poverty, etc. And if it is necessary to take note of all these points in estimations relating to a large district such as an entire parish, the necessity for it becomes still more urgent where smaller portions are the subject of investigation, where for instance the sanitary state of one street or group of streets has to be measured against another street or group of streets. And yet, after all, this is the branch of inquiry which most closely concerns a Local Board deputed to watch for and remove local sources of ill health. It is moreover that which is most fruitful of practical suggestions to a Medical Officer of Health, who has not only to point out where disease (fatal or otherwise) prevails, to lay his finger on the very streets, courts, or houses, but also to say why it presents itself where it does, to distinguish among its several causes those which the legislature has empowered the local authority to remedy, and those which can only be removed or guarded against by measures adapted to call into exercise the intelligence of the people and to rouse them to individual effort for the private maintenance of health.

It is now nine years since a census of the population was taken, and while I found it a most difficult task at the commencement of my period of office to form an estimate of our numbers which might command general concurrence, I cannot now venture upon the effort at all. No doubt the enumeration which will take place next year will be all that I can desire, and it is to be hoped that such arrangements will be made as will place in my hands all that minute information, as to individual localities, which it is so important that I should possess, and such as will enable me to arrive at juster and more accurate conclusions, as to our sanitary position, than any I have hitherto been capable of attaining. I shall therefore content myself this year with

submitting to your attention and thought the customary tables of mortality and sickness, associated with a few simple comments which naturally result from their consideration.

MORTALITY.

2. Two thousand six hundred and seventy persons, of whom 1310 were males and 1360 females, died in our parish between January 1st, and December 31st, 1859. As usual, I exclude from this enumeration all strangers dying in hospitals, as well as the children of the Clerkenwell Infant Establishment which has been located in Islington. To this number, however, must be added 141, our estimated proportion of deaths in hospitals, lying-in institutions and lunatic asylums in London, making a total of 2811. The mortality (estimated in the same manner) of 1857 exceeded that of 1856 by 120; and the mortality of 1858 exceeded that of 1857 by 235: the mortality of last year, however, only exceeded that of 1858 by 20. As it can scarcely be imagined that the population has only increased in a similar ratio, we must, I think, consider that the death-rate of 1859 is to be viewed in a favourable light.

3. The Births registered (and a varying number always escape registration) amounted to 4997, viz.:—2595 males and 2402 females, as represented in the following table:—

	West Sub-District.			East Sub-District.			Whole Parish.		
	Males.	Females.	Total.	Males.	Females.	Total.	Males.	Females.	Total.
1st Quarter....	352	329	681	347	296	643	699	625	1324
2nd Quarter....	350	292	622	298	256	554	628	548	1176
3rd Quarter....	333	314	647	306	277	583	639	591	1230
4th Quarter....	324	338	662	305	300	605	629	638	1267
Total.....	1339	1273	2612	1256	1129	2385	2595	2402	4997

4. The corrected proportion of Deaths for each Sub-district will be for the West 1465 and for the East 1346. In the West Sub-district then the deaths were 64 in excess of 1858; and in the East they were absolutely 44 less. The mortality of the East Sub-district in 1858 was however very high compared with that of the other half of the parish. The following table shows the corrected mortality of the four last years, with the differences in excess or the contrary.

Years.	West Sub-District.			East Sub-District.			Whole Parish.		
	Deaths.	Difference from previous year.		Deaths.	Difference from previous years.		Deaths.	Difference from previous years.	
1856	1275			1161			2436		
1857	1371	+ 30		1185	+ 24		2556	+ 120	
1858	1401	+ 30		1390	+ 215		2791	+ 235	
1859	1465	+ 64		1346	- 44		2811	+ 20	

5. The deaths that occurred each week and quarter appear on Table II. In all but the summer quarter the mortality was less than in the corresponding quarter of last year. The high mortality of the summer was mainly attributable to the influence of the excessively high temperature of July. (See Monthly Report No. XXVIII.)

6. Of the 2670 persons, of whose death I possess the record, 1215 or 45·7 per cent. were under five years of age, a proportion less than in any previous year. The infant deaths in the three previous years respectively amounted to 1066, 1108 and 1238. It follows that in 1859 these deaths were less than in 1858 by 23. The deaths under one year were in 1856, 612; in 1857, 642; in 1858, 648; and in 1859, 665.

CAUSES OF DEATH.

a. Zymotic Diseases.

7. Of the 2670 deaths recorded in Table I, 746, viz. 404 in the West Sub-district and 342 in the east, arose from diseases included in the zymotic class. Of these, 519 occurred in children under 5 years of age, 200 of whom were under one year. The zymotic mortality then among children was only one more than last year, and for all ages corresponded to 27·9 per cent. of all the deaths in the Parish, or in the West sub-district to 28·9 per cent. and in the East to 26·8 per cent. The greatest zymotic mortality occurred in the summer quarter, and amounted to a total of 253, the highest number of either of the three preceding years being 196. The mean temperature of the quarter was 8·3 degrees above the average, and that of the second week of July was 9·3 above the mean of 45 years. The highest weekly zymotic mortality, viz. 32, (mainly the result of diarrhoea) occurred in the third week of July. In the fourth quarter again the zymotic mortality was very high, viz. 202; but this was the effect of an epidemic of scarlet fever.

I will now refer to the progress of the most important of these diseases during the year, taking them in the order of greatest mortality.

8. *Scarlet Fever.* It will be recollected that in 1858 this disease began to present an epidemic character towards the beginning of the summer, and after destroying 103 persons, suddenly subsided in the month of December. In the course of 1859, however, it destroyed in Islington 216 persons (128 in the West and 88 in the East sub-district) of whom 138 were under 5 years of age. The increase of the disease was gradual through the year, during the first thirteen weeks it carried off 28 persons, during the second 33, during the third 55, and during the fourth 101, the mortality amounting in the last quarter to as many as 9, 10 and 12 weekly. It did its worst in the month of November. Of all parts of the parish none suffered from it so fearfully as Sidney Grove in St. Peter's Ward. In this place 7 deaths occurred from scarlet fever out of a population of only about 224 persons, or one out of every 32 individuals. The total mortality of this place, from all causes, for the year was 13, which is equal to one out of every 17 individuals or at the rate of 58 in the 1000 inhabitants. The cause of all this is most apparent in the construction of most of the houses, the staircases of which are so arranged as to bid defiance to any circulation of air, while the back rooms, which are used as sleeping rooms, have neither window nor fire place, and only communicate by small folding doors with the front rooms: each floor being occupied by a single family, no effectual separation of the sick from the healthy is possible, but on the admission of a case of infectious disease all are compelled to breathe the same poisonous atmosphere, which no effort on the part of the inhabitants can avail to purify or dilute.

9. *Diarrhoea* occasioned only 120 deaths, 168 under 5 years of age: this is only 8 above the un-corrected average of the last three years. The greatest weekly number of deaths, viz.—20 and 18 took place in the third and fourth weeks of July.

10. *Whooping Cough* destroyed 102 children, the East district (as was the case in each of the two preceding years,) suffering more than the West. The uncorrected mean of the three previous years was 114.

11. *Fever*. The deaths attributed to Continued and Infantile Fever together amounted to 78. In the three preceding years the mortality was 86, 95 and 70.

12. *Measles* was less fatal than in any of the three years during which I have recorded its progress. It only occasioned 63 deaths, 36 in the East and 27 in the West Sub-district. Eight of the deaths in the East Sub-district occurred in Bowman's Place North. The total mortality in this place, from all causes, was 18 out of a population which may be estimated at about 248. Measles then in the course of a few weeks of the summer quarter carried off one out of each 31 of the inhabitants, and in the whole year from one cause or another (13 from disease of the zymotic class) one out of every 14 persons died, or at the rate of 72 in the 1000! In Milton's Yard again, a place which I have repeatedly had occasion to complain of to the Sanitary Committee, there were 5 deaths from measles in a population of 161 persons, and where the mean nightly population of each room exceeds three persons. Here then Measles, at a time too be it recollected when the disease was presenting less than its usual virulence, destroyed one out of each 34 persons. In this wretched place there were 8 deaths during the year, all of children under 5 years of age, and six of them from zymotic diseases representing a mortality from all causes of about one out of every thirteen persons, or at the rate of nearly 80 per 1000! a rate which equalled the mortality of London in the 17th century. In what respect I would ask did the determining causes differ in the two instances? I may add, by way of affording the means of contrast, that the general mortality of London in 1859 was 22 per 1000.

13. *Diphtheria* (as a primary disease) is said to have occasioned 45 deaths, or if we add 6 deaths from other forms of sore throat, we have a mortality of 51 against 59 last year. The greatest number occurred in the first and third quarters, and they were pretty equally distributed between the two Sub-districts. Two deaths from this disease occurring in a house in Adelaide Square, gave occasion to a special enquiry into the sanitary condition of that square, especially in regard to the over-crowding of the houses. In the whole year I find that 12 persons died there from various diseases out of a population of 220, seven of whom were children under 5 years of age. This represents a mortality of one out of every 18 inhabitants, or at the rate of 54 per 1000.

14. *Small Pox*. Since the termination of the year to which this report has reference, small-pox has been spreading so extensively in our parish as well as in other parts of the metropolis, that it is desirable that we should clearly ascertain our position with regard to it. In 1856 the deaths from small-pox in Islington were 8, in 1857 they were 12, in 1858 they were 18, in 1859 they rose to 23, including 2 deaths from chicken-pox, tabulated with those of small-pox. These numbers then exhibit a pretty steady annual increase. During 1859 the mortality moreover steadily increased from quarter to quarter. Of the 23 true fatal cases, 1 only occurred in the first quarter, 3 took place in the second, 8 in the third, and 11 in the fourth, while 14 deaths were registered during the first two months of the present year. The same sort of progression obtained in London generally. In 1857 and 1858 the numbers registered were 154 and 247 respectively, and in the several quarters

of 1859 they were 201, 233, 287 and 435, making a total for the year of 1156 deaths. And yet of all forms in which death may approach a man, it is this which may be guarded against the most readily, the most certainly, and the most conveniently. It is not my intention to discuss the question lately mooted as to whether vaccine lymph as now procured after years of transmission through the system of innumerable human beings has or has not lost its pristine protective power. My own observation and experience have satisfied me that vaccination well performed and in a sufficient number of places, is quite as effectual in protecting against fatal small-pox now as it was when my first acquaintance with both was made, and this too is the verdict of others whose period of observation dates back much further than my own. I believe the true explanation of the present spread of small-pox to lie not in the inefficacy of vaccination as a protection, but in the negligence of parents who fail to submit their children to the process, in the slovenly manner in which the operation is sometimes performed, and in the carelessness which leads people to be satisfied with the mere puncturing of the arm without affording the medical practitioner the opportunity of pronouncing upon its success or the contrary. Partly too there can be little question that the present spread of the disease is to be attributed to the neglect of re-vaccination when the period of childhood and early youth has been passed.* From these various causes, it arises that there is a large mass of persons in the community, both of adults and children, who lie very much at the mercy of the epidemic poison whenever it wanders forth to destroy. Probably the greater number of the unvaccinated are to be met with among the poorer classes, and it is to be recollected that the conditions under which they live and herd together many families in a house are precisely such as to facilitate the communication of the disease. Is it possible to estimate the number of persons thus unprotected or imperfectly protected in Islington? Approximately, within certain limits as to age, I think it is. Shortly before writing this report, I made, at the request of the Board of Trustees, and assisted by their Medical Officers, a careful examination of the children attending the several poor and charitable schools in the parish. The following is very briefly the result. The total number on the books was 8685, of whom I succeeded in examining the arms of 6500. Of these, 655 presented no mark or scar on either arm, and may therefore be pronounced to be unvaccinated, and 183 presented one or two so faint and imperfect that the vaccination they had undergone could only be regarded as of very doubtful protective power, and in respect of whom I felt that I ought to advise re-vaccination. The whole of these 838 children were open to attack of small-pox of greater or less severity. There cannot I think be less than 48000 children in Islington under 15 years of age, that is of the ages usually met with in charitable schools; but if we make an allowance of 8000 for the children of the more wealthy whose vaccination has probably been carefully attended to, and if in respect of the remainder we assume that the proportion of unprotected is the same as in the 6500 examined, we arrive at the number of about 4090 either born in the parish or brought into it from other places who have never been successfully vaccinated at all, and of about 850 who are only vaccinated imperfectly. Probably the latter estimate is much too low, as where the children in the schools were very young I did not consider it desirable to recommend re-vaccination so long as even an indifferent vaccination scar was visible on the arm. It would be a salutary regulation in charitable schools that no child should be admitted unless its satisfactory vaccination shall have been previously certified by a competent medical gentleman—a suggestion which I submit for the consideration of the committees of these institutions.

* This remark applies principally to the spread of the disease in an epidemic season, when adult persons vaccinated in infancy and who might be sufficiently protected for ordinary non-epidemic seasons, are more endangered than persons who have recently undergone the vaccine disease. For further information on this subject, see Mr. Simon's Report on Vaccination (published by the general Board of Health) p. 29, etc.

b. Tubercular Diseases.

15. The deaths from this class of maladies were 463; from pulmonary consumption, which is the chief member of the class, they were 338. This is an increase upon former years. It remains to be seen hereafter whether it is such as it might be expected from the increase of our population at the ages which tubercular maladies principally affect.

c. Diseases of the Organs of Respiration.

16. There can be no question about the low fatality of these diseases during 1859. The deaths only amounted to 375. In former years they reached 392, 422, and 494. There were 196 in the West Sub-district and 179 in the East. Of the whole number 153 occurred in early childhood, the numbers in former years being 183, 210, and 261.

SICKNESS.

17. The tables of Sickness (Tables IV and V.) appended to this report must be regarded as supplementary only, inasmuch as while the mortality tables represent all the deaths that occur in the parish, the tables of Sickness only exhibit a portion of the illnesses that require medical aid, that portion which seeks relief in public institutions. I must add with respect to the Small-Pox Hospital, that the comparison of 1170 admissions, with the 358 of the previous year, does not of course represent the proportionately increased prevalence of the disease in London, inasmuch as the Hospital can be but full, as it has been the whole year. Forty-eight cases were received from Islington. The type of continued fever remains the same, viz. abdominal; 48 cases admitted into the London Fever Hospital being of true Typhus, the remaining 172 of Typhoid.

SANITARY IMPROVEMENTS DURING THE YEAR.

18. The Table VI. is a summary of the house inspections and improvements carried out under the orders of the Sanitary Committee between the 26th March, 1859 and the 25th March, 1860. But in addition I should mention the removal of a vast number of other nuisances, such as foul accumulations of various kinds, obstructions in panned privies and drains, most of which arise from the neglect of furnishing them with a proper water supply. In Smith's Buildings, Jennings patent water supply apparatus has been set up for domestic purposes. It remains to be seen how far this is applicable to houses of that class.

TRADE NUISANCES.

19. Among trade nuisances I should record the complete abolition of a Piggery in the rear of Francis place, Holloway, and of the trade of fat melting carried on in a house in Nowell's Buildings, both of which had been the cause of frequent and loud complaints from the neighbours. In several instances where overcrowding of dwelling houses occupied by more than one family was discovered, the fault was remedied on the receipt of a letter from the Office. The only instances where it was found necessary to take proceedings before the magistrate, were the cases in Brand Street, Holloway, Payne Street and Adelaide Square.

20. At the annual licencing in October, there were 103 applications for Slaughter houses. These places were all visited by the Inspectors, and such as it was necessary I should see, by myself. Their cleanliness and other arrangements being satisfactory, only one application was refused on the ground of non-compliance with the requirements of the act of Parliament and the demands of the Justices.

I have the honour to be, Gentlemen,

Your very obedient Servant,

EDWARD BALLARD, M.D., M.R.C.P.
Medical Officer of Health.

*Vestry Offices,
April 30th, 1860.*

DEATHS WHICH HAVE OCCURRED IN ISLINGTON AND ITS SUB-DISTRICTS DURING THE YEAR

[illegible]

DISTINGUISHING THE AGES AND CAUSES OF DEATH (Deaths of Strangers in Hospitals excluded).

[illegible]

TABLE II.

WEEK ENDING	Jan	Feb	Mar	Apr	May	June	July	Aug	Sept	Oct	Nov	Dec	Total	1st	2nd	3rd	4th	5th	6th	7th	8th	9th	10th	11th	12th	13th	14th	15th	16th	17th	18th	19th	20th	21st	22nd	23rd	24th	25th	26th	27th	28th	29th	30th	31st	32nd	33rd	34th	35th	36th	37th	38th	39th	40th	41st	42nd	43rd	44th	45th	46th	47th	48th	49th	50th	51st	52nd	53rd	54th	55th	56th	57th	58th	59th	60th	61st	62nd	63rd	64th	65th	66th	67th	68th	69th	70th	71st	72nd	73rd	74th	75th	76th	77th	78th	79th	80th	81st	82nd	83rd	84th	85th	86th	87th	88th	89th	90th	91st	92nd	93rd	94th	95th	96th	97th	98th	99th	100th
Under a year	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								
1 to 2 years	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								
2 to 3 years	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								
3 to 4 years	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								
4 to 5 years	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								
5 to 6 years	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								
6 to 7 years	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								
7 to 8 years	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								
8 to 9 years	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								
9 to 10 years	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								
10 to 11 years	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								
11 to 12 years	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								
12 to 13 years	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								
13 to 14 years	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								
14 to 15 years	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								
15 to 16 years	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								
16 to 17 years	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								
17 to 18 years	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100																								

TABLE III

Shewing the Number of Deaths registered in each Ward, distinguishing those from certain important Diseases and Classes of Disease.

[Omitting Deaths in the Workhouse and Infant Poor-house, and such Deaths as could not be correctly referred to the Ward in which they occurred.]

	Ward 1.	Ward 2	Ward 3.	Ward 4.	Ward 5.	Ward 6.	Ward 7.	Ward 8.
All Cases.	267	221	253	534	347	210	257	456
Zymotic diseases ..	73	76	94	107	92	57	84	116
Tubercular diseases	39	39	59	87	45	43	48	89
Deaths under 5 yrs.	69	109	132	265	116	81	188	317
Phthisis ..	19	19	36	56	37	35	28	74
Dis. of Org. of Res.	15	29	35	79	37	29	45	74
Small Pox	10	3	0	3	1	0	3	5
Measles ..	17	3	6	14	9	1	6	4
Scarlatina ..	19	31	39	57	23	17	8	31
Diphtheria	3	1	4	13	6	4	2	9
Whooping Cough ..	3	7	14	17	16	11	10	23
Diarrhoea & Cholera	9	16	18	39	17	13	13	22
Fever ..	7	5	12	13	11	8	7	7

TABLE IV

New Cases of Disease coming under Treatment during the Week ending Saturday the 31st day of December, 1859, at the following Institutions:—

	Workhouse.	Infant Poor Hospitals.	Out-Patient Dispensary.	Holway Dispensary.	Peterson Conv. House.	Total.	London Fever Hospital.	Small-Pox Hospital.
Small-Pox	4	..	54	13	..	71	13	1179
Chicken-Pox	..	..	60	15	..	75	3	1
Measles	..	235	140	..	..	375	..	..
Scarlatina	..	1	130	161	..	292	106	2
Hooping-Cough	5	..	200	64	..	270	..	..
Whooping-Cough	..	..	..	..	..	..	..	..
Dysentery	47	..	389	363	92	1471	3	..
Diarrhoea	6	..	3	16	..	25	..	..
Cholera	..	7	4	4	..	11	..	..
Ague	1	..	12	18	..	31	..	..
Confined Fever	4	..	131	86	..	211	210	1
Intermittent Fever	..	..	46	13	..	59	..	..
Rheumatic Fever	17	..	54	29	..	101	6	..
Erysipelas	8	..	86	18	..	112	..	..
Diphtheria	..	..	11	7	..	18	..	..
Sore-throat or Tonsillitis	..	..	227	157	8	392	6	..
Cynura	..	..	..	..	..	..	..	..
Brachitis and Catarrh	148	..	1887	472	307	2014	10	..
Polivitis	..	..	22	11	..	33	..	18
Furunculæ	3	..	48	6	..	57	..	..
Cellulitis, Erysipelas	12	..	11	..	..	23	..	..
Ischuria	20	..	33	3	..	56	..	..
Chancres	3	..	15	2	1	21	..	..
All other Diseases	470	110	1608	3552	528	11860	168	7
Total	727	110	6065	2321	926	14860	268	118

*Forty-eight of these from Islington.
†Forty-eight were cases of Typhus and 172 Typhoid.

TABLE V.
COMPARATIVE TABLE OF SICKNESS AMONG PAUPER PATIENTS DURING
1857, 1858, & 1859.

	Total.			Per cent. of all Cases.		
	1857.	1858.	1859.	1857.	1858.	1859.
New Cases	9432	10591	11923			
Small Pox	23	37	38	0.24	0.34	0.48
Chicken Pox	35	45	72	0.37	0.42	0.6
Measles	138	248	238	1.46	2.28	1.99
Scarlatina	61	154	311	0.64	1.45	2.6
Whooping Cough	189	247	208	2.0	2.33	1.74
Croup	5	8	8	..	..	..
Thrush	10	39	39	..	..	..
Diarrhoea	1078	648	1026	11.4	6.1	8.6
Dysentery	6	18	25	..	..	..
Cholera	2	3	7	..	..	..
Mumps	} not specially enumerated.			25	..	..
Purpura, Scarry	2	3	5	..	..	..
Ague	9	26	16	..	..	..
Diphtheria	} not specially enumerated.			11	..	1.99
Sore Throat	} 227			..	..	..
Typhus and Infantile Fever	145	87	124	1.5	0.8	1.04
Metria	0	1	0	..	..	..
Rheumatic Fever	99	55	80	1.09	0.51	0.67
Erysipelas	54	88	92	0.57	0.83	0.77
Syphilis	70	55	49	0.74	0.51	0.41
Nema	5	4	6	..	..	..
Total of Zymotic Diseases	1933*	1866*	2627	20.49*	17.6*	22.0
Diseases of Respiratory Organs ..	1997	2353	2119	21.17	22.2	17.7
Phthisis	103	120	133	1.09	1.13	1.11
Carbuncles and Boils	44	67	49	0.46	0.63	0.41
Chronic and Local Rheumatism ..	299	375	517	3.17	3.54	4.33
Other Diseases	4668	5368	6013	49.49	50.6	50.4
Accidents and Injuries	388	442	465	4.11	4.17	3.9

* Exclusive of Mumps, Diphtheria, and Sorethroat.

TABLE VI.
HOUSES INSPECTED AND SANITARY IMPROVEMENTS EFFECTED.
From March 26th, 1859, to March 25th, 1860.

	West Sub-District.	East Sub-District.	Whole.
Number of Houses inspected	572	429	1001
Drains	Constructed Improved or repaired Traps fixed		
Cesspools	Abolished Cleaned or disinfected		
Privies and Water Closets ..	Pan, trap, and water supply furnished Pan and trap only furnished Water supply furnished		
Dust-Bins	Constructed Repaired and covers adapted		
Surface Drains and Pavements of Yards	Constructed Rebid		
General Water Supply	New receptacle provided Receptacle repaired and cleaned Water supply provided		
Other Improvements	Houses generally repaired cleaned or limewashed ventilated		

REPORT
ON
THE SANITARY CONDITION

Of the Parish of St. Mary, Islington,

DURING THE YEAR

1860.

BY

EDWARD BALLARD, M.D., (LOND.)

MEMBER OF THE ROYAL COLLEGE OF PHYSICIANS;

FELLOW OF THE ROYAL MEDICAL AND CHIRURGICAL SOCIETY OF LONDON, ETC.,

MEDICAL OFFICER OF HEALTH.

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1861.

REPORT ON THE SANITARY CONDITION

OF THE PARISH OF ST. MARY, ISLINGTON.

PRESENTED TO THE VESTRY OF ST. MARY, ISLINGTON.

1860.

EDWARD HALLAM, M.D. (Lond.)

PHYSICIAN TO THE PARISH OF ST. MARY, ISLINGTON.

REPORT ON THE SANITARY CONDITION

TO THE VESTRY OF ST. MARY, ISLINGTON.

GENTLEMEN,

1. While awaiting the result of the census just taken, it is my duty to present to you my Fifth Annual Report on the Sanitary condition of the Parish. Since it will be necessary when these results are in my possession, to travel again over the same ground, and to review our Sanitary position during 1860 and some previous years in the new light which will then be thrown upon it, I shall abstain now from drawing any general definite conclusions, limiting my remarks upon the Tables which form the basis of the Report within the boundaries that prudence under such circumstances appears to prescribe.

MORTALITY.

2. The deaths of residents in Islington which actually occurred between the 1st of January and the 31st December, 1860, amounted to 2845. To these must be added, as fairly belonging to us, 147 of the deaths which took place in the various general and special hospitals, lying-in institutions, and lunatic asylums in London. This addition raises our mortality to 2992, an excess of 181 deaths above the mortality of 1859.

3. The mortality of the two Sub-districts similarly corrected is represented in the West Sub-district by 1511 deaths, and in the East by 1481. That of the West, therefore, is higher by 16 than the mortality of 1859, and that of the East higher by 165 deaths.

4. The births registered during the year amounted to 5184, viz. 2655 males and 2529 females; the total number being precisely equally distributed between the two Sub-districts thus:—

1860	West Sub-District.			East Sub-District.			Whole Parish.		
	Males.	Females.	Total.	Males.	Females.	Total.	Males.	Females.	Total.
1st Quarter....	354	342	696	330	318	648	684	660	1344
2nd Quarter....	286	304	590	319	316	635	605	620	1225
3rd Quarter....	319	303	622	344	323	667	653	626	1279
4th Quarter....	364	320	684	339	303	642	703	623	1326
Total	1323	1269	2592	1332	1266	2592	2655	2529	5184

5. The deaths which occurred in each week and quarter of the year are represented in Table II. There was one week, viz. that ending June 23, during which only 25 persons died in the Parish: the lowest mortality during any week in five years. The low mortality of the third or summer quarter was unquestionably associated as an effect with the comparatively low temperature which prevailed and the abundant rainfall, which effectually cleansed the surface and flushed the drains and sewers.

6. Of the 2845 persons of whose deaths I possess the record, 1410 were males and 1435 females. 1382 or 48·5 per cent. were children under 5 years of age. The infant deaths in four previous years were 1066, 1108, 1238, and 1215. The infant mortality, then, exceeds that of 1859 by 167, a number which, within 14, represents the whole excess of mortality (181) over 1859.

CAUSES OF DEATH.

a. Zymotic Diseases.

7. Of the deaths recorded in Table I. 663 are attributed to maladies of the zymotic class. Under the classification now adopted, it will be observed that causes of death are now embraced under this class which were formerly excluded; nevertheless, the number of deaths recorded has been unusually small. The number tabulated in 1859 was 746. In the 'miasmatic' division, which pretty nearly corresponds with the former 'zymotic class,' the deaths amount to 632; viewed in this light the mortality thus represented amounts to 114 less than last year. It will be seen also by Table V. that the number of cases of disease of this class attended by the Parochial Surgeons fell short of the number of similar cases in the three previous years. This result is mainly due to the small amount of diarrhoea and other bowel affections, which the temperate summer brought out.

8. *Small Pox.* When the year 1859, to which my last Report referred, terminated, we were in the midst of an epidemic of small-pox. Since then the epidemic has in great measure died out, not, however, without leaving its impression on very numerous victims, 26 of whom, viz. 10 males and 16 females, it has consigned to the grave. We may now perhaps enter with advantage upon a brief retrospect of its course and progress. Thanks to the facilities afforded me by the Board of Trustees, the kind co-operation of the Parochial Medical Staff, of the Medical Officers of the Dispensaries and especially their Resident Officers, as well as of private practitioners, I am, I think, in a position to trace, although imperfectly, the outline of the course the epidemic pursued in Islington. I say imperfectly, for although I have been able to ascertain the occurrence and locality of 272 cases, yet there were very many others which I heard of by chance, the particulars of which I was debarred from obtaining. The places where these occurred appear in Table VII. The enumeration commences with the beginning of 1859, and proceeds month by month till December 1860.

The following is an abstract of the table:—

	CASES.								Total.
	Upper Holloway.	Lower Holloway.	Highbury.	Thornhill.	Brunswick.	St. Mary.	Canbury.	St. Peter's.	
1859—January ..	1	2	..	1	..	..	2	2	8
February ..	3	..	..	1	1	2	1	1	9
March	2	..	..	1	3	1	..	..	10
April	..	..	..	..	2	1	3	1	7
May	..	1	..	1	..	2	1	..	6
June	..	..	..	5	..	..	..	3	8
July	2	1	..	4	..	..	..	..	11
August	6	..	1	1	..	..	..	1	8
September ..	3	1	..	3	..	..	..	..	8
October	2	..	..	2	1	..	..	..	6
November ..	5	..	..	..	..	..	..	..	11
December ..	1	2	..	6	1	..	2	2	13
1860—January ..	3	5	1	27	5	..	..	3	44
February ..	1	5	3	10	1	1	..	2	23
March	4	4	5	11	1	..	2	11	38
April	1	2	1	..	4	1	..	11	20
May	1	1	1	4	1	..	..	9	16
June	..	..	1	..	..	..	..	..	1
July	3	..	..	..	..	..	2	2	7
August	2	..	1	2	..	..	..	1	1
September ..	..	..	..	..	..	..	..	..	2
October	..	1	..	..	..	..	1	..	2
November ..	..	..	..	1	3	..	1	..	5
December ..	..	..	..	..	4	1	..	..	5
Cases	40	27	13	79	27	10	16	60	272
Of which died ..	12	3	0	13	3	0	3	12	46

It appears, then, that while from 5 to 13 cases monthly came under my cognizance during 1859, the number rose in January 1860 to as many as 44; in February there were 23, in March 38, in April 20, and in May 16. The epidemic was now declining, 7 cases being the largest number returned to me from all sources during any succeeding month. The monthly mortality of the disease may be gathered from Table II. The largest number of deaths occurred in the months of January and February. In March, April, and May, although the disease still prevailed extensively, only 6 deaths occurred from it—its virulence was from some cause checked. This will appear more clearly on comparing the fatality of the cases prior and subsequently to the close of February 1860. Up to this time, according to the above table, 178 cases came under notice, of which 36 or 21 per cent. were fatal. From March to December 162 cases occurred, of which only 10 or 9·8 per cent. were fatal. This result is remarkable.

The parts of the Parish which, so far as my information extends, suffered most, were the Wards of Upper Holloway, Thornhill, and St. Peter's. These also were the Wards, as appears from Table III. of the present and last year's Report, in which the mortality was greatest. The parts which were least affected were Highbury and St. Mary's Wards. The streets, courts, &c., in which the disease appeared to spread most, were the following:—

In Upper Holloway—Gordon Court, in which I ascertained the occurrence of 7 cases and 3 deaths; Brunswick Road, in which I found 7 cases and 5 deaths; Bow-

- man's Place North and Bowman's Mews, in which together there were 9 cases and 2 deaths; Upper Whittington Place, 3 cases.
- In Lower Holloway—Blundell Street gave 4 cases and 1 death, and George Street 4 cases.
- In Thornhill Ward—I ascertained in Netherland Place 8 cases and 2 deaths; in Caledonia Street, 10 cases; in Sydney Street, 4 cases and 2 deaths; in Bemerton Street, 4 cases and 1 death.
- In Barnsbury Ward—I ascertained in Denmark Grove, 7 cases and 1 death; in Henry Place, 5 cases and 1 death.
- In St. Mary's Ward—I ascertained in Albert Square, 4 cases.
- In Canonbury Ward—I ascertained in Frederick's Place, Balls Pond, 3 cases and 1 death.
- In St. Peter's Ward—I ascertained in Pickering Street, 4 cases; in Sidney Grove (where scarlet fever formerly prevailed to a frightful extent—see Report for 1859, p. 5), 9 cases; in Elder Walk, 8 cases and 1 death; in Britannia Row, 6 cases and 1 death.

The names of most of these places are already familiar to you as those where the population is crowded, ignorant, and careless, and where other zymotic maladies flourish and extend.

It is interesting, and perhaps may be instructive, to notice the order in which these several districts were attacked by the epidemic. Compared with the population of the districts, the Ward which earliest exhibited the greatest proportion of cases was Upper Holloway, the locality earliest and most severely attacked being the neighbourhood of the Small-Pox Hospital. Even in 1858 two deaths from small-pox had occurred in Gordon Court. From July 1859 to March 1860, 27 cases, 11 of which were fatal—between a half and a third of the whole number—occurred within my knowledge in this Ward and neighbourhood. It is difficult to divert one's mind of the idea that this high fatality might not only have been attributable to the period of the epidemic, but to the large doses of the atmospheric poison imbibed. From Upper Holloway the disease appears to have travelled in its greatest intensity into Thornhill Ward. Cases appeared to have become more numerous there during the summer months of 1859, 14 cases, only 2 of which, or one-seventh, were fatal, having been noticed between May and September. In December, just as the disease appeared to be subsiding somewhat in Upper Holloway, came the first warnings of the renewed outbreak in Thornhill Ward, and between this time and March 53 cases occurred, of which 10, or about one-fifth, were fatal. Lower Holloway and Highbury Wards about the same time felt to a more moderate extent the shock of the epidemic, 16 cases with only 1 death, however, occurring in the former Ward, and 9 cases, without any death, in the latter. In fact, it is well worthy of remark that, in Highbury and St. Mary's Wards, no single death from small-pox occurred throughout the two years.

In St. Peter's Ward the violence of the epidemic appeared late. Cases had occurred throughout the epidemic, but had not exceeded 6 in the month (of November). As the disease, however, was subsiding in Thornhill, it began to spread more extensively in this part of the Eastern Sub-district, 31 cases having come to my knowledge during March, April, and May, 1860, of which 5 or one-sixth were fatal.

Taking the entire number of cases in each Ward, the fatality of the disease was

as follows, placing the Wards in the order of greatest fatality or virulence of the epidemic:—

Upper Holloway	30	per cent. of cases were fatal.
St. Peter's.....	20	ditto ditto
Canonbury	18	ditto ditto
Thornhill	16	ditto ditto
Lower Holloway	11	ditto ditto
Barnsbury.....	11	ditto ditto
St. Mary, Highbury.....	None	fatal.

The measures which were taken to arrest the progress of the epidemic in this parish may now be briefly recalled. Having placed myself in communication with the Board of Trustees in the month of January, when the disease was assuming serious proportions, I undertook to examine personally the children attending all the charitable schools in the Parish, with a view to satisfying myself as to their vaccination. In this work I was accompanied in each instance by one of the vaccinators of the district. The Trustees also provided me with the necessary assistance for the delivery of handbills in such localities as the information I received each morning pointed out as new foci of infection. These handbills, and large posting bills also distributed over the Parish, were designed to call public attention to the necessity of immediate vaccination, and in certain instances of re-vaccination. And, lastly, another most valuable and efficient assistant was provided me by the appointment of Dr. J. Thompson as a Vaccine Inspector, to inquire into the vaccination of all uncertificated children born within the previous year, with power to vaccinate when he thought it desirable—a power which he exercised under certain restrictions, which, for professional reasons, I thought fit to impose. The operation of all this was what was most to be desired. The public generally were awakened to the presence of the pest among them, and to the adoption of the best means to arrest its progress. Not only were the public vaccinations greatly increased, but private practitioners, without exception, informed me that the applications for vaccination made to them on the part of individuals and families were also very numerous. Similar steps were taken in other Parishes. I firmly believe that in this Parish the energetic measures adopted during the early months of the year, and the stimulus given to protective vaccination must be regarded as one of the main causes of the remarkable check which the epidemic received, and especially of that diminution in virulence which (apart from the spread of the malady) was noticed subsequently to March.

And now I am upon the subject of vaccination, I should like to place on record my conviction that the great sub-division of the public vaccination in this Parish is not calculated to promote its efficiency, because it interposes difficulties which no considerations of convenience to the poor can counterbalance. For such a Parish as this, three, or at most four, well-chosen vaccine stations are as many as ought to be provided, and at these the vaccination, except where there is immediate danger of small-pox, should be performed only once or at most twice a-week. The difficulty to be met in this, that in order to produce good pocks, and thoroughly to protect, the lymph should be taken from selected children, and used fresh from arm to arm. In order to do this, the district assigned to each vaccinator must be large enough, and the days of operation limited enough to bring together on each day eight or ten children, at least, who had been vaccinated eight days previously. From these the best arm could be selected

for perpetuation of the supply of lymph. I fear I am not making a very popular suggestion here, but I do it in the interest of the public health, that interest, which, in my position, I am bound first to consider. It is for the Board of Trustees to reflect upon this. Their motive for multiplying the stations as they have done is doubtless most creditable, but founded upon a mistake. The only alternative to this restriction, consistently with the public benefit, appears to be to throw open the public vaccination to all practitioners who chose to work it, and to pay the fee prescribed by the Act for every vaccination certified as successful by some independent Inspector. Private interest on this arrangement would be enlisted in the public service, for young unoccupied medical men might be expected readily to enter upon this branch of practice. Still this is an arrangement which would be open to other and grave objections.

Measles.—One hundred and thirteen deaths occurred from this disease, the average of the last four years being 77. The five Wards which constitute the southern half of the Parish were those which furnished the greatest number (see Table III). Judging from Table V. this malady has been more prevalent among the poor, as well as more fatal than in either of the other three former years.

Scarlet Fever occasioned 135 deaths during the year; last year the deaths were 216. The first and fourth quarters were those in which it chiefly prevailed. The number of cases which were attended on behalf of the Parish and by the Holloway Dispensary were 193; last year the cases thus attended amounted to 472. There has, therefore, been a decline in the prevalence of this disease. I have frequently called attention to the highly infectious character of scarlatina; let me add another fact which may assist to impress upon the minds of the parishioners the important lesson in preventive medicine which I have been so anxious to teach. Of the 135 fatal cases, 68 occurred in 39 streets, and 37 in 17 houses in those streets. In other words, out of 97 streets where scarlatina deaths took place there were 30 in which from 2 to 4 deaths occurred, and there were 17 houses where from 2 to 3 deaths occurred. Measles is an infectious disease, but the difference in infection and in fatality appears from this, viz., that while there were 18 streets, &c., in which 42 deaths occurred, or from 2 to 4 in a street, there were but 4 houses in which 2 deaths occurred from measles in the year. Can I more strongly enforce my repeated warning that cases of scarlet fever should be immediately isolated, and, where a house is at all crowded, removed as soon as possible to the Fever Hospital? I cannot avoid expressing a belief that had these ordinary precautions been taken, some, at least, of these 37 lives sacrificed to scarlet fever might have been saved.

Whooping Cough.—This is another disease, the prevalence of which, as well as the mortality occasioned by it, has been below the average. Of 92 deaths 35 took place in the course of the last quarter of the year.

Fever.—The deaths attributed to continued and infantile fever, together, amounted to 76; the mean of the previous four years being 82.

Bowel complaints, diarrhoea, dysentery, and cholera, together, occasioned only 80 deaths; in former years the numbers were 120, 147, 99, and 135. During the summer quarter they proved fatal to only 49 persons, the number in previous summer being 83, 104, 65, and 109. The number of cases attended by the Parochial Surgeons also shows the favourable influence of this remarkable season in relation to these disorders. They amounted altogether only to 509, the number in former years being 1086, 668, and 1058.

Diphtheria appears to have become an established disease amongst us like measles or scarlet fever. Thirty-five deaths, 22 of children under 5 years of age, were referred to it. This, however, seems to be some improvement upon former years.

b. Constitutional Diseases.

9. Five hundred and twenty-three deaths are referred in Table I. to this class of diseases. The most important and definite malady it embraces is *pulmonary consumption*. To this 313 deaths have been attributed; the mean of four previous years being 305. The total number, and that of the deaths in each Sub-district, is nearly the same as in 1858.

c. Local Diseases.

10. To the so-called local diseases 1218 deaths were attributed. *Diseases of the organs of respiration* carried off 622, or more than half of these persons. So high a mortality from these maladies has not occurred during the previous four years, the numbers being 392, 422, 494, and 375. Stated, in periods of 13 weeks each, from January 1 to December 29, the deaths in each quarter were—

1st quarter	251,	the mean of four years being	143
2nd ditto	127,	ditto	87
3rd ditto	60,	ditto	48
4th ditto	174,	ditto	141

Of these deaths 338 were under 5 years of age, giving an increase of infant deaths for this over the former years of 77 from these diseases. It is thus that the cold and wet weather we experienced made up to us the mortality which a hot summer would have brought about in another way.

d. Developmental Diseases.

11. Among the 354 deaths tabulated thus, we find those of 128 infants (107 of whom were under 1 year of age), attributed to "atrophy and debility." The deaths of 62 are said to be due to their having been born prematurely. A glaring defect in our system of birth registration prevents us from either knowing or investigating the number of children said to be "still-born."

e. Violent Deaths.

12. Of these there were 72. Forty-three are attributed to accident or negligence, 10 to suicide, and 12 were held to be the result of premeditated violence—they were murdered persons. True, they were infants that were murdered, but the little insight even thus afforded us into the gulf of crime of which these murders are the last and deepest products, should induce reflection. While considering this class of deaths we stand with one foot upon the domain of the moralist, but only with one. Religious teaching, training in schools, churches, missionary efforts to reclaim the outcast and fallen are good enough and useful enough in their way, but what can religious teaching, what can moral training avail, when home influences and home surroundings are such as to forbid, I was about to say religious and moral practices, but the merest decencies of family life. What, think you, would be the verdict if the true history were known of the 67 deaths of *illegitimate* and deserted children included in the mortality tables for 1860? 20 of them—a most disproportionate number—were said to have died from "atrophy," "continual wasting," "congenital debility," &c.; how many of these were the victims of wilful neglect? 4 of them were said to have died

from "want of breast milk"—three in Islington Workhouse between the 10th of October and the 2nd of November. The proportion of mothers who cannot suckle their infants, if they choose, is not 4 in 67. Ten, or more than one-sixth, of those under 2 years died in "convulsions!" Again a large proportion. Brain symptoms, which thus terminate, and may thus be registered, arise from other besides natural causes. Ten were downright murdered, one having had its throat cut; one was permitted to bleed to death from the navel; three died from venereal contamination.

SANITARY WORK OF THE YEAR.

13. Table VI. is a summary of the house inspections made, and the amendments effected in dwelling-houses during the year ending March 23, 1861. It does not however, include all the work carried out under the superintendence of your Sanitary Officers, as the removal of offensive and injurious accumulations of various kinds, the discontinuance of the use of underground sleeping-rooms, and the lessening, after notice from the office, of the number of inhabitants in various overcrowded houses. Frequently during the year, and especially in the summer, fruit, fish, and meat, in a state unfit for food, has been seized and destroyed. The filthy and unwholesome state of many of the stable and cab yards in the Parish, to the effluvia from which on several occasions I have seen good reason to attribute fatal disease in their vicinity, has induced me to undertake a systematic inquiry in relation to them. In the West Sub-district 66 stables have been inspected, and in the East eighty. Two very unhealthy cow-sheds have been done away with, one situated in George's Place, which I referred to in my 34th Monthly Report, and another (after summoning the owner) in the Lower Road. Prior to the annual licencing in October 52 slaughter-houses were inspected in the West Sub-district, two of which were opposed; the licences to those two were refused by the Magistrates. In the East Sub-district the number of slaughter-houses inspected and licensed was 59.

I have the honour to be, Gentlemen,

Your very obedient Servant,

EDWARD BALLARD, M.D., M.R.C.P., etc.

Vestry Offices,

May 10th, 1861.

Medical Officer of Health.

Postscript.—Since the above was written, the total population of the Parish, as ascertained by the recent census, has (subject to revision) been made known. It amounted to 155,291. The ten years just elapsed have consequently added to our numbers 59,962 persons. This is an increase at the rate of 5 per cent. each year, upon the population of the preceding one. Assuming this rate to have been uniform through the ten years, the population of each may be readily calculated in the same way as the accumulation of capital at compound interest.

The following Table exhibits the population of Islington thus estimated at the spring of each year from 1856 to 1860. The total increase occurring during each year showing approximately how much has been natural increase by births exceeding deaths, and how much by immigration from other places; together with the rate of mortality observed each year:—

Year.	Estimated Population.	Total Increase occurring during year.	Natural Increase occurring during year from excess of Births over Deaths.	Proximate Increase by excess of Immigration over Emigration.	Difference of Increase by Immigration over preceding year.	Corrected Mortality.	Deaths per 10,000 living.
1856 ..	121668	6083	1873	4210	..	2456	270
1857 ..	127751	6387	2035	4352	+ 142	2556	200
1858 ..	134138	6707	1771	4936	+ 584	2791	208
1859 ..	140845	7043	2186	4857	— 79	2811	199
1860 ..	147888	7403	2192	5211	+ 354	2992	202
Mean ..	134458	6724	2011	4713		2717	202

Nothing can be more satisfactory than these results of our battle against unsanitary influences. Islington stands in respect to the *mode of increase of its population* in a remarkable position. During the ten years from 1851 to 1861, the increase of population in London by immigration of strangers was 1-4th less than the natural increase by excess of births over deaths. In Islington, during the five years of my official connexion with the parish, the increase by immigration was absolutely one and a third times greater than that by natural increase. Of course, whatever sanitary advantages the older residents in the parish have enjoyed, these new comers every year bring in themselves the results of those less sanitary conditions in which they have hitherto dwelt, and which render the general death-rate of England about 220 in the 10,000. No one can study the table I have given above without seeing three things—first, that notwithstanding our rapid growth by immigration, our average death-rate has been comparatively low, viz., 202 per 10,000; secondly, that wherever in the five years the death-rate has risen, it has been in connexion with the increased influx of strangers from without (see years 1858 and 1860); thirdly, that in the course of the year even these strangers, incorporated into our community, derive the benefit of the sanitary advantages afforded them in a diminished death-rate (comparing the death-rate of 1859 with that of 1858). Were our increase to be simply or mainly that by birth, it is probable that our death-rate would not exceed 190 in the 10,000.

E. B.

June 20th, 1861.

TABLE I.

DEATHS WHICH HAVE OCCURRED IN ISLINGTON AND ITS SUB-DISTRICTS DURING THE YEAR 1904.

West.	East.	Cause of Death.	All Ages.	AGES.													
				Under 1	1-2	2-3	3-4	Under 5	5-10	10-15	15-20	20-25	25-30	30-35	35-40	40-45	45-50
437	1408	All Causes	2845	747	326	107	98	54	1382	134	45	135	153	171	168	214	259
(Classes.)																	
347	316	I. ZYMOTIC DIS. ...	663	157	129	74	51	36	447	77	25	25	21	18	12	8	10
267	256	II. CONSTITUTL. ...	623	70	46	22	17	6	161	15	9	80	84	71	41	29	21
611	607	III. LOCAL ...	1218	291	122	64	25	12	604	29	11	26	39	74	105	166	187
172	182	IV. DEVELOPMNL. ...	354	189	26	5	2	..	222	..	..	2	3	4	1	5	30
38	34	V. VIOLENT DTHS. ...	72	32	3	2	2	..	39	9	..	3	5	4	2	1	..
2	13	Not Specified	15	8	..	..	1	..	9	..	..	1	1	..	2	..	1
(Orders.)																	
327	303	I. 1. Miasmatic Dis. ...	633	133	128	74	51	36	422	77	25	23	19	17	12	7	10
29	27	Small Pox	35	5	..	..	..	..	13	1	..	..	..	..	..	..	..
62	54	Measles	113	35	43	24	9	5	105	9	1	1	..	..	..	..	..
59	76	Scarlatina	133	12	17	18	10	26	57	12	5	3	1	..	..	..	..
11	24	Diphtheria	33	3	5	4	5	2	22	9	1	..	..	..	..	..	..
..	..	Quinsy	2	1	..	..	..	..	..	..	..	..	..	..	..	..	..
9	14	Croup	23	2	..	..	..	..	31	2	..	..	..	..	..	..	..
44	24	Whooping Cough ..	59	28	33	15	6	4	85	6	..	..	..	..	..	..	..
..	..	Typhus & Typhoid ..	26	2	3	4	3	7	19	15	5	3	6	2	4	3	1
4	9	Erysipelas	12	5	1	..	..	..	6	..	..	..	..	..	..	..	..
..	..	Parapneumonic Fever ..	4	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Carbuncle	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Infarction	7	3	1	..	..	..	4	..	..	..	..	..	..	..	..
4	1	Dysentery	5	2	3	..	..	..	5	..	..	..	..	..	..	..	..
69	40	Diarrhea	68	43	19	4	3	..	62	..	..	..	..	..	..	..	..
..	..	Cholera	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Ague	1	1	..	..	..	..	1	..	..	..	..	..	..	..	..
..	..	Rheumatism	17	..	1	..	..	..	1	2	2	1	2	2	3	1	..
..	..	2. Euthetic	7	7	..	..	..	..	7	..	..	..	..	..	..	..	..
..	..	Structure of Uterus ..	7	7	..	..	..	..	7	..	..	..	..	..	..	..	..
..	..	Hydrophobia	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Cholera	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
13	6	3. Diabetic	10	12	1	..	..	..	13	..	..	..	..	..	..	..	..
..	..	Privation	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
11	..	Want of Br. Milk ..	13	12	1	..	..	..	13	..	..	..	..	..	..	..	..
..	..	Purpura and Scoury ..	4	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Alcoholism	5	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	4. Parasitic	5	5	..	..	..	..	5	..	..	..	..	..	..	..	..
..	..	Tubercle	5	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Worms	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
24	39	II. 1. Diabetic	63	1	..	..	..	..	3	..	1	5	12	9	13	12	6
..	..	2. Heart	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Dropsy	13	..	..	..	..	..	..	..	..	..	..	..	..	..	..
16	24	Cancer	32	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Noma	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Mortification	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
343	217	2. Tubercular	460	69	46	22	15	6	158	10	9	79	59	32	16	9	..
..	..	Scrofula	16	2	2	1	..	..	8	1	2	1	..	..	..	..	..
32	15	Tubercle	37	21	7	4	1	..	33	2	5	78	92	24	8	..	..
153	159	3. Pulmonary	313	10	19	..	..	..	32	2	..	..	..	..	..	..	..
50	44	Hydrocephalus	94	35	81	6	16	3	85	8	1	..	..	..	..	..	..
137	157	III. 1. Nervous Syst. ...	204	79	31	16	10	3	139	9	5	6	12	24	33	38	20
..	..	Cephalitis	26	1	3	3	1	..	17	2	1	1	1	3	3	1	..
24	35	Apoplexy	40	1	..	..	..	..	1	..	..	..	..	..	..	..	..
22	25	Paralysis	48	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Insanity	5	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Chorea	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Epilepsy	9	..	..	..	..	..	..	..	..	..	..	..	..	..	..
45	62	Convulsions	107	59	29	9	4	2	107	..	..	..	..	..	..	..	..
15	23	Brain Diseases, Ac. ...	39	4	3	..	..	..	12	1	2	..	..	..	..	..	..
55	58	2. Organs of Circ. ...	115	2	1	..	..	..	3	3	1	4	10	24	30	29	6
..	..	Pericarditis	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Aneurism	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..
54	55	Heart Disease	189	5	1	..	..	..	3	3	1	4	9	23	29	26	6

TABLE I.

DISTINGUISHING THE AGES AND CAUSES OF DEATH (Deaths of Strangers in Hospitals excluded.)

			AGES.																		
West.	East.	Cause of Death.	All Ages.	Und 1	1-2	2-3	3-4	Und 5	5-10	10-15	15-20	20-25	25-30	30-35	35-40	40-45	45-50	50-60	60-70	70-80	Up 85
327	295	3. Respirat. Orgs. 662	150	89	35	15	9	338	7	3	11	13	29	35	66	83	53	4	..	..	..
..	..	Laryngitis	17	6	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Tracheitis	17	6	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Pneumonia	194	85	28	16	6	4	154	1	1	4	3	9	11	7	7	..	..	..	..
..	..	Long Disease, Ac. ...	6	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	4. Digestive Orgs. 128	15	1	..	..	..	..	20	7	1	4	9	12	18	32	21	..	..	..	..
..	..	Gastritis	19	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Enteritis	11	3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Peritonitis	10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Ascites	7	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Obstruction of Intest. ...	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Hemorrhage	7	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Intussusception	5	1	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Stricture of Intest. ...	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Fistula	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Stomach Dis. Ac. ...	9	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Pancreas Dis. Ac. ...	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Hepatitis	11	7	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Jaundice	9	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Liver Disease	41	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Spleen Disease	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	5. Urinary Organs 34	..	..	..	..	..	..	1	6	2	4	6	3	9	11	5	..	..	..	..
..	..	Nephritis	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Ichthæmia	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Nephritis	18	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Diabetes	4	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Stone	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Cystitis	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	6. Gen. Orgs. ... 15	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Gonorrhea	5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Uterine Disease	9	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Other Diseases	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	7. Gr. Orgs. of Locomot. ...	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Arthritis	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Joint Diseases	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	8. Skin, Ac. ... 9	..	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Eczema	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Pimples	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Ulcer	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Skin Diseases	7	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
58	38	IV. 1. Die of Children 64	82	11	1	..	..	..	94	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Prenatal Death	69	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Cyphosis	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Spina Bifida	3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Other Malformations	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	2. Die of Adults 10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Furunculæ	10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Chancroids	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	3. Diseases of Age 122	10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Old Age	122	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	4. Die of Nutrit. 122	107	15	4	2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Atrophy and Debility	122	13	7	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
27	15	V. 1. Accid. or Neg. 43	19	3	2	2	..	..	26	8	..	..	..	..	1	1	3	1	..	..	..
..	..	Fall	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Wounds	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Burns and Scalds	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Poison	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Drowning	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Suffocation	20	15	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Otherwise	12	12	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	2. Homicide 10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	3. Suicide	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	4. Wounds	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Poison	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	Drowning	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	5. Hanging	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	6. Poison	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	7. Suffocation	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
..	..	8. Unexplained	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..

TABLE II.
DEATHS OCCURRING IN EACH WEEK AND QUARTER, DISTINGUISHING CERTAIN IMPORTANT DISEASES AND AGES.
[Deaths of Non-Residents in Hospitals, and of Hospital Patients in Hospitals elsewhere, not included.]

WEEK ENDING		Jan.	Jan.	Jan.	Feb.	Feb.	Feb.	Mar.	Mar.	Mar.	Apr.	Apr.	Apr.	May	May	May	Jun.	Jun.	Jun.	Jul.	Jul.	Jul.	Aug.	Aug.	Aug.	Sep.	Sep.	Sep.	Oct.	Oct.	Oct.	Nov.	Nov.	Nov.	Dec.	Dec.	Dec.	4th Quarter		
		7	14	21	28	5	12	19	26	3	10	17	24	31	7	14	21	28	5	12	19	26	3	10	17	24	31	7	14	21	28	5	12	19	26	3	10	17	24	
Total		66	53	57	61	62	78	79	88	85	63	75	81	77	65	59	56	55	58	44	42	57	25	45	61	59	58	54	51	42	43	44	45	46	47	48	49	50	51	52
Under 3 years		21	23	29	29	30	34	35	38	37	36	35	32	29	25	23	21	19	15	13	10	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	
79 years and upwards		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1		
Small Pox		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1		
Measles		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1		
Diphtheria		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1		
Scarlatina		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1		
Whooping Cough		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1		
Diarrhoea & Cholera		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1		
Unlabeled		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1		
Mean Temperature		57.2	57.3	57.4	57.5	57.6	57.7	57.8	57.9	58.0	58.1	58.2	58.3	58.4	58.5	58.6	58.7	58.8	58.9	59.0	59.1	59.2	59.3	59.4	59.5	59.6	59.7	59.8	59.9	60.0	60.1	60.2	60.3	60.4	60.5	60.6	60.7	60.8	60.9	

TABLE III.
Showing the Number of Deaths registered in each Ward, distinguishing those from certain important Diseases and Causes of Disease.
[Omitting Deaths in the Workhouse and Infant Poor-house, and such Deaths as could not be correctly referred to the Ward in which they occurred.]

	Ward 1.	Ward 2.	Ward 3.	Ward 4.	Ward 5.	Ward 6.	Ward 7.	Ward 8.
All Causes	175	274	338	543	597	334	319	389
Zymotic diseases	40	60	67	143	73	59	75	108
Tubercular diseases	30	30	50	87	45	47	32	79
Deaths under 5 yrs.	64	145	154	309	199	104	154	274
Phthisis	16	31	38	56	30	29	25	69
Dis. of Org. of Res.	21	69	63	145	55	47	71	110
Small Pox	3	0	0	10	2	0	1	7
Measles	1	8	7	28	15	16	14	18
Scarlatina	14	19	19	34	13	13	17	23
Diphtheria	2	3	8	6	3	0	4	8
Whooping Cough	3	9	10	20	11	4	8	11
Diarrhoea & Cholera	7	7	7	16	7	10	11	13
Fever	1	9	8	7	14	3	3	10

TABLE IV.
New Cases of Disease coming under Treatment during the Year ending Saturday the 29th day of December, 1860, at the following Institutions:—

	Workhouse.	Infant Poor House.	Old Patient Paupers.	Infirmary Dispensary.	Hallway Dispensary.	Post-office Central Dispensary.	Total.	London Fever Hospital.
Small Pox	..	..	111	18	12	1	143	9
Chicken Pox	..	..	59	14	23	..	96	12
Measles	..	1	365	91	80	..	578	123
Scarlatina	..	1	133	66	69	..	369	94
Whooping Cough	..	..	4	1	112	16	133	..
Croup	..	..	10	..	4	..	14	..
Diarrhoea	20	..	459	88	112	..	672	..
Dysentery	..	..	1	..	..	..	1	..
Cholera	..	..	..	..	..	..	..	..
Ague	..	..	..	..	..	..	..	..
Continued Fever	..	..	62	8	82	..	152	115
Purpura Fever	..	..	..	..	..	..	..	..
Rheumatic Fever	16	..	33	14	18	..	57	..
Erysipelas	..	..	84	39	..	..	123	..
Diphtheria	..	..	..	..	6	..	6	..
Scarlatina	..	..	..	61	..	..	61	..
Furunculosis	..	..	..	..	123	10	256	..
Pharyngitis	..	..	249	727	895	325	4396	..
Bronchitis and Catarrh	167	8	249	727	895	325	4396	..
Ischuria	..	..	11	..	..	..	11	10
Ischuria	..	..	29	2	..	..	34	12
Ischuria	..	..	7	1	..	..	8	14
Insanity	37	..	40	40	..	1	61	..
Carbuncle	..	..	8	1	..	..	9	..
All other Diseases	410	119	5278	8567	5183	1142	12517	..
Total	660	119	5278	8567	5183	1142	20509	374

* = Typhoid.

† Of these 26 were cases of "typhus" and 17 of "typhoid" fever.

TABLE V.
COMPARATIVE TABLE OF SICKNESS AMONG PAUPER PATIENTS DURING
1857, 1858, 1859, and 1860.

	Total.				Per cent. of all Cases.			
	1857.	1858.	1859.	1860.	1857.	1858.	1859.	1860.
New Cases	9432	10591	11923	10348				
Small Pox	23	37	58	111	0.24	0.34	0.48	1.07
Chicken Pox	35	45	72	59	0.37	0.42	0.6	0.57
Measles	138	348	238	386	1.46	3.28	1.99	3.73
Scarlatina	61	154	311	124	0.64	1.45	2.6	1.19
Whooping Cough	189	247	208	157	2.0	2.33	1.74	1.51
Croup	5	8	8	10	..	..	..	..
Thrush	10	39	39	31	..	..	..	..
Diarrhoea	1078	648	1026	490	11.4	6.1	8.6	4.73
Dysentery	6	18	25	18	..	..	..	..
Cholera	2	3	7	1	..	..	..	..
Mumps	not specially enumerated.	25	6	..	..	..	..	..
Purpura, Scurvy	2	3	5	2	..	..	..	..
Ague	9	26	16	22	..	..	..	..
Diphtheria	not specially enumerated.	11	2	..	..	..	..	..
Sore Throat	not specially enumerated.	227	181	..	..	1.99	1.76	..
Typhus and Infantile Fever..	145	87	124	64	1.5	0.8	1.04	0.61
Metria	0	1	0	2	..	..	..	..
Rheumatic Fever	99	55	80	45	1.49	0.51	0.67	0.43
Erysipelas	54	88	92	86	0.57	0.83	0.77	0.83
Syphilis	70	55	49	39	0.74	0.51	0.41	0.37
Noma	5	4	6	6	..	..	..	..
Total of Zymotic Diseases ..	1933	1866	2627	1842	20.49	17.6	22.0	17.8
Diseases of Respiratory Organs	1997	2353	2119	2733	21.17	22.2	17.7	26.6
Phthisis	103	120	133	111	1.09	1.13	1.11	1.07
Carbuncles and Boils	44	67	49	39	0.46	0.63	0.41	0.37
Chronic & Local Rheumatism	299	375	517	516	3.17	3.54	4.33	4.92
Other Diseases	4668	5368	6013	4705	49.49	50.6	50.4	45.4
Accidents and Injuries	388	442	465	382	4.11	4.17	3.9	3.69

TABLE VI.
HOUSES INSPECTED AND SANITARY IMPROVEMENTS EFFECTED.
From March 26th, 1860, to March 25th, 1861.

	West Sub. District.	East Sub. District.	Whole Parish.
Number of houses inspected	1006	699	1705
Drains			
Constructed	12	46	58
Improved or repaired	63	100	163
Traps fixed	141	172	313
Cesspools			
Abolished	21	40	61
Cleansed or disinfected	2	1	3
Privies and Water Closets			
Pan, trap, and water supply furnished	25	6	31
Pan and trap only furnished	..	4	4
Water supply furnished	22	143	165
Dust-Bins			
Constructed	38	32	70
Repaired and covers adapted	40	89	129
Surface Drains and Pavements of Yards			
Constructed	43	8	51
Relaid	46	19	65
General Water Supply			
New receptacle provided	25	6	31
Receptacle repaired and cleansed	1	58	59
Water supply provided	..	..	..
Other Improve- ments			
Houses generally repaired	21	67	88
" cleansed or limewashed	58	153	171
" ventilated	1	..	1

TABLE VII.

List of Localities where Cases of Small-Pox are known to have occurred each month during 1859 and 1860, indicating the Fatal Cases also.

<i>January, 1859.—Small-Pox Cases, 8.</i>	
5, Hampden Road.	
1, Waterloo Place.	
59, Cleveland Road.	
Golden Lion, Great William Street.	
Water's Court.	
31, Arundel Square.	
18, Canonbury Street.	
6, Sidney Grove.	
<i>February, 1859.—9 Cases.</i>	
1, Edward Street, King's Cross.	
1, Church Street, Arlington Square.	
D 5, Gordon Court, 3 cases.	
1, Vittoria Street.	
1, Church Street.	
9, Northampton Street.	
2, Albert Square.	
<i>March, 1859.—10 Cases.</i>	
6, Milton Place, Holloway.	
1, Edward Street, King's Cross, 2nd case.	
6, Albert Square, 2nd in Albert Square.	
6, Gordon Court, 4th in this house.	
Workhouse, 2 cases (tramps).	
12, Gordon Court, 5th in Gordon Court.	
2, Paradise Court.	
6, Albert Square, 2nd in this house and 3rd in Albert Square.	
<i>April, 1859.—7 Cases.</i>	
21, Park Street.	
D 13, Alpine Villas. [ampton Street.	
4, Northampton Street, 2nd in North-	
4, Church Road West.	
Workhouse, 2 cases.	
D 3, Dalby Terrace.	
<i>May, 1859.—5 Cases.</i>	
2, Luard Street. [4th in square.	
2, Albert Square, 2nd in this house and	
2, Queen's Place.	
19, Halton Street.	
D 4, Brandon Road, Belle Isle.	
<i>June, 1859.—8 Cases.</i>	
Bryan Street Reformatory, 2 cases.	
D 29, Upper Binglefield Street.	
6, George Street, St. Peter's.	
12, Adelaide Square.	
9, Pickering Street.	
23, Twyford Street.	
10, Walter Place.	
<i>July, 1859.—11 Cases.</i>	
D White Horse, Metropolitan Cattle	
D 56, Shepperton Terrace. [Market.	
53, Alfred Street.	
3, Red Cap Court.	
7, Eton Grove, John Street. [house.	
9, Pickering Street, 2 cases, 3rd in this	
23, Twyford Street, 3 cases, 4th in this	
house and family.	
10, Pickering Street, 4th in this street.	
<i>August, 1859.—8 Cases.</i>	
4, Victoria Road.	
D 1, Archway Place.	
7, St. John's Park.	
D 58, Brunswick Road.	
Slope Cottages, Highgate Hill.	
D 3, Bowman's Place North.	
Cottonham Road.	
D 29, Randall's Row.	
<i>September, 1859.—8 Cases.</i>	
3, Bowman's Place North, 2nd in this	
D 16, Upper Whittington Place. [house.	
10, William Street, St. Peter's.	
31, Randall's Row, 3 cases, 4th in this	
6, Junction Road.	
D 34, Blundell Street.	
<i>October, 1859.—6 Cases.</i>	
6, Junction Road Villas.	
17, Denmark Street.	
1, Thornhill Bridge Place.	
16, London Street.	
D 28, Brunswick Road, 2nd in this road	
<i>November, 1859.—11 Cases.</i>	
10, Bowman's Place North, 3rd in this	
street.	
D 15, Frederick Place, Goswell Road.	
16, Upper Whittington Place, 2nd in	
this house.	
York Hotel, Duncan Street.	
DD 13, Newhall Street, 2 cases.	
12, Sidney Grove, 2nd in this court.	
7, Greenman's Lane.	
2, Bowman's Place North, 4th in this	
street. [this street.	
DD 30, Brunswick Road, 2 cases, 4th in	
this street.	
<i>December, 1859.—13 Cases.</i>	
D 15, Whittington Terrace.	
7, Pleasant Row.	
D 2, Clyde Terrace.	
9, Thornhill Square.	
Balmoral Castle, Caledonian Road.	
7, Clephane Road.	
D 3, St. James Street.	
7, Upper Freeling Street.	
D 29, Sidney Street.	
23, Windsor Street.	
3, Devonshire Street.	
5, Cornelia Terrace.	
D 149, City Road.	
<i>January, 1860.—44 Cases.</i>	
Bowman's Lodge.	
14, Denmark Grove.	
D 7, Gordon Court, 6th in this court.	
15, Gifford Street.	
DD 4, Netherland Place, 4 cases.	
18, Caledonian Crescent.	
2, Churchill Place. [this house.	
7, Upper Freeling Street, 4 cases, 5th in	
8, Felix Street.	
9, Clifton Gardens.	
8, Paltney Street.	
6, Norfolk Gardens.	
4, Clifton Gardens, 2nd in this place.	
1, George Street.	
8, George Street, 2nd in this street.	
15, Netherland Place, 2 cases, 6th in this	
place.	
D 11, Cumming Street North. [place.	
6, Netherland Place, 7th in this place.	

TABLE VII.—Continued.

3, Netherland Place, 8th in this place.	
6, Upper John Street.	
21, Popham Street.	
Caledonia Street, 10 cases.	
4, Park Place, Highbury Vale, from Mel-	
ton Mowbray. [cases.	
11, Charles Street, Wellington Road, 3	
D 2, Brunswick Road, 5th in street.	
D Small-Pox Hospital, from Islington	
workhouse.	
<i>February, 1860.—23 Cases.</i>	
23, Sidney Street, 2nd in this house.	
8, River Street.	
4, Newington Crescent.	
4, William Street.	
D 15, Gordon Court, 7th in this court.	
D 49, Richmond Road.	
6, Lower Street.	
D 32, Bemmerton Street.	
10, Bath Place, Church Street.	
22, Woodland Terrace, 2 cases.	
D 12, Sidney Street, 3rd in this street.	
2, Hope Place West, 2 cases.	
5, George's Grove.	
D 2, Churchill Place, 2nd in this house.	
3, Almira Road.	
1, Clayton Street.	
4, Thornhill Street.	
2, Ebenezer Cottages, Malden Lane.	
8, George Street, 2nd in this house, 3rd	
in the street.	
1, George Street, 2nd in this house, 4th	
in the street. [house.	
D 18, Caledonia Crescent, 2nd case in this	
street.	
<i>March, 1860.—38 Cases.</i>	
45, Bemmerton Street, 2nd case in street.	
17, Upper Binglefield Street, 2nd in this	
street.	
2, Ebenezer Cottages, 2nd in this house.	
19, Britannia Row.	
1, Swan Yard.	
3, Nelson Place, 3 cases.	
19, Story Street.	
17, Upper Whittington Place, 3rd in	
73, Windsor Street, 2nd in this street.	
8, Gifford Street, 2nd in this street.	
14, Tavistock Terrace. [street.	
D 14, Upper Binglefield Street, 3rd in this	
4, Roman Road.	
7, Andover Grove, Hornsey Road.	
23, Sherborne Street.	
62, Brunswick Road, 6th in this street.	
13, Little Queen Street.	
19, Graham Street.	
26, Edward Terrace.	
45, Britannia Row, 2nd in this street.	
7, Sidney Street, 4th in this street.	
45, Bemmerton Street, 2nd in this house,	
17, Lavinia Grove. [3rd in street.	
D 6, Henry Place.	
12, Sidney Grove, 4 cases, 5th in this	
house, 6th in court.	
13, Sidney Grove, 7th in the court.	
31, Edward Square.	
D 6, Britannia Row, 3rd in this street.	
2, Jordan Place, Elder Walk.	
2, Victoria Place, Queen's Road, 2 cases.	
7, Hope Street, 2 cases.	
<i>April, 1860.—20 Cases.</i>	
15, Brunswick Place.	
4, Litchfield Terrace.	
Cole's Cottage, Blundell Street, 2 cases,	
3rd in this street.	
2, Jordan Place, Elder Walk, 2nd in this	
house. [place.	
4, Jordan Place, Elder Walk, 3rd in this	
house. [cases.	
16, Sidney Grove, 8th in this court.	
6, Henry Place, 2nd in this house.	
11, Sidney Grove, 9th in this court.	
6, Henry Place, 2 cases, 4th in this house.	
7, Henry Place, 5th in this place.	
7, Astley's Row. [Elder Walk.	
4, Cole's Place, Elder Walk, 4th in	
D 4, Elder Walk, 6th in Elder Walk.	
4, Cole's Place, 2nd in this house, 7th in	
Elder Walk.	
25, Paradise Place, 2 cases.	
14, Prospect Place.	
6, Thornhill Street, 2nd in this street.	
6, Bryan Place.	
8, Ward's Place, 2 cases. [cottages.	
5, Baldwin's Cottages, 5th case in these	
55, Havlock Street. [street.	
21, Bowman's Place North, 5th in this	
3, Cole's Place, 8th in Elder Walk.	
D 11, Graham Terrace.	
<i>June, 1860.—1 Case.</i>	
1, Blackstock Lane.	
<i>July, 1860.—7 Cases.</i>	
1, Bowman's Mews, 3 cases.	
3, Tibborton Square.	
45, Britannia Row, 4th in this street.	
9, Frederick's Place, Balls Pond.	
4, Northampton Grove.	
<i>August, 1860.—7 Cases.</i>	
29, Gifford Street, 3rd in this street.	
29, Bowman's Place North, 6th in this	
street.	
48, Britannia Row, 2 cases, third in this	
house, 6th in the street.	
25, Brunswick Road, 2nd in this house,	
7th in this road.	
64, Bemmerton Street, 4th in this street.	
Highbury Vale.	
<i>September, 1860.—1 Case.</i>	
D 108, Packington Street.	
<i>October, 1860.—2 Cases.</i>	
16, Blundell Street, 4th in this street.	
24, Frederick Place, 2nd in this place.	
<i>November, 1860.—5 Cases.</i>	
9, Denmark Grove, 2nd in the grove.	
D 24, Frederick Place, 2nd in this house,	
3rd in the place.	
59, Albert Street.	
D 3, Edward Street.	
<i>December, 1860.—5 Cases.</i>	
D 10, Denmark Grove, 3 cases, 4th in this	
house, 6th in grove.	
5, Denmark Grove, 7th in the grove.	
9, Chapel Street.	

REPORT.

THE Interim Report by Mr. NAPIER in May, 1857, exhibited the progress which had been made up to that time in the Experiments undertaken for the purpose of deodorising and precipitating the sewage waters of Pinkstone Burn. Since that period considerable progress has been made in our inquiries, and though the subject is still far from exhausted, we are now in a position to indicate to the Sanitary Committee the conclusions at which we have arrived, and to make some suggestions as to the mode in which the sewage of towns may be most advantageously treated.

The experiments at the Pinkstone Burn were, in the first instance, confined to examining the action of slacked lime upon the water, applied in a manner very similar to that which has been used at Leicester, Manchester, and elsewhere. The results of these experiments have been fully narrated in Mr. Napier's Report, but it is necessary to recur to them shortly here for the purpose of rendering intelligible some of the conclusions to be afterwards stated.

The average daily run of the Pinkstone Burn amounts to about two million gallons, and this quantity of water, under ordinary circumstances, carries down with it in mechanical suspension about 7 tons of solid matter, or about 2555 tons per annum, capable of producing from 12000 to 15000 tons of mud of ordinary consistence, which must be annually deposited in the estuary of the Clyde. The complete precipitation of

the whole water was found by Mr. Napier to require about 54 tons of lime per diem, but as from the defective nature of the apparatus there was a considerable loss of lime, Mr. Napier is of opinion that, with a more satisfactory arrangement, 4 tons of lime would be sufficient for the purpose. The effect of the lime upon the water was quite instantaneous, the smell immediately disappeared, and the precipitate began to sink to the bottom, leaving a fluid which was perfectly clear, although generally of a light straw tint. The smell did not return even after some days exposure to the air at a summer heat, but in particular states of the water was partially restored by the addition of strong acids. The precipitate, in a perfectly dry state, amounted to 5110 tons annually, but if merely dried sufficiently to admit of its being easily carried off in carts, it would amount to about 20,000 tons yearly, or 55 cart loads for every working day.

The cost of the operation, assuming 4 tons of lime per day to be sufficient, and including the expense of removing the precipitate from the tanks in the moist state would amount to £1,370 annually, so that the expense of precipitation alone, exclusive of drying, would be 5s. 4d. for each ton of dry matter obtained.

It is somewhat difficult to estimate the annual cost of treating the entire sewage of the City with lime, because no data exist for accurately determining its total quantity, but excluding the Clyde and the Kelvin, it cannot be less than 20 times the volume of the Pinkstone Burn or 40 million gallons per day. The daily supply of water from the Water Works is now 17 or 18 million gallons, and is expected to be still larger when the Loch Katrine water is introduced, all of which must of course pass into the sewers, and in addition, there are the Molendinar Burn and other streams, with a large amount of water from the Canal and various local contributions which go to swell the amount.

It is possible that some of the other sewers contain a larger amount of solid matter than the Pinkstone Burn, but assuming the latter to be a fair sample of the rest, and about one twentieth of the whole, the gross result would be as follows:—

The annual deposit of mud between Glasgow and Greenock, exclusive of what may be brought down by the Clyde, the Kelvin and the Cart, would be about 320,000 tons, and if the whole of the sewage were subjected to precipitation by lime, the amount of the soft residuum capable of being carted away would be 408,800 tons per annum, or 1,300 cart loads for every working day, and if reduced to a perfectly dry state it would amount to 102,200 tons. The annual cost of the mere precipitation and removal of the deposit from the tanks would be £27,400.

Our next object was to ascertain the probability of any part of this large outlay being recovered by the sale of the deposit as a manure, and for this purpose it was necessary to have a proper estimate of its manurial value. As removed from the tanks the deposit contains a quantity of moisture amounting on the average to about 80 per cent., but varying within pretty wide limits in different samples, and in order that fairly comparable results might be obtained we have determined the value of the dry matter after complete removal of the moisture by exposure to a temperature of 212°.

A specimen which was considered to be a fair average of a large quantity of the dry Pinkstone Burn deposit was found to contain—

Organic Matter,	-	-	-	-	-	47.49
Peroxide of Iron,	-	-	-	-	-	2.65
Alumina,	-	-	-	-	-	7.97
Phosphoric Acid,	-	-	-	-	-	1.44
Carbonate of Lime,	-	-	-	-	-	29.41
Magnesia,	-	-	-	-	-	0.50
Potash,	-	-	-	-	-	0.39
Soda,	-	-	-	-	-	0.38
Chlorine,	-	-	-	-	-	0.16
Sulphuric Acid,	-	-	-	-	-	0.70
Sand,	-	-	-	-	-	8.91
						100.00
Ammonia,	-	-	-	-	-	3.51

Another sample prepared in exactly the same manner, but at a different time, gave on partial analysis:—

Phosphoric Acid, - - - - -	4.17
Ammonia, - - - - -	2.62

In estimating the value of manures it has been customary to compare them with Peruvian Guano, which, from its extensive use and well known properties, affords a convenient standard, and calculating on this principle, the first of these samples is worth £2 13s. per ton, the second, about £2 5s. Assuming the deposit as removed from the tanks to contain 80 per cent. of water, it would be worth about 10s. per ton; but in this crude and moist state it would, of course, be quite unsaleable, and it would be absolutely necessary to get rid of the greater part of the water so as to leave a material containing about 80 per cent. of solid matter, which would then be worth about £2 per ton. No data founded on experience on the large scale exist for determining the cost of the drying process to which the deposit would have to be subjected. Mr. Wicksteed, who is the patentee of the process in operation at Leicester, estimates it at 4s. 2d. per ton, and he states the whole cost of manufacture, including interest on capital, to amount to 12s. 8d. per ton. This estimate appears to us to be much too low, and we are of opinion that it could not be safely reckoned at less than double that sum, or say £1 5s. per ton.

As the manure, according to the analyses already given, is worth about £2 per ton, it at first sight appears that a considerable profit is to be derived from this operation. But it is important to notice that though the value just given is obtained by putting upon its constituents the same prices as those used in valuing guano, it could not compete with it or other concentrated manures at that rate, and the reason for this becomes obvious when we contrast the total expenses of the application of a ton of guano and an equivalent quantity of the lime precipitate to the soil. This may be most simply illustrated by reference to a particular case. Supposing a ton of guano to be carried 10 miles by railway and 5 miles by road, the entire cost of its application to the soil would be as follows:—

1 Ton Guano, - - - - -	£13 0 0
Cartage to Station, - - - - -	0 1 6
Carriage by Railway 10 miles, - - - - -	0 0 10
Cartage 5 miles, - - - - -	0 5 0
Application, - - - - -	0 0 8
	£13 8 0

But in order to produce the same manurial effect the farmer must employ $6\frac{1}{2}$ tons of the sewage manure, and the cost would be:—

$6\frac{1}{2}$ tons Lime deposit, - - - - -	£13 0 0
Cartage to Station, - - - - -	0 9 0
Carriage by Railway 10 miles - - - - -	0 5 5
Cartage 5 miles, - - - - -	1 12 6
Application, - - - - -	0 4 4
Total cost of Lime deposit, - - - - -	£15 12 0
„ of Guano, - - - - -	13 8 0
Difference, - - - - -	£ 2 4 0

Hence the $6\frac{1}{2}$ tons of sewage deposit cost £2 4s., or almost exactly 7s. per ton more than its value. A farmer so situated could not afford to give more than £1 13s. per ton for the sewage manure, and for every mile of greater distance from the manufactory a proportionately increased deduction would have to be made. If we add to this the expense of agents for the sale of the manure, the risk of bad debts, and various other commercial charges, it will be obvious that the profit to be derived from the manufacture is highly problematical. But in all that precedes we have taken the most favourable view of the case, and assumed that the deposit from the whole sewage of Glasgow would be equal to that obtained in the course of the experiments at the Pinkstone Burn, an assumption which the experience of other places by no means warrants. On the contrary, the deposit in question is of materially higher quality

than any yet examined, as will be obvious from the subjoined estimates of the value of the absolutely dry deposits deduced from the analyses of different chemists:—

Leicester, - - -	£0 15 5	(Voelcker.)
Do., - - -	0 17 0	(Versmann.)
Clifton Union, - - -	0 16 9	(Hofmann.)
London, - - -	1 18 8	(Do.)
Pinkstone Burn, - - -	2 13 0	(Anderson.)
Average, - - -	£1 8 2	

In comparing these analyses we are at once struck by the greatly inferior value of the three first, and the difference becomes still more interesting when we know that they are deposits actually prepared on the large scale as manufacturing products; while the two last were made for experimental purposes, and under the immediate superintendence of scientific chemists who watched the whole details of the process in each instance. It is impossible to resist the inference that when the operation comes to be carried out by ordinary workmen, and upon the whole sewage of Glasgow, the product will more nearly resemble that obtained at Leicester and Clifton. But assuming it to reach the average of the whole samples, and making a deduction of one-fifth on the supposition that the manure as delivered to the consumers would contain 20 per cent. of water, its value would not exceed £1 2s. 6d. per ton, and even this price could not be obtained in practice, because it would unquestionably vary much in composition at different times, and in any such manufacture the quality of each quantity sent out of the works could not be guaranteed, so that it would be necessary to fix the price, not at the average, but at the minimum value of that produced. To this must be added the uncertainty of sale, a difficulty which has been experienced to a great extent at Leicester, where, though offered at 3s. per ton, it has accumulated for want of sale in immense quantities. It is probable that the manure would be more readily purchased by the farmers in this neighbourhood, but when it is borne in mind that the total produce of the Glasgow sewers may

amount to perhaps 120,000 tons per annum, its complete absorption for agricultural purposes is very unlikely. It is obvious also that unless each year's production were sold within its own year the whole economy of the works would be disarranged and additional expenses entailed.

On the conclusion of the lime experiments your reporters were desirous of submitting to examination the process of precipitation, patented by Mr. Manning, to which their attention had been specially directed by the Sanitary Committee. Application was accordingly made to that gentleman, and he was informed that the tanks erected at the Pinkstone Burn would be placed at his disposal, for the purpose of illustrating his process and affording an opportunity of determining its value. After much correspondence and delay, Mr. Manning objected to the construction of the tanks, which he considered were not well adapted to his process, and stated that he should prefer exhibiting it at the works he was then erecting at Edinburgh. Our object being to examine the process under the most favourable circumstances, Mr. Manning's proposition was readily agreed to, and he undertook to give us due notice when his apparatus was ready for work. After repeated delays, Mr. Manning at length announced that his preparations were complete, and Mr. Napier proceeded to Edinburgh for the purpose of inspecting the process which it was resolved should be carried out entirely by Mr. Manning and in the manner which he thought best. Mr. Napier adhered strictly to this arrangement, confining himself to noting the quantities of the materials used and the results, and having obtained specimens of the precipitants and of the product he returned to Glasgow.

Mr. Manning's works are situated by the side of an open sewer at the South Back of the Canongate, Edinburgh, and consist of two precipitating tanks communicating with a third smaller and on a lower level, intended for the reception of the deposit after precipitation and from which it is lifted by a Jacob's ladder into a large vessel where the fluid part is drained off and it is converted into a dry manure. Each of the precipitating tanks is furnished with a stirring apparatus

composed of long wooden arms driven by steam, and close to them is placed a large tub from which the precipitating ingredients after being mixed with water are allowed to flow into the tanks.

The precipitating ingredients are three in number:—

1st. Alum sludge; a refuse matter consisting chiefly of sub-sulphates of iron, and alumina deposited during the evaporation of the fluid from which alum is prepared. Its cost at the alum works is 7s. 6d. per ton.

2d. Prussiate maker's charcoal which is a refuse product of the manufacture of prussiate of potash, containing about 30 per cent. of charcoal along with a good deal of peroxide of iron and saline matters. Its price is from 5s. to 7s. per ton.

3d. Refuse lime from the manufactures of chloroform, obtained free of cost.

The average quantity of sewage let into the tanks at each filling amounted, according to Mr. Napier's measurement, to 29,219 gallons, and the charge of precipitating materials employed was 6 cwt. of alum sludge, 3 cwt. of waste lime and 2 cwt. of charcoal. These materials were mixed with water in the tub already mentioned, and when the tank was nearly full the stirring apparatus was set in motion and the precipitating mixture allowed to flow into the water during its passage into the tanks. The stirring apparatus was kept in motion for about ten minutes after the tank was full, when it was stopped and the deposit allowed to subside, which it did after the lapse of from three-quarters of an hour to an hour, when the supernatant fluid was run off. The tank was then again filled and the agitator set in motion, by which means the deposit was stirred up and served to precipitate a second quantity of the water without further addition of the ingredients. After a considerable quantity of the deposit had accumulated, it was only necessary to add the precipitants to every third charge of the tanks, and when it became too bulky it was allowed to flow into the deeper tank from which it passes to the filtering apparatus where the further process of manufacture is carried out.

Taking into account the repeated use of the precipitants, Mr. Napier assumes that the charge of the materials added in the operations he inspected serves on the average for precipitating 73,047 gallons of sewage. On this assumption the cost of precipitating 1,000,000 gallons, and the quantity of materials required will be as follows:—

4 tons 2 cwt. Alum Sludge,	-	-	£1 10 9
1 „ 7 „ Charcoal,	-	-	0 8 3
2 „ 1 „ Waste Lime,	-	-	0 0 0
			£1 19 0

giving for the total sewage of Glasgow £28,470 per annum, which estimate, however, does not include the cost of carriage of materials, labour, nor the expense of removing the deposit from the tanks.

The success of Mr. Manning's process as inspected by Mr. Napier was far from perfect, for though the grosser impurities of the sewage were precipitated, the fluid was in no instance either limpid or inodorous, but always retained a muddy appearance and distinct sewage smell. It is only justice, however, to Mr. Manning, to observe that the cause of this defect may be fairly attributed to the fact that his object at Edinburgh is not so much to deodorise the sewage as to manufacture a manure from it, and hence in his operations the quantity of precipitants is reduced to the minimum requisite for the latter purpose. Mr. Napier found that when the charge of the precipitants was increased by about a third, a perfectly limpid and inodorous fluid was obtained, but in order to secure this result it was necessary to add the same proportion of the ingredients every time the tank was filled, thus at once increasing the cost of the operation to about three times that already given.

The necessity for using a much larger quantity of the precipitants was further borne out by experiments on the small scale with the water of several of the Glasgow sewers. Mr. Napier experimented on the Pinkstone, Gallowgate, and Gorbals sewers in this way, and found that to thoroughly

precipitate and deodorise a million gallons each, would require the following quantities of the materials:—

	Sludge.	Lime.	Charcoal.
Pinkstone, - - -	19,500 lbs.	12,600	6,700
Gallowgate, - - -	16,600 "	11,400	5,400
Gorbals, - - -	40,600 "	50,000	26,000

and if we suppose a mixture of equal quantities of the water of these sewers to be operated upon, the expense per million gallons would be—

14 tons 13 cwt. Alum Sludge, - - -	£5 10 1½
5 " 15 " Charcoal, - - -	1 13 6½
11 " Lime, - - -	0 0 0
	£7 3 8

Thus making the entire annual charge for the City of Glasgow to amount to £104,876.

This estimate it must be understood was obtained not from experiments in the tanks, but by operating on the small scale and cannot therefore pretend to absolute precision, but it is a sufficient approximation to the truth to show that the outlay requisite for completely deodorising the sewers of Glasgow, will materially exceed the estimate formed from Mr. Manning's operations at Edinburgh.

Independently of the increased quantity of the precipitants, there are other sources of expense which are not taken into consideration in these estimates, but which must be incurred in carrying out the process on the large scale.

It is obvious that whenever the operation extends to a whole town no adequate supplies of waste lime can be obtained free of expense, and it would consequently be necessary to purchase lime from the kiln, adding at the lowest estimate and assuming that one ton of quick lime will do the work of four tons of waste lime, about £6 per day or £2,190 a year to the expense, while at the higher estimate, the additional expenditure would be £20 a day or £7,300 a year. The same observation

applies to alum sludge, of which the production is necessarily limited by the manufacture of alum. We do not know the exact proportion which the sludge bears to the alum produced, but it is less than five per cent., and as the total quantity of alum manufactured in Scotland is under 6000 tons, the annual production of sludge cannot exceed 300 tons, while the quantity required to precipitate the Glasgow sewers amounts, at the lowest estimate, to nearly 30,000 tons.

Mr. Manning has foreseen this difficulty and has taken a subsequent patent for the manufacture of a substitute for alum sludge. He proposes to treat alum shale in exactly the same manner as that at present in use for the manufacture of alum, but in place of drawing off the fluid from the sludge produced during the boiling, the whole is to be evaporated to complete dryness, by which means a residue is obtained which has nearly four times the precipitating power of the common sludge, and which Mr. Manning expects to produce for 5s. per ton. Had this report been addressed to persons familiar with the manufacture of alum, we should have left this statement to speak for itself, for they would at once see the impossibility of obtaining the product in question at this rate. We may state, however, that it requires 5 tons of good alum shale to produce one ton of alum, but as the quantity of such shale is limited it would be necessary to use that of inferior quality, of which, perhaps, 10 or 20 tons would be required to produce a ton of Mr. Manning's material; and taking this and the general expenses of such a manufacture into consideration, we are fully satisfied that it could not be produced for less than £2. As 4 tons of alum sludge, which are capable of producing the same effect, can be obtained for £1 10s., the use of the new product would add about 30 per cent. to the estimates already given.

In estimating the value of the precipitate obtained by Mr. Manning's process, exactly the same steps were followed as in the lime experiments. Mr. Napier selected a fair sample of the precipitate produced at Mr. Manning's works at Edinburgh during his visit, which was brought to Glasgow, and analysed after drying at 212°. The results were:—

Organic matters, - - - - -	49.96
Peroxide of iron and alumina, - - - - -	13.39
Lime, - - - - -	6.22
Magnesia, - - - - -	0.52
Potash, - - - - -	0.29
Soda, - - - - -	0.17
Common salt, - - - - -	0.16
Sulphuric acid, - - - - -	0.90
Phosphoric acid, - - - - -	1.38
Carbonic acid, - - - - -	5.61
Sand and siliceous matters, - - - - -	22.00
	100.00
Ammonia, - - - - -	2.56

The value of this manure in the dry state is therefore about £2 2s. per ton, or assuming the manufactured article to contain 20 per cent. of moisture, its price would be £1 14s.

As this value is considerably below that of the lime precipitate, we think it unnecessary to enter into further details regarding Mr. Manning's process, as all the observations made in reference to the former apply with double force to the latter, and we have no hesitation in expressing our opinion that there is no prospect of Mr. Manning's plan being carried out with profit.

As a commercial speculation, therefore, the manufacture of the sewage residuum into a manure is by no means hopeful. It is scarcely necessary for us, however, to remark that the question of profit is a matter of comparatively little importance, and we should consider that we had sufficiently fulfilled the requirements of the Sanitary Committee if we could show that the disinfection of the Glasgow sewage could be carried out with a certain loss, the limits of which could be accurately defined. But we are not in a position to do this. The experiments we have made regarding the two processes already described have enabled us to determine the cost of mere precipitation, and we should have no difficulty in furnishing similar data regarding any other process submitted to us should that appear desirable. It is, however, impossible for us to

determine with any degree of certainty the cost of the further processes required to bring the product into a marketable condition, because we have no means of testing the plans, generally involving rather complicated mechanical arrangements, which have been proposed for the purpose. Such opinions therefore, as we have ventured to express upon this part of the subject, have been derived from our knowledge of branches of manufacture involving the drying and reducing to a portable state similar commercial products and being fully cognisant of the expense attending these operations, we are satisfied that they would prove by far the most expensive part of the process, while it is also questionable whether any of them could be carried into operation on the large scale without producing a serious nuisance in the neighbourhood of the works.

On the other hand we have no data for estimating the price the farmer could afford to give for the deposit. In the preceding pages we have chosen to compare it with Peruvian Guano, because that substance has been commonly used as a standard of comparison with other artificial manures. But the justice of applying this system of valuation to a sewage deposit is to say the least of it very doubtful, for practically each great class of artificial manures must be and actually is estimated by reference to a standard of its own, which is fixed by purely commercial considerations. The force of this may be best understood by the statement of the fact that, if the mode of valuation in use for Peruvian Guano be applied to farm yard manure, its value will be found to vary between 10s. and 15s. per ton, while its current price is only 5s. As far as general composition is concerned sewage manure is really in many respects much more comparable to farm yard manure than to guano, and with the fact just stated regarding the former, we are satisfied that the sum obtainable for the sewage precipitate would be much below the estimates we have formed, and in that case the loss on the operation would be very large, while the risk of the product accumulating at the works would be exceedingly great.

While we found these opinions to some extent on the results of our experiments, we must also observe that they are borne

out by a common sense consideration of the whole subject, and we are particularly anxious to place this in a distinct point of view, because it appears to us that the public are greatly misled by the current statements regarding sewage matter. It is frequently stated that the annual value of the excreta of each individual of the population amounts to about £1, on which principle the total value for the City of Glasgow is estimated at £400,000. Without entering into any discussion regarding the precise accuracy of this estimate, it will suffice for our purpose to remark that this value could only apply to it *in a marketable condition*, while the very first operation of our present sanitary arrangements, by diluting these matters with an enormous quantity of water renders them entirely unmarketable and consequently annihilates their value, and then the assistance of the chemist is demanded in the hope that he may be able to undo this work and render back the matters which have been thus made valueless, with the additional proviso that they shall be inoffensive and innoxious. We have no hesitation in asserting that this problem does not admit of an *economic* solution, and we are satisfied that the first step towards arriving at useful practical results is to disabuse the public mind of any such expectation. No process of precipitation can possibly recover the whole of these excreta, because the most important and valuable of their constituents cannot be thrown down by any agent, and must necessarily escape with the clear water, while that actually precipitated carries down with it a large quantity of valueless matters, which increase the bulk and deteriorate the value so much as to render the product of little practical use, and prevent its competing, except at a mere nominal price, with concentrated manures.

While there is no prospect of a successful result attending any of the processes of precipitation as at present proposed, the idea that chemical agency cannot be usefully applied must not be hastily adopted, for any engineering operations which would effectually remove the sewage from the River Clyde, would be so expensive that a considerable annual loss resulting from the chemical purification of the water would be considerably under the interest of the engineering outlay. With a view to the

elucidation of this matter we have considered the whole question in a different point of view, and shall now proceed to state generally the plans which we think merit consideration.

None of the methods of deodorising sewage appear to us to go to the root of the matter and they are all defective, chiefly because they have been hampered with the condition of recovering the valuable matters and converting them into a marketable manure, which makes it necessary to apply the process to the sewers at their outlet. Admitting the possibility of doing this in an effectual manner, it is obvious that though a great boon to the population on the banks of the river it would be of comparatively little advantage to the inhabitants of the city itself, because the sewers would continue to be filled with noxious and offensive matters stagnating in them often for a long period of time and undergoing putrefaction there. In the case of a small town where the fall is good, the condition of the sewers themselves is of comparatively little importance, for their contents scarcely remain in them sufficiently long for putrefaction to advance to any great extent, but rapidly reach their outlet generally in a river, where they mix with a large volume of running water, which is the most important and powerful natural deodoriser. But in a large town, where the fall is defective, the sewage remains for a long time in the sewers, and when it at length reaches the river, putrefaction has reached a stage at which running water is comparatively powerless to arrest it. For exactly the same reason, deodorisers, applied to the sewage, after it has stagnated in the sewers, must be either larger in quantity or of a more powerful nature than would be necessary if the water could be treated when it enters in place of when it leaves the sewers, and whether it be taken in an economic or a sanitary point of view, our impression is, that deodorisation ought to take place within the sewers themselves. In fact, the perfection of such an arrangement would be to have each house furnished with a reservoir of some deodoriser which might be allowed to flow in a continuous current into its sewers, but it is scarcely necessary to observe, that any such plan could not be easily carried out in practice, and the nearest approach to it, which could be successfully worked, would be

to divide the city into a certain number of districts, in each of which the minor sewers should be made to converge to some central point, where a station should be erected for throwing a deodoriser into the main sewer. These stations would have to be arranged in such as to admit of the greatest possible amount of fall in the small sewers, so that their contents may reach the deodorising station as rapidly as possible. Beyond this point the fall in the main sewers would be of less importance, although even the disinfected sewage should be got rid of without unnecessary delay.

The most important consideration in carrying out a plan such as that now referred to, is the substance to be employed as a deodoriser. It is obvious that any agent which deodorises by precipitation would be objectionable owing to the risk of the precipitate accumulating in and choking the sewers—an event almost certain to occur, except in places where an unusual amount of fall could be secured. For this reason lime and all other substances hitherto suggested, which deodorise only when added in sufficient quantity to produce a deposit, are precluded, and it is necessary to seek for some substance of opposite properties, and which shall deodorise without precipitating, or, if possible, shall increase the tendency of the insoluble matters to remain in suspension. The latter requirement cannot well be fulfilled in practice, nor is the number of substances which deodorise without precipitating very large. That which will at once suggest itself to every chemist as the agent most likely to be economically applied to this purpose is sulphurous acid, which is well known to be a powerful deodoriser and disinfectant. Dr. Smith and Mr. M'Dougall of Manchester, who have devoted much attention to the use of sulphurous acid, have proposed to employ it along with carbolic acid, a substance extracted from coal tar and remarkable for its powerfully antiseptic properties, so that when a mixture of these two agents is employed the sulphurous acid deodorises the putrid matters, while the carbolic acid arrests the tendency to further change. Dr. Smith and Mr. M'Dougall consider that two pounds of sulphurous acid and an ounce of carbolic acid will suffice for deodorising the excreta of 300 persons for one

day, and that the cost of these materials will be about a penny, which gives, for the annual expense of deodorising a town of 400,000 inhabitants, about £1900. They propose to prepare their deodoriser at different stations throughout the town, and throw it at once into the sewers; and they state that each of these stations will cost £500, and require the services of a man and a boy. The plan thus described is no more than a general sketch, and does not permit us to form a satisfactory estimate of the total expenses. Indeed, the complete arrangement would involve a consideration of the general distribution of the sewers, so as to determine the number of deodorising stations which it would be necessary to erect, and on this point, as being a matter of detail, we have not as yet ventured to enter. It is easy, however, to form some sort of general idea from the data which have been furnished; and we would remark that the estimated quantity of sulphurous and carbolic acids is founded on the assumption that the excreta of each individual contain 2 oz. of dry matter per day, but as drainings from stables, the refuse of manufactories, and various other animal and vegetable matters find their way into the sewers, it may be safely assumed that double the quantity stated by Dr. Small and Mr. M'Dougall will be required in practice, and the cost of the material will consequently amount to about £400 per annum. It is probable that about 20 different stations for the manufacture of the deodoriser will be required for the City of Glasgow, involving, therefore, an outlay of £10,000, and we may estimate the cost as follows:—

Cost of materials, - - - -	£4000
Wages (20 men and 20 boys), - - -	1250
Interest on outlay, and wear and tear, -	1000
	£6250

The sum actually required may be greater or less than this, and must depend on the amount of deodoriser and the number of stations found necessary on practice; but we think it would not be safe to estimate it at less than £6000.

As regards the prospect of this plan proving successful, our statements must also be of a very general nature. We have made various experiments on a small scale which leave no doubt as to the deodorising effect of the mixture of sulphurous and carbolic acids, but from the nature of the process it is difficult to estimate in this way the quantity which would be required on the large scale, because we have no guide such as is offered by precipitation for determining when the operation is complete, and are compelled to trust to the sense of smell, which is a somewhat fallacious index. We have resolved, therefore, to make an experiment on a sufficient scale to deodorise the whole of the Kelvin, and to keep it so for some time so as fully to test the efficacy of this process. Mr. M'Dougall has accordingly prepared for us a large quantity of material for the purpose of testing his process, and we are only waiting a suitable opportunity for the trial. Unfortunately the state of the sewers during the present summer has been very unfavourable for such experiments, because from the large fall of rain they have been unusually clear, and the Kelvin has been almost free of smell, so that we are compelled to reserve our experiment until its state is such as to afford us a satisfactory proof of the effect these agents produce.

It may be objected to this plan that though it might deodorise the river, it would still leave it as disagreeable as ever to the eye. But we do not attach much importance to this objection, for it is not to be expected that any process applied to the sewers will convert the Clyde into a clear and pellucid stream, while on the other hand, should it ever be desirable to attempt the application of the Glasgow sewage to the purpose of liquid manuring, the use of the deodoriser should be found to act well in practice, would do away with the great objection which has been always offered to that process, for the deodorised sewage could be applied to the land and even conveyed in open conduits without producing the slightest nuisance to the neighbourhood.

We would by no means have it supposed that we suggest this as a plan to be at once adopted, but merely as a subject meriting further enquiry, which may or may not prove practically useful on the large scale, for we believe it is only by pretty

large and somewhat long continued experience that all the advantages and disadvantages of the process can be satisfactorily elucidated. But having satisfied ourselves that, though the precipitation and deodorisation of sewage can be well effected by lime and other processes at a cost which can be accurately defined, the precipitate itself in place of being a set off against this cost is more likely to increase it by entailing the necessity for further manufacturing and large commercial transactions, the expense of which cannot be estimated, we have thought it right to indicate the plan which, so far as we can see, combines the greatest amount of sanitary advantage with the smallest expenditure compatible with the existing or any modified arrangement of sewers.

Looking at the question in its widest aspect, it is doubtful whether any of these plans reach the root of the evil, and we are inclined to go much further and to express our belief, that the principle on which the refuse matters of towns are now universally got rid of is radically wrong. The system of sweeping them away by means of an abundant supply of water is no doubt very simple, and has certain advantages, provided it could be effectually carried out. But the imperfect success attending it in practice is sufficiently attested by the present state of the sewers in all our large towns. Nor can we fail to see that a great part of this want of success is the natural and unavoidable consequence of a system which, by the facilities it affords for getting rid of refuse, renders individual sanitary precautions less a duty than they were under the older arrangements. The sewers are open to all without let or hindrance, and every description of waste is indiscriminately thrown into them in its most offensive condition, and provided an individual gets rid of his own refuse, he considers himself at liberty to disregard the convenience and comfort of his neighbour, or at all events, to throw upon the public the burden of getting rid of a nuisance which he might have abated at a less cost than the public can, and it may be in some instances with absolute profit to himself. The cure for all this lies in the fact, that there is really a large amount of preventable contamination of our sewers, which should never be allowed to flow into them,

but should be preserved apart and submitted to some process for the preparation of manures, which it would then be possible to produce with much greater prospect of success than after they have been diluted with the enormous quantities of water passing through the sewers. Many manufacturers are in this predicament, and as a remarkable instance of the practical application of the principle, now actually in use, Teal's patent for the recovery of the fat from the soap liquor of wool scouring may be mentioned, which process is successfully in use in Glasgow and many other places, with the effect of converting a very filthy matter into a clear though not a colourless fluid. We would not by any means have it supposed that similar results are to be obtained from the refuse of all manufacturing processes, but no doubt there are others to which some mode of purification might be applied.

These observations apply with equal force to excrementitious matters; and we are fully persuaded that if these substances are to be converted into a manure with any prospect of profit, it must be by excluding them from the sewers and preserving them in an inoffensive state by means of a dry deodoriser. If this could be successfully done, the advantage to the sewers is sufficiently obvious, for it is these excrementitious matters which are the most offensive part of their contents, and it would have the effect also of doing away with the existing system of water-closets, which in a sanitary point of view, are quite behind the age. In expressing this opinion, we are well aware that we run the risk of being misrepresented and described as the advocates of an obsolete system which has been properly abandoned, and the offensive and noxious character of the old plan will be contrasted with the many advantages of the water-closet. We by no means advise a return to the old system in its primitive form, for that cannot for a moment be supported, but it appears to us that had the ingenuity which for a long series of years has been devoted to the contrivance of ingenious water-closets, been turned to plans for the effectual application of dry deodorisers, that the result in a sanitary point of view would have been quite as good, probably better, and the product might be converted by

a simple process into a manure which would probably sell at about £3 per ton. We refrain from entering into details regarding this plan, but its importance must not be underrated. At present but a small proportion, not above one-fourth of the houses in Glasgow are furnished with water-closets, and their rapid increase must tend to magnify very greatly the evils at present experienced. The plans proposed for precipitating and deodorising the sewers are no more than palliatives, and with the extension of the city and the increase in the number of water-closets, these palliative measures must become day by day more costly, so that it is really a matter for grave consideration whether the attention of the public should not be directed to the contrivance of some substitute for the conveniences now in use, which shall fulfil sanitary requirements without contaminating the sewers, and yield a marketable manure.

The importance of adopting some plan of excluding from the river the offensive contents of the sewers, has been made still more obvious, by a series of experiments which have been going on simultaneously with those relating to the precipitation and deodorisation, and which have an important bearing on both the chemical and engineering features of the case. The experiments, though not yet carried out as far as is desirable, already exhibit very important data for our guidance in determining the best mode of treating the sewage, and of preventing the pollution of the river. While it was well known, and would probably be universally admitted, that the sewage of the city, after flowing into the Clyde, remained there a long time, driven backwards and forwards by the tides, before it finally escaped to sea; and while it would most likely also be admitted, that the improvements in the river, which have so largely benefitted the navigation, by increasing the depth of water, have, at the same time, penned back the natural current of the stream, and retarded the downward passage of the fresh water and the sewage; the extent of the evil, and the length of time which was really required to carry the sewage clear of the river, were unknown, and could scarcely be guessed at with a probability of being near the truth.

Experiments were therefore suggested for the purpose of ascertaining how long the sewage remained within the upper reaches of the river; and they were constructed by Mr. Ure, with the consent of the Clyde Trustees, partly in June, 1857, and partly during the present summer. The latter are not yet complete, but the data obtained in 1857, seems to give a general idea of the facts. The experiments were made with a number of floats, introduced into the river at various points between the Victoria bridge, at Glasgow, and Dumbarton rock. These floats were followed by two men in a boat, night and day, in their journeys up and down the river, as they were carried backwards and forwards by the tide, for several days together. The floats were put into the water at high water of spring tides, a day or two before the highest tides, the natural volume of the river being very low at the time. This period of tide would be the most favourable for the rapid descent of the sewage, which may be supposed to have entered the river at the same time, and the experiments were not conducted long enough to determine what would have been the state of things with the sewage which would enter at low water of neap tides. They are, therefore, not yet as complete as they ought to be, but they nevertheless throw considerable light on the subject, and enable us to make some proximate calculation.

The results are pretty much the following.—That sewage, entering the river at Victoria Bridge at high water of spring tides, when the natural volume of the river is small, as in summer or dry weather, will not reach Govan Ferry in less than a week, nor the mouth of the River Cart in less than a fortnight. The distance to Govan Ferry is about 2½ miles, and to Cart mouth 6 miles; so that, the average downward progress is something less than half-a-mile a day. In fact, the float put in at Victoria Bridge only descended the river (1½ miles) in five days, and did not in that time reach Govan Ferry, where the second float had been introduced, by three-quarters of a mile. Had the float been put in at Victoria Bridge at low water of neap tides, it would have been carried high up above the city, and would probably not have reached the mouth of the Cart in less than three weeks.

The other experiments which were made by floats introduced at Govan Ferry, Renfrew Ferry, Erskine Ferry, and Dumbarton Ferry, do not afford the means of accurate calculation by reason of one float not always reaching the point at which the next lowest had been started during the period of observation; but from the speed at which they respectively descended the river, there can be no doubt that sewage water entering the Clyde at Glasgow at low water, when the natural volume of the river is small, will be considerably more than a month in reaching Dumbarton.

The importance of ascertaining this fact with greater certainty, and also of ascertaining to what distance the sewage water, which has arrived at Dumbarton at low water of neap tides, will be driven up the river again by the succeeding springs, will be very apparent whether the question of disposing of the sewage of the city be considered in an Engineering or a Chemical point of view. If it has finally to be conveyed by Engineering works to some outlet on the Clyde, this information is necessary for the purpose of determining the proper point for the outlet and the proper period for allowing the sewage to flow into the river. If deodorization can be practically and economically applied, it is important to learn for what length of time it must be effectual in repressing all disagreeable or noxious evaporations.

The great importance of those parts of our enquiry has become gradually more and more evident, and it has been considered desirable to extend these experiments so as to ascertain the rate of progress of the sewage in the river at different states of the tide. These experiments are now nearly completed, and though we shall not attempt to go into their details now, it may be stated that they fully bear out the conclusions drawn from those of last year as to the very slow progress of the sewage in the Clyde.

In concluding, we may be allowed to sum up the results of our enquiries as follows:—

1st. There is no doubt that the process of deodorising, by precipitation, can be effectually carried out by several different processes, but the outlay will be exceedingly large, as

there is no prospect of selling the precipitate at a price which will cover the cost of manufacture.

2nd. That any engineering operations, having for their object to carry the sewage down to the lower part of the river, would be of an expensive and difficult character, because, from the tendency of sewage to remain in the river for a long time, as shown by the experiments with floats, it would be necessary to convey it to a very great distance.

3rd. That the most advisable plan would be to use a deodoriser, which does not precipitate, and which could be thrown into the sewer, so as to sweeten their contents, and admit of their being run into the river without producing a nuisance.

The last plan is that which, should the statements of Dr. Smith and Mr. M'Dougall prove correct, is likely to be carried out at the smallest expense; and it has the advantage of leaving the sewage in a state in which it could be used for irrigation, if that should at any future time be considered desirable. But before pronouncing an opinion either for or against this process, we must await the result of further experiments with the material furnished us by Mr. M'Dougall, meanwhile we must guard ourselves against the supposition that any statements of cost are more than approximations. The greatest difficulty with which we have to deal is the almost entire absence of information derived from extensive experience regarding any of the proposed processes for treating sewages; so that, while the experimental facts may be safely relied on, we think it quite possible that the expenses of the processes may prove in practice higher than we have estimated them.

We have the honour to be,

Gentlemen,

Your very obedt. Servants,

THOMAS ANDERSON.

J. F. BATEMAN.

APPENDIX.

REPORT on Mr. MANNING's process as carried out at his works at Edinburgh, and on Experiments with the Glasgow Sewage, by Mr. JAMES NAPIER.

At the request of the Sanitary Committee, and after consulting with Professor Anderson, and arranging with Mr. Manning as to his convenience, I went to Edinburgh to see and report upon Mr. Manning's process for purifying the sewage waters of the City, and I may here mention, that every facility was given me for inspecting the operations.

Mr. Manning's works are situated at South back of Canongate, at the side of an open sewer or burn, and are composed of two circular depositing tanks, built of bricks, and lined over the bottom with asphalt, and measuring each 40 feet diameter by 6 feet deep, and covered over with boards. The bottom of the tanks have a slight incline to one side, where there is a sluice which goes direct to the bottom of the tank, leading into a third and deeper tank, into which the deposit from the precipitating tanks is allowed to run, and from which it is afterwards raised by a series of buckets, or Jacob's ladder, into a large wooden vessel, where it is drained and pressed to a solid consistency. Another sluice is at the higher side of the precipitating tanks for letting off the clarified water. The outlet at this sluice is ten inches above the bottom of the tanks, by which means the precipitated sludge is retained, while the purified water is being let off. Each of the tanks is furnished with apparatus for stirring, composed of long wooden arms, stretching from a central perpendicular shaft going to the bottom of the tank, which are made to revolve by a steam engine. At the side of the two precipitating tanks, and raised above the surface, is a large tub, in which are mixed the matters used for purifying the waters, and so arranged, that when these matters are well mixed, by pulling out a plug from the bottom of the tub, the precipitants are allowed to run into the water as it is passing into the tanks.

The water of the sewer is let into the tanks by intercepting its passage in the sewer by means of a sluice, when it flows by another channel leading direct to the tanks, in the course of this channel are two deep wells for preventing sand and loose stones being carried into the tanks; by means of small sluices the water is let into or shut off from any of the tanks as required.

The average quantity of water passing into the tanks during my stay, was from 280 to 300 gallons per minute. The average depth of the water let into the tanks, each time measuring from the top of the bridge at the outlet sluice to the top of the sluice, was 3 feet 9 inches, giving 29,219 gallons.

When the tanks were within a few inches of being full, the purifying matters, which were previously mixed in the large tub, were let in, at the same time the stirring apparatus was set in motion, and continued until about 10 minutes after all the purifying mixture was let in and the tank full. In from 45 minutes to 1 hour the precipitate was completely subsided, and the supernatant water let off, which took from 15 to 20 minutes. As each tank required from 95 to 100 minutes to fill, there was ample time for the precipitate to subside and let out the pure water of one tank while the other was filling.

The charge of stuff put into each tank was made up of 6 cwt. of the refuse of Alum makers, called *Alum Sludge*, costing 7½ per ton, 3 cwt. of waste lime, from the manufacture of chloroform, obtained for the carting away, and 2 cwt. of charcoal, a refuse from the manufacture of prussiate of potash, costing from 5½ to 7½ per ton. These quantities of materials served for two fulls of a tank for some time after the tank had been cleaned of its deposit; but after there was a considerable accumulation of deposit, these quantities served for three fulls of the tank, therefore, taking an average of 2½ fulls for each charge of stuff, giving 73,047 gallons for the quantity of precipitants named above.

The composition of these precipitants, in the condition in which they were mixed, I found to be—

ALUM SLUDGE.

Alumina,	7.4
Silica,	5.5
Peroxide of Iron,	18.8
Sulphuric Acid,	29.1
Lime,	2.8
Coaly Matters,	1.2
Chlorine,	0.5
Water of Combination,	8.2
Water at 212°,	26.5
	100.0

Of these there is dissolved out in water 21.5 per cent., being principally Sulphuric Acid and a little Oxide of Iron and Lime.

CHARCOAL.

Carbon,	31.9
Peroxide of Iron,	14.2
Sulphide of Iron,	4.5
Lime,	2.3
Sulphuric Acid,	4.0
	56.9
Chlorine,	0.7
Sand,	10.0
Water at 212°,	31.6
Alkaline Matters' Trace,	42.3
	99.2

LIME.

Carbonate of Lime,	13.6
Sulphate of Lime,	4.7
Hydrate of Lime,	14.5
Chloride of Lime,	4.5
Chloride of Calcium,	5.0
Silica,	0.8
Alumina, with a trace of Oxide of Iron,	1.1
Water at 212°,	55.2
	99.4

The small quantity of soluble matters in these materials is worthy of remark.

In the operations during my stay at the Works, there was no instance when the water let off was either clear or pure, whether from the first, second, or third full of the tank, after the purifying matters were added. The water let off had an ashy or clayey colour, which long standing did not take away, and still retained a perceptible sewage smell. I would, however, remark that the stuffs Mr. Manning uses are capable of making the water clear and free of smell. The sewer being operated upon is a very bad one, and the proportions of purifying matters used were insufficient, but by using one-third more of each ingredient to that given, the water was rendered as clear and limpid as the finest spring, and entirely free of sewage smell, and has retained its purity and appearance after being six weeks kept in a close and warm place; but on adding more sewage to the precipitate, more of the precipitants were required, nearly as much as the first charge.

I brought with me from Edinburgh a quantity of Mr. Manning's precipitants to apply to the Glasgow sewers, and compare the effect and cost of these with other precipitants. I also brought products of Mr. Manning's operations in Edinburgh for analysis. It is but right to state that my experiments in Glasgow were not done in tanks, but in the Laboratory, with the water from different sewers brought to me at different times of the day.

The sewage experimented upon in Glasgow were Pinkstone Burn, St. Enoch's Burn, Gallowgate Burn, and Gorbals. During the experiments

upon these sewers separately, I was struck with the appearance of St. Enoch's Burn—it contained a considerable quantity of a light brownish matter floating in it, which deposited in a few minutes, leaving the water clear, but having a light straw tint, and *without smell*. Thinking that this may have been the result of some accidental circumstance, I procured quantities upon two other separate days, and different hours of the day, and found it to be all of the same character. Both the deposit and water were then subjected to analysis. The solid matters which precipitated by standing amounted to 60 grains per gallon, and were composed of—

Carbonaceous Matters,	18.6
Silica or Sand,	27.3
Alumina,	21.3
Peroxide of Iron,	4.8
Oxide of Manganese,	2.3
Lime,	2.0
Sulphuric Acid,	2.8
Chlorine,	0.6
...	19.7

The loss by burning at a red heat were taken as Carbonaceous Matters.

The clear water in the three different days differed in the quantity of salts they contained in solution, but not in the kind. One of them being taken immediately after a heavy fall of rain had only half of the other two. The following I give as the mean of the two strongest. Salts held in solution, per gallon of water, 144 grains, dried at 212°.

Chloride of Manganese,	47.8
Perochloride of Iron,	47.4
Chloride of Calcium,	16.5
Sulphate of Lime,	12.4
Silica,	2.8
Carbon,	1.2
Alkaline Chlorides,	15.9
...	144.0

These analyses showed at once the cause of the deodorising of the sewer, and verified what Mr. James Young had shown several years ago, that Protochloride of Manganese was a powerful deodoriser. The Perochloride of Iron has also the same property.

It was evident from these results that if the St. Enoch Sewer was deodorised by the Iron and Manganese Salts being let into them, the same effects would be produced in the other sewers by the same cause. I therefore mixed the Gorbals and Gallowgate Sewers with an equal quantity of St. Enoch Sewer, and after stirring well, the sewage smell was removed, but not so perfectly as in the St. Enoch Sewer alone, probably from the sewage having been kept some time before the mixing, which would not be the case in

the St. Enoch's Sewers, for the odour is much more easily prevented in new matters than it is after decomposition has fairly begun. I also found that by adding two parts of St. Enoch's to one part of the Pinkstone, the whole was both clarified and deodorised.

As there was not a sufficient quantity of St. Enoch's burn to deodorise all the sewage of the City, I then thought by what means a greater supply of these salts that affected the deodorising could be obtained, and thinking it probable that they issued as waste from St. Rollox Works. I with the approval of Professor Anderson went to these works, and found that the materials referred to in St. Enoch's burn evidently proceeded from these works, and I also found that large quantities of hydrated peroxide of iron, with a small quantity of manganese, were constantly accumulating there, which when dissolved in hydrochloric acid, formed the requisite salts for deodorising, and that quantities of these salts were constantly let off from these works.

Laying aside the consideration of this view of the question for a little, I will now give the results of the experiments for purifying the several sewers with lime, and with Mr. Manning's materials, and also with these in connection with the St. Enoch's Burn, as it is presently constituted, taking in the first instance the relative cost of purifying 1,000,000 gallons of sewage by Mr. Manning's materials,—

	ALUM SLUDGE.	LIME.	CHARCOAL.
Gallowgate,	19,500 lbs.	12,600 lbs.	6,700 lbs.
Gorbals,	16,600	11,400	5,400
Pinkstone,	40,600	50,000	26,000
...	3 08,700	3 74,000	3 38,700
...	32,900	24,666	12,900

thus a mixture of these sewers require for making the water clear and free of smell for 1,000,000 gallons.

Alum Sludge, 295½ cwt., @ 4½d.	£5 10 1½
Charcoal, 115 cwt., say @ 3½d.	1 13 6½
220 cwt. Lime @	0 0 0
...	£7 3 8

Nothing is here allowed for cartage, which would vary according to distance. Lime shell cost, delivered in Glasgow, 12½ per ton. In slaking I found an average of 10 per cent. of unburned lime; one ton of Lime shell produced, after deducting this 10 per cent., 25½ cwt. of slaked lime in the state in which it was added to the tanks, making it 6d. per cwt. A mixture of the three sewers required (30 cwt. @ 6d., £1 5 0) 50 cwt. of the slaked lime for 1,000,000 gallons.

The next question was, what effect a mixture of a small quantity of St. Enoch's sewer with these others have along with these precipitants? Would the precipitation of the salts in St. Enoch's aid the precipitation of the impurities in the other sewers? The following are the results obtained, by mixing equal parts of the four sewers, namely, Pinkstone, St. Enoch's, Gallowgate and Gorbals, calculating as above upon 1,000,000 gallons with

Mr. Manning's materials,	£3 15 0
With Lime	0 7 6

Taking the quantities which were applied to the Edinburgh sewers as the basis for calculation, without reference to the water being perfectly clarified, for in my experiments I never found these proportions making clear water, even with the Glasgow sewers, namely, 6 cwt. alum sludge, 3 cwt. lime, and 2 cwt. charcoal for 73,047 gallons, 1,000,000 gallons will require—

82 cwt. Alum Sludge @ 4½d.	£1 10 9
41 " Lime,	0 0 0
27½ " Charcoal, @ 3½d.	0 8 0
	1 18 9

The whole sewage of the City has been calculated at 50,000,000 gallons daily. The cost per year for this quantity of sewage will therefore be,

BY MR. MANNING,

Without St. Enoch's,	£131,095 16 8
With one-fourth St. Enoch's,	68,437 10 0
Edinburgh data,	46,146 0 8

WITH LIME.

Without St. Enoch's,	£22,812 10 0
With St. Enoch's,	6,843 15 0

I may here mention in connection with the matters in St. Enoch's, that the refuse peroxide of iron left when making sulphuric acid from iron pyrites, often contains small portions of soluble per sulphate of iron which, when put into the sewers along with lime, has a similar effect in facilitating the precipitation of the organic matters of the sewers, to the salts in St. Enoch's burn.

I may also mention that, in operating with Mr. Manning's precipitants, I found that the lime which he uses, if used in about double the quantity recommended by Mr. Manning, will clarify the water without the addition of the other matters.

As to the purity of the water after being acted upon by these several precipitants, it may be stated, that in no case is the water in a state of perfect purity, having always in it less or more soluble salts. With the lime alone, the clarified water is occasionally of a straw tint, especially that of the Pinkstone burn, but when the lime is mixed with the St. Enoch's, and also when purified by Mr. Manning's precipitants, there is no colour. The water in general is as pure to the eye as ordinary river water, and in many

cases could be drunk without any very disagreeable taste being experienced, and during these experiments, I supplied a fish globe with the water as decanted from the precipitants; changing the water daily as there was a paucity of air in the water, and the fish lived as well and healthy like as when in the water which is supplied to the City. The only smell observed, and that only occasionally and very faint, was the odour of brewers' wash. The matters held in solution after clarification were exceedingly varied in quantity, being sometimes the double of other times, within a few hours. The following deduced from several analyses will give an idea of the quality and quantity of these purified sewers.—The solid matters after evaporation were dried at 212° and then burned—the loss by burning is called carbonaceous matters. The contents of one gallon evaporated may be stated at 102 grains composed of—

Carbonaceous matters burned off,	29.5
Silica,	1.4
Alumina,	1.6
Lime,	26.7
Carbonic Acid,	10.3
Sulphuric Acid,	7.5
Chlorine,	11.8
Magnesia,	1.7
Phosphoric Acid,	trace
Potash,	3.8
Soda,	2.2
	102.0

These matters were in combination forming salts. This water put into the river, would not be detrimental to any process of manufacture for which the water of the river was used.

The next thing is the quantity of solid matters deposited by the sewers *with* and *without* precipitants. In all the sewers there is a quantity of solid matter floating in them that deposits by a few minutes repose. This deposit, in ordinary sewers, has a striking similarity of character, having about 42 per cent. of organic matters, and 58 per cent. of earthy matters, composed of silica, alumina, oxide of iron and lime. The average quantity of these solid matters in the four different sewers under examination, taken at different hours of the day, and in different states of the sewers, were 50 grains per gallon, dried at 212° Fah. The different sewers gave—

Gallowgate, - - - - -	23 grains.
Gorbals, - - - - -	27 "
St. Enoch's, - - - - -	70 "
Pinkstone, - - - - -	80 "
	200

This solid matter, in a million gallons of the mixed sewers, will be 7,140 lbs., being per year 58,172 tons of *dry* matter. By experiments I found that this solid matter, in a wet state, but sufficiently consistent to be cut into

cubes and retain its shape, contains 70 per cent. of water; and that in a state from which the water is merely drained off, as it would be retained upon a punt when dredging, it retains 86 per cent. water, hence, there will be in this state, as dredged, 415,525 tons per year, but in the more solid form, suitable for carting, 193,906 tons. All this, however, may not require to be dredged from the river, as portions may be carried down by the current, and also part may be decomposed and dissolved, giving off gaseous emanations, but in either way they deteriorate the Clyde; and this altogether apart from the soluble sewage matter's undergoing constant decomposition.

In my experiments with the different precipitants, they were applied, without previously separating the solid matters floating in the sewage, and consequently, the weights obtained and referred to, include this solid matter. By Mr. Manning's matters, the quantity of precipitate deposited is fully five times more than the quantity of solid matter given above; and that by lime alone is two and one-half times the quantity given above, while that with lime, and St. Enoch's sewers does not exceed two times that given above. These will therefore stand as follows, taking it with 70 per cent. of water, which is the condition fit for cartage:—

By Mr. Manning's process, - - -	959,539 tons.
By Lime alone, - - - - -	484,765 "
By Lime and St. Enoch's, - - -	387,812 "

The great excess by Mr. Manning's process is doubtless owing to the great quantity of insoluble matters in his precipitants. Should these deposits have a manurial value and a market obtained for them, Mr. Manning's materials yield by far the largest product.

Should it be advisable to use precipitants, and setting the manurial value of the deposit aside, and providing for its removal from the tanks or place of deposit, to prevent undue accumulation, I would suggest that the operations be performed near the river; say that the whole sewage of the north side of the river was conducted by a main sewer to the side of Kelvin above the Clyde, the deposit could be lifted direct from the tanks by a series of buckets into punts, and conveyed either up or down the river to places of deposit, to fill up ground on the banks of the river, as is done with the dredging matter of the river at present, and thus much of the present dredging of the river saved, and the water kept clear and free of nuisance. A similar arrangement could be made for the south side of the River. Or were the liquid refuse let out from St. Rollox Works, which could be greatly increased in quantity and value by dissolving the accumulating peroxide of iron, referred to in a former part of this Report, supplied at the fountain-head to Pinkstone, St. Enoch's, and Molendinar Burns, the principal portion of the city would thus be deodorised; and if led into tanks, as suggested, all the sewers from other sources will thus be mixed and deodorised without any precipitants, and the solid matters prevented going into the river; and although it will not be valuable as a manure, it will be excellent for filling up.

Another question suggested itself during these operations, which is clearly connected with the composition of the sewers and the interest of the river—namely, the effect these sewage waters will have upon the sheathing of vessels lying in the river.

These sewers, not deodorised, all contain sulphur, or matters capable of decomposing and giving off sulphur, which has an extraordinary affinity for copper. The persalts of iron, such as that existing in St. Enoch's Burn, are also powerful solvents of copper or yellow metal, even more powerful than the same quantity of sulphuric or muriatic acids would have were they not combined with the peroxide of iron.

I submitted pieces of copper and yellow metal sheathing to the action of these sewage waters separately, and the results are as follows, giving the number of grains dissolved, from a square foot of these metals kept in the sewage waters for 24 hours without any agitation, taking the mean of a good many trials, for sometimes the action was much more rapid than at others, owing to the state of the sewer:—

	COPPER.	YELLOW METAL.
Pinkstone Burn, - - -	10·2 grains.	9·0 grains.
St. Enoch's Burn, - - -	15·2 "	20·3 "
Gallowgate, - - - - -	2·9 "	1·3 "
Gorbals, - - - - -	5·0 "	4·4 "
Giving a mean of - - -	7·9 "	8·8 "

It must be borne in mind, however, that the action will be much less after the sewers become dilute by the waters of the river; at the same time, I may remark, that the action of these matters upon sheathing is seldom equal over the whole surface of the vessel, but generally in patches; so that, were the action of the river equal to only two grains per square foot in 24 hours over the whole surface, by this irregular action, caused by galvanic influence, small portions of the surface may be acted upon at a much higher rate than given above, while other portions will not be acted upon at all, being protected at the expense of the few feet acted upon.

The water, after being purified by any of the precipitants, had no perceptible action upon either copper or yellow metal after being kept in it for several days.

JAMES NAPIER.

ON THE CONNEXION
BETWEEN THE
HEAT OF THE BODY
AND THE
EXCRETED AMOUNTS OF UREA, CHLORIDE OF
SODIUM, AND URINARY WATER,
DURING
A FIT OF AGUE.

BY
SYDNEY RINGER, M.R.C.S.,
LATE PHYSICIAN'S ASSISTANT IN UNIVERSITY COLLEGE HOSPITAL.

COMMUNICATED BY
RICHARD QUAIN, F.R.S.

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Received June 6th.—Read June 28th, 1859.

The observations on the temperature of the body during ague fits are now numerous and accordant,² and may be considered sufficient to indicate the general condition of the animal heat during the several stages of ague.

¹ The following observations were made at the suggestion of Dr. Parkes, from whom, from time to time, I have received assistance. Dr. Parkes has also supplied the entire literature in the paper, and has kindly examined it with care.

² Zimmermann; Bärensprung, Müller's 'Archiv,' 1852, p. 217; Michael, 'Archiv für Phys. Heilk.,' 1856, p. 39; Wunderlich, 'Archiv für Phys. Heilk.,' 1858, p. 12.

The increase in the amount of urea and of chloride of sodium during the cold and hot stages has been affirmed by Traube and Jochmann,¹ Moos,² Redenbacher,³ and Hammond.⁴ These observations have been criticised, however, and certainly some of the cases were not examined with any great minuteness. In no case yet reported either has any comparison been drawn between the rise in the temperature and the amount of the urea and chloride of sodium.⁵ It therefore seemed extremely desirable, on the occasion of a patient with ague being admitted into University College Hospital, under the care of Dr. Parkes, to examine the subject, and to see, first of all, whether the increase of urea and of chloride of sodium really occur in the cold and hot stages of ague, as affirmed by Traube, Redenbacher, and Hammond; and, secondly, whether any close connexion could be traced between the amount of urea and the abnormal temperature. My position as one of the resident officers in the hospital at the time gave me the opportunity of carrying out the inquiry with all the minuteness necessary for accuracy.

The general results may be thus stated—that not only was the increase of urea and of chloride of sodium constant during the cold and hot stages of ague, but that their amount was in very close relation to the temperature. The first case recorded in this paper will, I believe, give little short of mathematical proof of the connexion between these two phenomena, viz., the increase of heat of body and of the excretion of urea and chloride of sodium.

The second case communicated is one in which the phenomena were less carefully observed, but it is valuable as affording another instance of the increase of urea and chloride of sodium during the fit.

¹ 'Deutsche Klinik,' No. 46, Nov., 1855.

² Henle's 'Zeitschrift für rat. Med.,' Band vii, p. 291.

³ Henle's 'Zeitschrift,' Band ii (Dritte Reihe), p. 384.

⁴ 'American Journal of Med. Science,' April, 1858.

⁵ Some other observations have been made on the excretions of twenty-four hours, but these are of little value as febrile and non-febrile excretions are put together.

In addition, a case of hectic fever occurring in phthisis has been narrated, for in this case the phenomena were found to be identical with those presented by malarial ague. The result was the same in both cases, although the causes were so different.

CASE 1. *Quotidian ague.*—The patient, a man, was first attacked by tertian in August last, whilst working at Maldon, in Essex. The ague continued on him for three weeks, by which time he was apparently cured; he remained working at the same place till the December following. At that time, moving up to London, he broke his leg, and was carried to Charing Cross Hospital. Upon being discharged from that institution, cured, he caught cold, and an attack of ague immediately followed, and has continued more or less since, being immediately brought on by exposure to cold. This long continuance produced the usual effects of prolonged ague; and in this state he was admitted into the hospital, the disease having changed a day or two previously from the tertian to the quotidian type.

He suffers from slight aortic obstructive disease. His pulse is continually about 120 per minute,¹ exceedingly irregular in both force and rhythm. His arteries are very tortuous and visible. He never suffered from rheumatism, and has no arcus senilis. Except the above heart-disease, he suffers from no organic lesion.

He is fifty-nine years of age, but looks much older. His weight is 144 pounds; his height five feet nine and three quarter inches. His vital capacity (Hutchinson's spirometer) is 140 cubic inches.

The examination was conducted in the following manner. The patient was put to bed, and his urine collected through the night. At 5 a.m. he had breakfast, consisting of two eggs, bread and butter, and tea, the latter being measured. At 6 a.m. he was made to pass his urine, and all passed at

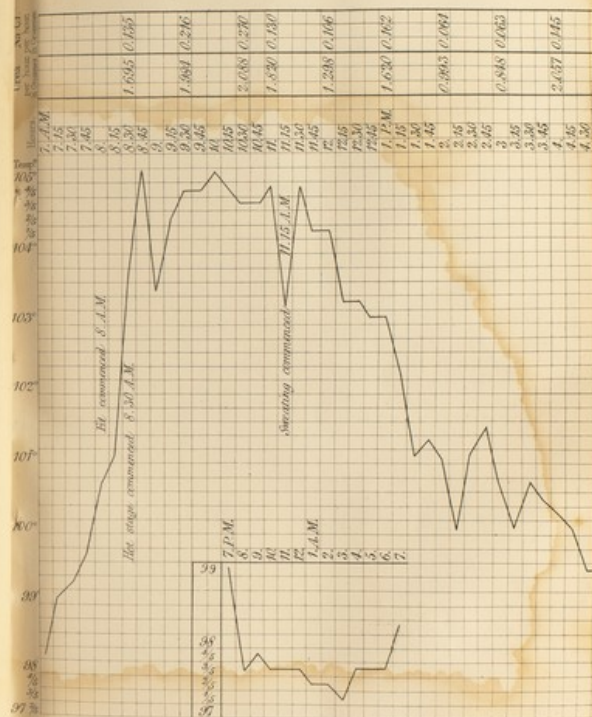
¹ No tables are therefore given of the pulse.

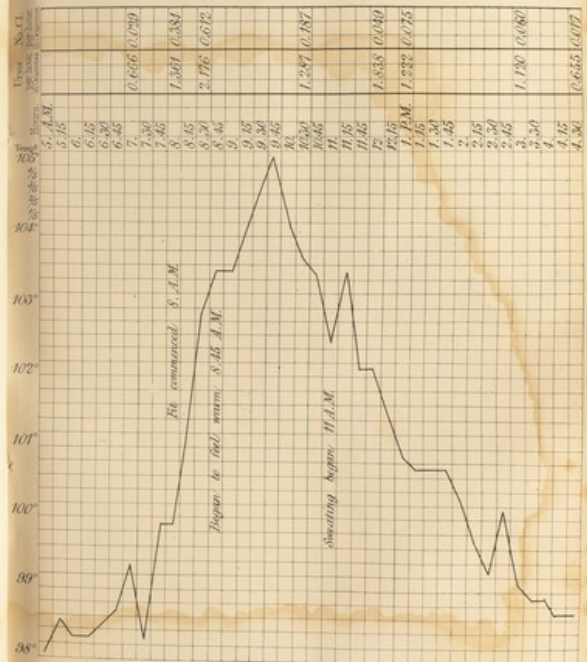
the time was mixed with that passed during the night. The thermometer (a good one, by Negretti and Zambra, and divided into fifths of a Fahrenheit degree) was next placed in his axilla, and kept there during the remainder of the day, the temperature being noted down every quarter of an hour. He was also made to pass his urine hourly, or upon the commencement of another stage, before the completion of the hour, and the next hour was dated from that time; by this means we not only obtained the urine of every hour, but also of each stage, separately. The patient was allowed no food until the completion of the fit, when he had a good meat dinner and six ounces of wine. Lemonade he was permitted to drink *ad libitum*, but the quantity was always measured and noted, with the time at which it was taken. He was weighed immediately after his breakfast, and again at the termination of the fit, and during this time no motions were allowed to be passed. The patient was purposely kept without medicine. In determining the amount of urea and chloride of sodium, Liebig's volumetric method with nitrate of mercury was used.

It was not possible to determine the other urinary constituents in this case. The chloride of sodium was not got rid of before testing for urea, but the usual correction was made.

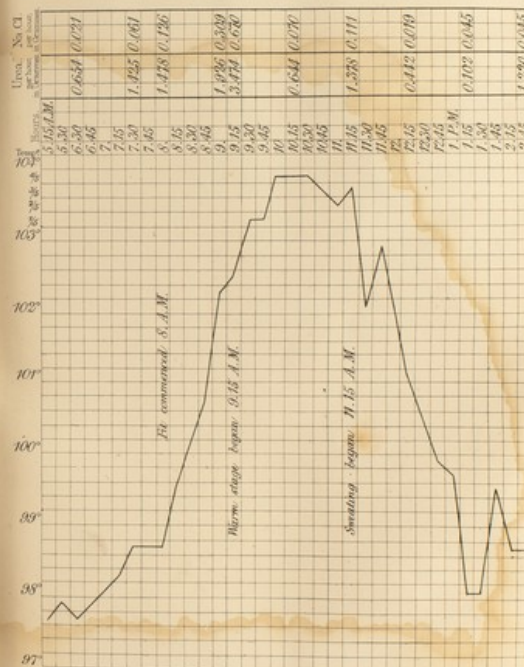
The following charts show the variations in the temperature in fifths of a degree, as taken every quarter of an hour. Above the table are two columns, one showing the amount of urea, the other the amount of chloride of sodium, poured out. The quantity per hour is stated in each case, and put down in the column denoting the time at which it was passed. It indicates, of course, the quantity formed during the previous hour. The amounts of urea and chloride of sodium are given always in French grammes, and the water in cubic centimetres. The commencement of each stage, as judged of in the usual way, by the sensations of the patient, is also noted in the column proper to the time at which it began.

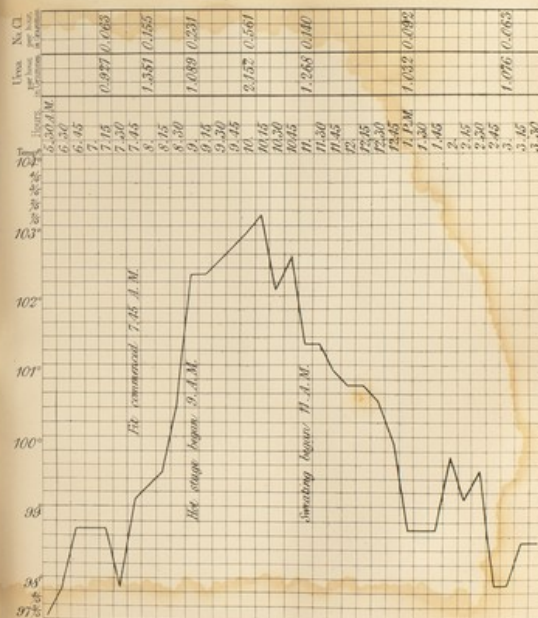
For the purpose of greater clearness and of giving fuller

APRIL 6TH

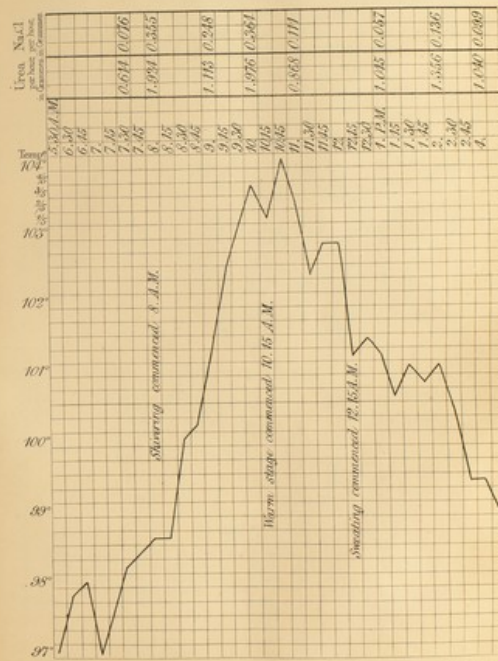
APRIL 7TH

APRIL 8TH



APRIL 9th

APRIL 11TH



details, tables are added, recapitulating the amount of the urea, chloride of sodium, and water, separately. In the first column of each table the stages are written down; in the next the hour at which the urine was passed; then follows the total amount of urea contained in the urine passed during the specified time; the next column shows the amount reduced to hours; and, lastly, the average amount of each stage per hour is given.

TABLE.

APRIL 6th.—UREA.

Stage.	Hour.	Total Quantity in this time.	Quantity per hour.	Average Quantity per hour during period.
		Grammes.	Grammes.	Grammes.
Cold	7-45 to 8-30 a.m.	1-356	1-695	1-695
Hot	8-30 to 9-30 a.m.	1-984	1-984	1-964
	9-30 to 10-30 a.m.	2-088	2-088	
	10-30 to 11 a.m.	0-919	1-830	
Sweating	11 to 12 a.m.	1-289	1-289	1-363
	12 to 1 p.m.	1-620	1-620	
	1 to 2 p.m.	0-993	0-993	
	2 to 3 p.m.	0-848	0-848	
	3 to 4 p.m.	2-057	2-057	

CHLORIDE OF SODIUM.

Cold	7-45 to 8-30 a.m.	0-108	0-135	0-135
Hot	8-30 to 9-30 a.m.	0-216	0-216	0-220
	9-30 to 10-30 a.m.	0-270	0-270	
	10-30 to 11 a.m.	0-065	0-130	
Sweating	11 to 12 a.m.	0-106	0-106	0-108
	12 to 1 p.m.	0-162	0-162	
	1 to 2 p.m.	0-064	0-064	
	2 to 3 p.m.	0-063	0-063	
	3 to 4 p.m.	0-145	0-145	

WATER.

Cold	7-45 to 8-30 a.m.	75 c.c.		
	8-30 to 9-30 a.m.	80 c.c.		
Hot	9-30 to 10-30 a.m.	90 c.c.		
	10-30 to 11 a.m.	82 c.c.		
Sweating	11 to 12 a.m.	59 c.c.		
	12 to 1 p.m.	90 c.c.		
	1 to 2 p.m.	54 c.c.		
	2 to 3 p.m.	55 c.c.		
	3 to 4 p.m.	121 c.c.		

APRIL 7th.—UREA.

Stage.	Hour.	Total Quantity in this time.	Quantity per hour.	Average Quantity per hour during period.
		Grammes.	Grammes.	Grammes.
During night...	3 p.m. to 5:30 a.m.	10.272	0.684	0.684
Period immediately before shivering.....	5:30 to 7 a.m.	0.999	0.666	0.666
Cold	7 to 8 a.m.	1.361	1.361	1.361
Hot	8 to 8:30 a.m.	1.088	2.176	2.176
Sweating	8:30 to 10:30 a.m.	2.375	1.287	1.287
	11 to 12 a.m.	0.838	0.838	
	12 to 1 p.m.	1.222	1.222	
	1 to 3 p.m.	2.240	1.120	0.955
	3 to 5 p.m.	1.311	0.655	

CHLORIDE OF SODIUM.

During night...	3 p.m. to 5:30 a.m.	0.642	0.029	0.029
Period before shivering.....	5:30 to 7 a.m.	0.440	0.29	0.29
Cold	7 to 8 a.m.	0.384	0.384	0.384
Hot	8 to 8:30 a.m.	0.306	0.612	0.612
Sweating	8:30 to 10:30 a.m.	0.375	0.187	0.187
	11 to 12 a.m.	0.049	0.049	
	12 to 1 p.m.	0.075	0.075	
	1 to 3 p.m.	0.120	0.060	0.064
	3 to 5 p.m.	0.034	0.017	

QUANTITY OF URINE PASSED.

Before shivering.....	3 p.m. to 5:30 a.m.	36 c.c.		
Cold	5:30 to 7 a.m.	25 c.c.		
Hot	7 to 8 a.m.	80 c.c.		
Sweating	8 to 8:30 a.m.	136 c.c.		
	8:30 to 10:30 a.m.	62 c.c.		
	11 to 12 a.m.	33 c.c.		
	12 to 1 p.m.	50 c.c.		
	1 to 3 p.m.	50 c.c.		
	3 to 5 p.m.	28 c.c.		

APRIL 8th.—UREA.

Period immediately before shivering.....	5 p.m. to 6:30 a.m.	8.832	0.654	0.654
Cold	6:30 to 7:30 a.m.	1.425	1.425	1.452
Hot	7:30 to 8 a.m.	0.738	1.478	
Sweating	8 to 9 a.m.	1.926	1.926	2.700
	9 to 9:15 a.m.	0.868	3.474	
	9:15 to 10:15 a.m.	0.644	0.644	1.011
	10:15 to 11:15 a.m.	1.378	1.378	
	11:15 to 12:15 p.m.	0.944	0.944	
	12:15 to 1:15 p.m.	0.102	0.102	0.498
	1:15 to 2:45 p.m.	1.830	1.220	

CHLORIDE OF SODIUM.

Stage.	Hour.	Total Quantity in this time.	Quantity per hour.	Average Quantity per hour during period.
		Grammes.	Grammes.	Grammes.
Night	5 p.m. to 6:30 a.m.	0.376	0.021	0.021
Before fit	6:30 to 7:30 a.m.	0.064	0.064	0.095
Cold	7:30 to 8 a.m.	0.063	0.162	
Hot	8 to 9 a.m.	0.309	0.309	0.381
	9 to 9:15 a.m.	0.167	0.670	
	9:15 to 10:15 a.m.	0.070	0.070	0.090
	10:15 to 11:15 a.m.	0.111	0.111	
	11:15 to 12:15 p.m.	0.019	0.019	
	12:15 to 1:15 p.m.	0.045	0.045	0.027
	1:15 to 2:45 p.m.	0.045	0.030	

QUANTITY OF URINE PASSED.

Night	5 p.m. to 6:30 a.m.	42 c.c.		
Before fit	6:30 to 7:30 a.m.	54 c.c.		
Cold	7:30 to 8 a.m.	60 c.c.		
Hot	8 to 9 a.m.	86 c.c.		
	9 to 9:15 a.m.	172 c.c.		
	9:15 to 10:15 a.m.	26 c.c.		
	10:15 to 11:15 a.m.	32 c.c.		
	11:15 to 12:15 p.m.	13 c.c.		
	12:15 to 1:15 p.m.	38 c.c.		
	1:15 to 2:45 p.m.	50 c.c.		

APRIL 9th.—UREA.

Immediately before shivering.....	6:15 to 7:15 a.m.	0.927	0.927	0.927
Cold	7:15 to 8 a.m.	1.081	1.351	1.351
Hot	8 to 9 a.m.	1.089	1.089	1.089
Sweating	9 to 10 a.m.	2.152	2.152	1.710
	10 to 11 a.m.	1.268	1.268	
	11 to 1 p.m.	2.164	1.032	1.129
	1 to 3 p.m.	2.353	1.076	

CHLORIDE OF SODIUM.

Before fit	6:15 to 7:15 a.m.	0.063	0.063	0.063
Cold	7:15 to 8 a.m.	0.124	0.155	0.155
Hot	8 to 9 a.m.	0.231	0.231	0.231
Sweating	9 to 10 a.m.	0.561	0.561	0.351
	10 to 11 a.m.	0.140	0.140	
	11 to 1 p.m.	0.184	0.092	
	1 to 3 p.m.	0.127	0.063	0.078

QUANTITY OF URINE PASSED.

Before fit	6:15 to 7:15 a.m.	35 c.c.		
Cold	7:15 to 8 a.m.	61 c.c.		
	8 to 9 a.m.	53 c.c.		
	9 to 10 a.m.	117 c.c.		
	10 to 11 a.m.	32 c.c.		
	11 to 1 p.m.	44 c.c.		
	1 to 3 p.m.	53 c.c.		

APRIL 10TH.—UREA.

Stage.	Hour.	Total Quantity in this time.		Quantity per hour.	Average Quantity per hour during period.
		Grammes.	Grammes.		
Before fit	6:30 to 8:15 a.m.	1.420	0.885	0.885	
Cold	8:15 to 9:15 a.m.	0.358	0.358	0.358	
Hot	9:15 to 10:15 a.m.	1.144	1.144	1.144	1.013
Sweating	10:15 to 11:15 a.m.	0.883	0.883	0.883	
	11:15 to 2:15 p.m.	0.332	0.177	0.177	

CHLORIDE OF SODIUM.

Stage.	Hour.	Total Quantity in this time.		Quantity per hour.	Average Quantity per hour during period.
		Grammes.	Grammes.		
Before fit	6:30 to 8:15 a.m.	0.140	0.087	0.087	
Cold	8:15 to 9:15 a.m.	Urine lost.	0.176	0.176	
Hot	9:15 to 10:15 a.m.	0.176	0.115	0.115	0.145
Sweating	10:15 to 11:15 a.m.	0.115	0.115	0.115	
	11:15 to 2:15 p.m.	0.070	0.032	0.032	

QUANTITY OF URINE PASSED.

Stage.	Hour.	Quantity in c.c.
Before fit	6:30 to 8:15 a.m.	50 c.c.
Cold	8:15 to 9:15 a.m.	20 c.c.
Hot	9:15 to 10:15 a.m.	44 c.c.
	10:15 to 11:15 a.m.	32 c.c.
	11:15 to 2:15 p.m.	22 c.c.

APRIL 11TH.—UREA.

Stage.	Hour.	Total Quantity in this time.		Quantity per hour.	Average Quantity per hour during period.
		Grammes.	Grammes.		
During night	2:15 p.m. to 6 a.m.	9.785	0.609	0.609	
	6 to 7:30 a.m.	0.921	0.614	0.614	
Immediately before fit	7:30 to 8 a.m.	0.962	1.924	1.924	
Cold	8 to 9 a.m.	1.113	1.113	1.113	
	9 to 10 a.m.	1.839	1.839	1.839	1.476
Hot	10 to 11 a.m.	0.868	0.868	0.868	
Sweating	11 to 1 p.m.	2.090	1.045	1.045	
	1 to 2 p.m.	1.356	1.356	1.356	1.147
	2 to 4 p.m.	2.090	1.040	1.040	

CHLORIDE OF SODIUM.

Stage.	Hour.	Total Quantity in this time.		Quantity per hour.	Average Quantity per hour during period.
		Grammes.	Grammes.		
Before fit	2:15 p.m. to 6 a.m.	0.139	0.087	0.087	
	6 to 7:30 a.m.	0.115	0.076	0.076	
Cold	7:30 to 8 a.m.	0.355	0.355	0.355	
	8 to 9 a.m.	0.248	0.248	0.248	0.306
Hot	9 to 10 a.m.	0.364	0.364	0.364	
	10 to 11 a.m.	0.111	0.111	0.111	
Sweating	11 to 1 p.m.	0.114	0.057	0.057	
	1 to 2 p.m.	0.136	0.136	0.136	0.097
	2 to 4 p.m.	0.119	0.099	0.099	

QUANTITY OF URINE PASSED.

Stage.	Hour.	Total Quantity in this time.
Night	2:15 p.m. to 6 a.m.	...
Before fit	6 to 7:30 a.m.	24 c.c.
Cold	7:30 to 8 a.m.	74 c.c.
	8 to 9 a.m.	64 c.c.
	9 to 10 a.m.	76 c.c.
Hot	10 to 11 a.m.	31 c.c.
	11 to 1 p.m.	38 c.c.
	1 to 2 p.m.	57 c.c.
	2 to 4 p.m.	95 c.c.

CONCLUSIONS FROM THE PREVIOUS FACTS.

I. Temperature.

The temperature during the several fits, amid much general resemblance, presented numerous partial differences, all of which are, however, reducible to order.

(a) In every case, the temperature commenced to rise previous to the cold stage,¹ as experienced by the patient, that is to say, before any feeling of cold or illness of any kind. The time that it commenced, previous to the cold stage, varied from an hour and a half to three quarters of an hour, and no connexion between the time and the severity of the fit could be traced. The temperature commenced to rise—

April 6th. 1½ hour before any feeling of cold or illness.

" 7th. 1 " " "

" 8th. 1½ " " "

" 9th. 1½ " " "

" 10th. 1½ " " "

" 11th. ½ " " "

A close connexion between the severity of the fit and the character of the rise before the cold stage existed, the severity being in proportion to the continuousness of the rise, and also to the extent of each rise. As the fit became less severe, the temperature had a tendency either to oscillate, rise slowly, or remain stationary, and these tendencies

¹ The same fact is noted by Michael, *op. cit.*, p. 43.

increased as the fit decreased in severity, and eventually all combined.

(b) During the cold stage the temperature rose throughout, the rise being greater than during any other period; but here also great variations existed, having their counterpart in variations in the severity of the fit.

Thus, in the cold stage, the temperature rises continuously when the fit is severe; then, as the severity lessens, there is a tendency for the rise at the termination of the cold stage to flag, and eventually to become stationary; then the range of each rise becomes less; and lastly, it oscillates. It also appears that the alteration first affects the commencement and termination of the cold stage, an oscillation in its middle indicating a greater diminution of the severity of the fit than at either end.

There was no connexion between the duration of the stage and the severity of the fit, as measured by the temperature. Thus, it lasted—

On the 6th	$\frac{3}{4}$ hour.
" 7th.....	$\frac{1}{2}$ "
" 8th.....	$1\frac{1}{2}$ "
" 9th.....	$\frac{3}{4}$ "
" 10th.....	1 "
" 11th.....	2 hours.

The fits were most severe on the 6th and 7th, and declined in severity till the 11th, when the fit was again more severe.

(c) During the hot stage the temperature continued to rise, and in its early part closely corresponded to the former periods.

From a careful examination of the charts, it appears that in the severer cases the temperature in the hot stage ran up at once to its acme, and had a tendency to remain permanent; the permanency not, however, being obtained at once nor retained throughout, the temperature oscillating both at the commencement and termination. In less severe cases it ran up slowly, and did not remain stationary, and when least severe, oscillated in its rise.

The temperature in those fits becoming less severe first lost its permanency, and next the temperature rose to a less extent

and again became permanent; then, when they became still less severe, it rose to the same extent, but again lost its permanency. Again, in those cases where the temperature remained permanent, the hot stage ended at the termination of the permanent period in an oscillation, these cases being the severest at their own temperature. In all other cases the temperature fell before the sweating stage commenced, ending in an oscillation, and the less severe the fit the greater was the portion of the hot stage occupied by the fall of the temperature. Also the fit was less severe, and the fall greater, when the temperature in falling every now and then stood still for some time, and was still less severe when it oscillated.

(d) The temperature continued to fall through the sweating stage, and was often a long time before it reached the point from which it started. The temperature fell most rapidly in those cases in which the oscillations occurred, and least rapidly where the fall was continuous. Towards the termination of the sweating stage a rise occurred to a slight extent (perhaps followed by oscillations) in four of the six fits. Thus, on April 6th, sweating commenced at 11.15, when the thermometer marked $103\frac{1}{2}^{\circ}$; at 2.15 the temperature had fallen to 100° ; it then rose to 101° at 2.30, to $101\frac{1}{2}^{\circ}$ at 2.45; then fell to $100\frac{3}{4}^{\circ}$ and 100° at 3 and 3.15; then rose to $100\frac{1}{2}^{\circ}$ at 3.30, and then finally fell regularly to $97\frac{1}{2}^{\circ}$, a point not reached till seven o'clock. The charts show, at a glance, these slight but perfectly definite rises at this late period of the sweating stage.

II. Urea.

As the type of the disease in this man was quotidian, there was no opportunity of comparing the amount of urea on a fever and on a fever-free day. But the amount of urea passed in the fever-free hours was decidedly much smaller than might have been anticipated. He excreted only 0.650 grm. on an average per hour in the apyretic period, which would give in twenty-four hours 15.600 grms., had the excretion remained at the same amount during

the whole day. Now, a man of the same weight, between twenty and forty years of age, on a good diet, as this man was, would have secreted 32 grms. in the twenty-four hours, if he passed the average amount. Our patient was, however, older (fifty-nine), and would, no doubt, form less urea than a man at a more vigorous period of life. But it can hardly be supposed that the amount would be reduced so low as 15½ grms. in the twenty-four hours by this difference of age. It may, therefore, be concluded that, in accordance with Redenbacher's observations, the excretion of urea in the fever-free period was below the healthy amount.

The observations made by Traube, Redenbacher, and Hammond on the increase of urea during the cold and hot stages are entirely confirmed by this case, but a more minute statement of the kind and amount of the increase can now be given.

It must, however, be premised, that the only obvious causes of the increase of the urea in this case during the fit are, either the food taken at breakfast at 5 a.m., the fluid drunk during the fit, or the fit itself. No other known causes existed which could have had the effect of increasing the urea. The following objections to the idea of the food being the cause may be urged. That the amount of food was not great, and that the increase in the urea was far larger than has yet been noted after even the heaviest meal. For example, on the 7th, the amount per hour rose from 0.684 grm. to 2.176, being an increase of more than 200 per cent.; on the 8th, from 0.654 to 3.474, being an increase of nearly 500 per cent.; on the 9th and 10th, when the fits were slighter, the increase was less marked, though still considerable; while on the 11th, when the fit was severe, it was again 200 per cent. Such an amount is greater than has yet been found after an ordinary amount of food.¹ The time, however, at which the urea increases after food accords with our case; for, augmenting even

¹ Dr. Parkes has noted in one person without fever a rise from 0.665 grm. in a fasting hour to 1.554 grm. in a food hour, but this was after a hearty dinner.

during the first hour after food,¹ it attains its maximum sometimes in the third hour;² sometimes, however, not till the seventh hour; usually, however, it reaches its maximum at the fourth hour.

Now, in this case food was taken between 5 and 6 a.m., and the maximum amount of urea secreted was during the cold stage, from 8 to 9.30 a.m., or in the fourth and fifth hours. But it will probably be conceded, after it has been shown how closely the amount of the urea was associated with the variation in the temperature, that its increase in the fourth and fifth hours after food was merely a coincidence, and was not owing to the very moderate breakfast, but to the fact of the highest temperature occurring at this time. On one day, moreover, he took no food, having no appetite, and on this day the usual increase occurred. Again, after the fits were stopped by the quinine, the food was given him as usual, and the urine being collected on the same day, in the same way, comparatively little rise took place in the urea. These two last points, I conceive, set the question quite at rest.

With respect to the amount of fluid drunk, this could have no effect on the urea, as very little fluid was taken till after the time when the urea had commenced to rise. The urea, moreover, reached its maximum often at the termination of the cold stage, whilst he seldom drank anything between his breakfast and the hot stage. I believe, then, that I am justified in concluding that the rise in the amount of urea was not owing either to food or liquid. It must, therefore, have been owing to the fit.

The urea begins to increase in amount *before* the cold stage, as judged of by the first feeling of shivering, in four of the five fits. Thus, it rose—

On the 7th, from 0.666 to 1.361	per hour.
" 8th, from 0.654 to 1.425	"
" 9th, from 0.927 to 1.351	"
" 11th, from 0.614 to 1.924	"

¹ Voit, quoted by Meissner, "Report on Phys. for 1857," in Henle's 'Zeitschrift,' p. 352.

² Becker, Henle's 'Zeitschrift,' 1855, p. 549.

The time it commenced to rise before the subjective fit varied. Thus—

On the 7th.... $\frac{3}{4}$ hour before.
 " 8th.... $1\frac{1}{4}$ "
 " 9th.... $1\frac{1}{4}$ "
 " 11th.... $\frac{1}{2}$ "

It often began to increase, indeed, even before the temperature began to rise. Thus, as the temperature on the 7th rose before the fit $\frac{3}{4}$ of an hour, on the 8th $1\frac{1}{4}$, on the 9th $\frac{3}{4}$, the urea on those days commenced to rise previous to any similar change in the temperature.

Great apparent irregularity existed in the rise of urea, but a close correspondence is observed between these variations and similar ones in the temperature.

The alteration in the above case of urea and the temperature did not always exactly agree in time, though this was the rule, but sometimes the alteration in the temperature did not occur till after the alteration in the urea; the reverse never happened.

The quantity of urea continued to increase, and reached its highest point either at the termination of the cold stage, as on the 8th and 11th, or at the commencement of the hot, as on the 6th, 9th, 10th, and 12th. It then began to fall in quantity, slowly at first. On the 6th and 10th the temperature and urea commenced to fall simultaneously. On the 7th, 8th, 9th, and 11th, the temperature continued to rise, notwithstanding the fall in the urea. Up to the period when the urea commenced to decrease, the temperature rose rapidly, each rise being extensive, but after that, at the point where the urea commenced to fall, the temperature either oscillated, or every rise remained stationary for a short time, the rise being always slow and by small additions.

The urea continued to fall slowly from the end of the cold, or from the commencement of the hot stage, till the sweating stage began; and the temperature corresponding in time to the latter part of this slow decrease, after reaching its height, fell also slowly and slightly.

From the following table it appears that no close correspondence existed between the fall in the temperature towards the close of the hot stage and the decrease in the amount of urea.

Date.	Fall in Urea.	Fall in Temperature in same time.	Temperature oscillated.	Tendency to be stationary.	Temperature fell continuously.	Fall of Urea reduced to quantity for 1° Fall.
6th	0.268	1°	—	—	—	1.346
7th	0.830	1½°	—	—	—	0.533
8th	1.452	1°	—	—	—	...
9th	0.884	1°	—	—	—	0.884
10th	0.261	1°	—	—	—	0.435
11th	1.000	2½°	—	—	—	0.357

In the first column the date is given; in the second the fall in the amount of urea, corresponding in time to the fall in the temperature, from its highest point to the commencement of the sweating stage, is given; next, the number of degrees the temperature fell in the same time; then, in three columns, the character of the fall is given; and in the last column the fall in the amount of urea is reduced to the quantity corresponding to each degree. But though there is no intimate connexion, still the greater the fall in the temperature the greater is the decrease in the quantity of urea. At the same time the character of the fall varied; thus, sometimes the temperature showed a tendency to remain stationary, then the urea fell but little, as on the 6th and 10th. On other days the temperature fell gradually, and then the fall in urea rather increased. The greatest fall occurred on those days when the temperature oscillated greatly. Thus, the least decrease in urea corresponded to that temperature which has a tendency to remain stationary, more when it fell slowly but continuously, and much the most when it oscillated, as on the 11th; and it is

possible to judge of the rapidity of the decrease of the urea by the character of the temperature.

The temperature has been shown to fall either at the close of the hot or the commencement of the sweating stage. A similar fall occurred in the urea, and the amount of the decrease of urea corresponded to the length of the oscillation, and the subsequent rise in the urea corresponded to the rise in the oscillation, though the whole oscillation always occurred in an hour previous to that in which the subsequent rise in the urea occurred.

Date.	Fall in Temperature.	Fall in Urea.	Rise in Temperature.	Rise in Urea.
6th	1°	$\left\{ \begin{array}{l} 1.820 \\ \text{to} \\ 1.289 \end{array} \right\}$	1½°	$\left\{ \begin{array}{l} 1.289 \\ \text{to} \\ 1.620 \end{array} \right\}$
7th	1°	$\left\{ \begin{array}{l} 1.287 \\ \text{to} \\ 0.838 \end{array} \right\}$	1°	$\left\{ \begin{array}{l} 1.620 \\ \text{to} \\ 1.222 \end{array} \right\}$
8th	1½°	$\left\{ \begin{array}{l} 3.474 \\ \text{to} \\ 0.644 \end{array} \right\}$	½°	$\left\{ \begin{array}{l} 1.222 \\ \text{to} \\ 1.378 \end{array} \right\}$
11th	1½°	$\left\{ \begin{array}{l} 1.839 \\ \text{to} \\ 1.045 \end{array} \right\}$	1°	$\left\{ \begin{array}{l} 1.045 \\ \text{to} \\ 1.356 \end{array} \right\}$

A slight rise, however, took place at the same period, even when the temperature remained permanent, but then the increase was small in amount.

On the 9th, the temperature fell, but did not oscillate; the fall in the urea was not so great as when the temperature oscillated.

A relationship in the latter part of the sweating stage between the variation in the urea and temperature existed, corresponding to what is stated above. Thus, on the 6th, after the usual oscillation, the temperature had a tendency to fall, and then to remain stationary for some time, and again to fall and remain a second time stationary. During this time the urea fell in quantity slowly. The temperature then took a great fall, and oscillated greatly, and at the same time the fall in urea was very great.

On the 7th the temperature fell slowly, and often remained stationary; at the same time the urea fell slowly, and, like the temperature, did not reach the normal amount till late in the afternoon.

The same close correspondence exists between the other cases.

In order to test still further the relationship between the urea and the temperature, the following method was adopted. The total amounts of urea excreted during the fit, the quantity excreted during the rise and fall of temperature, during the period immediately before the fit, and during the cold and hot stages, respectively, have been added up and divided by the number of degrees the temperature rose or fell in the corresponding periods. By this means the amounts of the urea secreted during the whole and each of the divisions of the fit were reduced to a common standard, and could thus be compared more easily with one another.

Table¹ to show the amount of Urea corresponding to each degree of Fahrenheit of abnormal heat.

Date.	Fall in Temperature. The amount of urea for each degree of abnormal heat.	Rise in Temperature. The amount of urea for each degree of abnormal heat.	Fall in the temperature. The amount of urea for each degree of abnormal heat.	The quantity of urea excreted during the fit, before the fit.	The same in the cold stage.	The same in the hot stage.
6th	1.557	0.626	0.930	...	0.339	3.560
7th	1.413	0.615	0.795	0.972	0.391	1.609
8th	1.508	0.903	0.722	2.163	0.735	1.444
9th	1.469	0.675	0.710	0.566	0.246	2.793
10th	1.298	0.718	0.746	0.657	0.617	1.381
11th	1.781	0.712	1.280	0.601	0.590	0.868

¹ In this table each of the quantities of urea correspond to 1° of temperature. In the first column the date is given; in the second, the whole amount of urea passed during the entire fit (the fit being considered to

On comparing the different days, the closest correspondence existed when the whole fit was taken. The extreme in column two being taken, the difference only amounted to one fifth of a degree. The rise and fall also closely corresponded. The extreme again being taken, a difference amounting to one third of a degree was found.

As the duration of the fit might have varied greatly, and so the above mode of comparison of the different days one with the other have been invalidated, the following tables have been worked out.

The first shows the duration of the fit on each day.

6th.....	7½ hours' duration.
7th.....	8 "
8th.....	8½ "
9th.....	7½ "
10th.....	8½ "
11th.....	7½ "

commence with the rise of the temperature and the increase of the urea and chloride of sodium). In the third column the rise (in the temperature) only is taken, and when the temperature remained stationary at its highest point for some time the time of the rise is calculated up to the middle of the stationary part. In the next column the fall, calculated in the same way, is given. Then the cold and hot stages are given separately.

The amount of urea varied in this patient; for the night urine varied on one day greatly from the quantity passed during a similar period on other nights. This day, the 9th (the urine of the night before not having been saved), the amount of urea corresponding to each degree was greatly in excess; but on assuming that the quantity passed during the first hour (0.900 gm.) was the quantity normal to him for that day, and so deducting the excess, a close correspondence to the other days resulted. Thus, when the whole amount of urea was calculated as it stood, to each degree 2.043 grms. were found to correspond; but when the additional 300 grms. were deducted, then the quantity per degree was 1.460; and throughout all the different comparisons it will be then found closely to agree. On the 10th, again, the normal excretion was below par (0.170 gm. per hour), which also prevented any comparisons from being made. When this was raised to 0.600 gm., then a much closer correspondence to the other days was found.

Another calculation must now be made. On three days the urine was collected through the previous night; taking that hourly amount, and assuming that to be the amount normal to the man through that day, if there had been no disturbing influences, and deducting that quantity from the quantity passed hourly through the fit, the increase only is obtained, and this divided by the number of degrees the temperature rose, the comparison is then found to be very close.

April 7th.....	0.638 urea to each degree of abnormal heat.
" 8th.....	0.618 " "
" 11th.....	0.624 " "

If this table be compared with the former one, the comparison will be found to be even much closer. Unfortunately, the amount during the night was not determined except on these three days, so that this mode of calculation is not applicable to all the fits.

On looking at the second column of the table given at page 377, where the total amounts are compared, it is found that on the 6th, 8th, and 11th, the quantity in excess is somewhat over other days. The temperature on the 6th and 8th remained permanent after reaching its highest point. Thus, on the 6th it remained at 105° for two hours and a half; on the 8th at 103½° for one hour and a quarter; but when the amount passed during these permanent periods is deducted, then the urea on the 6th fell to 1.277 gm., and on the 8th to 1.366 gm., bringing each on a level with the 10th. On the 11th, the increase in urea occurred entirely in the fourth column, that is, during the fall of the temperature, the amount passed during the rise corresponding closely to the other days; and on examining the sweating stage on the 11th, there is found a great and sudden increase at its very termination, and this excessive quantity being replaced by the quantity passed on other days at the same period, the amount corresponding to each degree was 1.510, closely coinciding with the other days.

That the increase on the 6th and 8th was due to the above is rendered probable by the following statement: The whole quantity of urea passed on the 6th, during the stationary period of two hours and a half, was 1.957 grm. On the 8th, for one hour and a quarter, the quantity was 1.155. When compared to equal time, that is, the two hours and a half reduced to one hour and a quarter, then, on the 6th, the amount of urea was 0.978, corresponding closely to the quantity passed in the same time on the 8th. From this it would appear that the same amount of urea corresponds to 1° , when the temperature is permanent, whether it be at a high temperature or not, whilst the second table shows that *every degree in the hot stage corresponds to a much greater amount of urea than in the cold or any other stage, whilst the temperature is rising*; thus showing, that the higher the temperature, the more urea corresponds with each rise of a degree than formerly, and thus accounting in some measure for the greater quantity of urea to each degree, when the whole fit is compared, in the more severe fits.

The very close correspondence between the temperature and the urea seems to be thus placed beyond doubt; in fact, the one may be calculated from the other. An example or two will illustrate this.

Thus, on—

April 6th, the amount of urea excreted during the fit was 11.097 grammes; taking 1.503 gramme as the average quantity (all the degrees being taken) corresponding to each degree, this gives a rise of 7° , or a little over; the actual rise was from $98\frac{1}{2}$ to 105 , or $6\frac{1}{2}^{\circ}$.

April 7th, the total amount of urea during the fit was 10.635 grammes. The rise in the temperature here should be 7° ; the actual rise was $6\frac{1}{2}^{\circ}$.

April 8th, the total amount of urea excreted was 9.353 grammes, which would imply a rise in the temperature of 6° ; the actual rise was from $97\frac{1}{2}$ to $103\frac{1}{2}$, being exactly 6° .

III. Chloride of Sodium.

The chloride of sodium, to a very great extent, agreed with the urea in its relationship to the temperature. Traube's statement on this point is therefore confirmed. For the most part, the remarks above made on the urea hold good for the chloride of sodium; still some differences occurred.

On the 8th, the urea between 7.30 and 8 a.m. remained stationary, whilst the chloride of sodium rose continuously.

9th.—Between 8 and 9 a.m. the urea fell, the temperature corresponding, but the chloride of sodium rose to double its quantity.

Again, during the decline of the fit, they differed on the 8th and 9th; the urea at its termination rose somewhat, whilst the chloride of sodium continued to fall.

The following table corresponds to one given for the urea, and shows the intimate connexion between the severity of the fit and the amount of chloride.

Date.	Entire qt. The amount of sodium chloride of sodium for each degree.	Rise in the temperature taken during the hot stage, and the amount of chloride to each degree.	Fall in the temperature taken, and the amount of chloride to each degree.	The quantity of chloride to each degree of the temperature during the cold stage.	The same during the hot stage.	The same during the period before the cold stage.
6th	0.195	0.085	0.086	0.027	0.293	...
7th	0.225	0.159	0.066	0.085	0.259	0.640
8th	0.171	0.107	0.078	0.131	0.162	0.134
9th	0.241	0.150	0.062	0.175	0.702	0.163
10th			Lost.			
11th	0.180	0.149	0.041	0.102	0.176	0.785

Here, as occurred in the urea, the patient on two days

passed a different quantity normally from what he had on former days; so that on one day, the 8th, it was necessary to add 0.022 grm. to each hour to bring it up to the other days. On the 11th it was necessary to subtract 0.044 grm.

On the 6th the urine was not collected before the commencement of the cold stage, so that the amount for each degree is less than it would otherwise have been in the third and fourth columns.

When the extremes in the third column were compared, a difference corresponding to one third of a degree of temperature occurred. When the fourth column was compared, excluding the 6th, a difference of one third of a degree occurred. In the fifth column, a difference corresponding to less than one third occurred, excluding the 11th, on which day the chloride fell to its normal amount before the fit had ended.

In the cold stage an enormous difference was found.

In the hot the difference corresponded to half of a degree.

Beyond these above differences, all that has been said regarding the urea applies equally to the chloride of sodium.

IV. *Water of the Urine.*

In rising and falling in quantity, the water has a very close correspondence to the urea, though there is very little proportion between the different rises and falls in the two. On the 6th, 8th, and 9th, they corresponded in rising and falling at the same periods. On 11th and 7th the quantity of urine in the hot stage falls so considerably, that in the commencement of the sweating stage a slight rise occurs, this being followed, however, by a decided fall during the second hour.

The quantity of urine thus corresponding to the urea, must, like it, correspond somewhat to the variations in the temperature. That the quantity stands in close relation to the intensity of the fit is seen from the following table.

Date. ¹	Quantity of urine in the fit.	Number of degrees temperature rose.	Quantity of urine to each degree.
6th	608 c.c.	5½	117 c.c.
7th	411 c.c.	5½	71 c.c.
8th	449 c.c.	6½	72 c.c.
9th	382 c.c.	5½	71 c.c.
11th	435 c.c.	5½	75 c.c.

It has been shown that some correspondence exists between the oscillation of temperature at the commencement of the sweating stage and the fall and subsequent rise in the amount of urea. The following table shows the relationship between the same oscillation and the fall in the amount of urine passed.

On the 6th and 11th it has been said that the amount of urine secreted was so small during the hot stage that it rose somewhat at the commencement of the sweating, so that no comparison can be given on those days between the fall of the oscillation and the quantity of urine. In the next table, in the first column, as usual, the day of the month is given; in the second, the fall of the temperature in the oscillation; in the third, the fall in the amount of urine; in the fourth, the amount reduced to a degree; in the fifth column, the rise of the temperature in the oscillation is given; in the sixth, the rise in the amount of urine in the hour subsequent; and in the seventh, the quantity is reduced to that corresponding to a degree:

¹ In the first column the date is given. In the second, the total quantity of urine passed during the fit. In the third, the number of degrees the temperature rose during the fit. In the fourth, the number of cubic centimetres corresponding to each degree. Excluding the first day, a very close correspondence existed.

6th	Urine rose	...	1½°	0.030	0.020
7th	1°	0.030	0.030	1°	0.017
8th	1½°	0.039	0.025	½°	0.025
11th	Urine rose	...	1°	0.007	0.035

From this limited table, no great correspondence can be traced.

But though the urea, chloride of sodium, and water, thus constantly show a close correspondence to the temperature, the relative amount of rise was different in these three ingredients.

This indeed was shown well during the analysis, in which 10 c.c. of urine were taken; the amount of mercury solution required for the amount of urea varied greatly from hour to hour, showing no regularity of rise; whilst, on the other hand, the amount required in testing for the chloride gradually rose and then gradually sank, often again rising somewhat at the very close of the fit. Thus, to take the 7th of April:

UREA.

Hour.	Amount of mercury solution required for 100 parts of urine.	Same for NaCl.	Hourly amount of water passed.
7 a.m.	298 c.c.	48 c.c.	25 c.c.
8 a.m.	198 c.c.	144 c.c.	80 c.c.
8.30 a.m.	188 c.c.	135 c.c.	136 c.c.
10.30 a.m.	224 c.c.	90 c.c.	62 c.c.
12 noon.	272 c.c.	45 c.c.	33 c.c.
1 p.m.	268 c.c.	45 c.c.	50 c.c.
3 p.m.	248 c.c.	36 c.c.	50 c.c.
5 p.m.	258 c.c.	18 c.c.	28 c.c.

The urea varies in its proportion to the water; thus, during excessive diuresis, the per-centage amount falls, whilst, on the other hand, the per-centage amount of chloride even then increases, up to the time the greatest amount is poured out, then it as steadily falls, even when the water fluctuates

greatly. These remarks apply to the per-centage of chloride of sodium in the urine.

It appears that the amount of urea undergoes an increase definite in amount, independent of the water. The chloride of sodium also undergoes a definite increase, which, also, is independent of the amount of water; but the water being increased, the same per-centage of chloride is poured out as would have been the case if a smaller amount of water had been voided, the per-centage not being lowered by an excess of water, as is the case with urea.

The chloride has thus a tendency to rise and fall steadily, not observing the various alterations corresponding to temperature that the urea does, but the water, corresponding to the urea in this respect, causes variations of the same character in the total amount of chloride poured out.

The time of greatest per-centage excretion of chloride of sodium does not always correspond to the greatest hourly excretion.

Thus, on April 6th, the greatest amount of saturation was at 8 a.m., whilst the greatest hourly excretion was at 8.30, when the water was at its greatest amount.

7th.—The hourly excretion and the per-centage amount agreed in the time at which they occurred, and so also did the water.

8th.—The same occurred on this day.

9th and 10th.—The same occurred also on these days.

11th.—The per-centage amount of chloride was greatest at 9 a.m., whilst the hourly amount excreted was greatest at 10, the water being most abundant during the last period.

The urea, on the other hand, often decreased in per-centage during the fit, especially if the increase in the water was great, its highest per-centage amount corresponding to the lesser amount of water. Thus, on April 6th, the per-centage amount decreased through the entire fit. The decrease was, however, much more gradual at its commencement.

7th.—The greatest per-centage amount was at 7 a.m., the amount of water being 25 c.c. for the hour. The water

then rose to 80 c.c., and the per-centage amount of urea fell; the amount of mercury solution required for 10 c.c. of urine fell from 29 c.c. to 19 c.c. The water then again rose to 136 c.c., and the solution of mercury required fell in amount to 18 c.c. The water next fell to 62 c.c., and the solution of mercury rose to 22.6 c.c. The water again fell to 33 c.c., and the amount of the solution required rose to 27.4 c.c. The water next rose to 50 c.c., and the solution required fell to 26.4 c.c. The water then remained at 50 c.c., but the solution fell to 24 c.c. Then the water fell to 28 c.c., and the quantity of the solution rose to 25 c.c.¹

From these facts it is evident that both the urea and chloride increase, independent of the influence of the amount of water; that any increase in the latter does not modify the aggregate amount of urea, but that it does that of the chloride.

No connexion exists between the quantity of urine passed during the entire fit and the amount of water drunk.

This is seen in the following table:²

Date.	Water drunk.	Urine passed.
6th.....	2135 c.c.	648 c.c.
7th.....	1335 "	550 "
8th.....	1050 "	428 "
9th.....	500 "	499 "

The urine was passed much more equally as regards the different periods than the water drunk, for the entire quantity was mostly drunk during the hot stage only.

Thus, the urine, urea, and chloride of sodium, were independent, to a large extent, of the quantity drunk,

¹ Every c.c. of the mercury solution corresponds to 10 milligrammes of urea.

² As usual, the date is given in the first column; in the second, the quantity of water drunk during the entire fit, and in the third, the amount of urine passed during the entire fit, are given.

though some connexion appeared to exist, as, when the patient drank copiously, the next time the urine was collected it was found to be somewhat in excess; but this might be a mere coincidence.

TABLE.

APRIL 12.—UREA.

Stage.	Hour.	Total Quantity in this time.	Quantity per hour.	Average Quantity per hour during period.
		Grammes.	Grammes.	Grammes.
Before fit	6-15 to 7-15	1.868	1.868	1.630
	7-15 to 8-15	1.815	1.815	
	8-15 to 9-45	0.604	1.209	
Cold	8-45 to 9-45	0.550	0.550	2.310
	9-45 to 10	0.445	1.780	
Hot	10 to 11	2.287	2.287	2.219
	11 to 11-30	1.075	2.150	
Sweating	11-30 to 12-30	0.756	0.756	0.442
	12-30 to 3-30	0.384	0.128	

CHLORIDE OF SODIUM.

Before fit	6-15 to 7-15	0.153	0.153	0.153
	7-15 to 8-15	0.225	0.225	0.225
	8-15 to 9-45	0.540	1.080	1.080
Cold	8-45 to 9-45	0.210	0.210	0.321
	9-45 to 10	0.100	0.403	
Hot	10 to 11	0.625	0.625	0.467
	11 to 11-30	0.154	0.309	
	11-30 to 12-30	0.090	0.090	0.173
	12-30 to 3-30	0.708	0.236	

QUANTITY OF URINE PASSED.

Before fit	6 to 7-15	73 c.c.		
	7-15 to 8-15	75 c.c.		
	8-15 to 9-45	54 c.c.		
Cold	8-45 to 9-45	25 c.c.		
	9-45 to 10	84 c.c.		
Hot	10 to 11	125 c.c.		
	11 to 11-30	86 c.c.		
	11-30 to 12-30	32 c.c.		
	12-30 to 3-30	128 c.c.		

On the 12th, everything being conducted in the same manner, upon the temperature commencing to rise, the patient was given 3j of quinine, which caused the tempera-

ture to fall again. It continued to fall for half an hour, and then began to rise; the cold stage came on an hour later than on the previous day. The fit, however, was afterwards as severe as usual. The effects on the urea corresponding to this fall cannot be ascertained, as, for some reason, the amount secreted was three times as great as on previous days, for the first two hours it was collected; it then fell somewhat, but continued high throughout the fit, the quantity to each degree being 0.980 grammes.

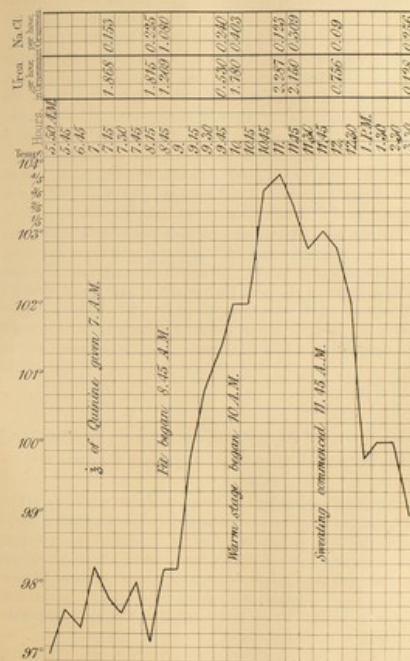
The temperature obeyed all the rules laid down previously—rising to the commencement of the cold stage; then remaining permanent for half an hour; then rising through the whole of the cold stage, the latter being shorter than the former; and just at the commencement of the hot stage, it again remained stationary for a quarter of an hour; then rose, but began to fall before the sweating stage commenced, and at that point oscillated, the oscillation being small; it then fell rapidly.

The urea corresponded also to these variations, after the manner described. Thus, it fell where the temperature remained stationary, but the fall was out of proportion to the alteration in the temperature, being very great. Possibly, this was due to the quinine. It then rose rapidly, reaching its highest point at the same time as the temperature. It fell slowly during the latter part of the hot stage, then fell greatly during the oscillation at the commencement of the sweating stage; but as the urea did not subsequently rise, the rise in the oscillation of the temperature was only one fifth; then the urea fell greatly.

The chloride of sodium differed somewhat from the urea at the commencement and termination. Thus, though an unusual quantity was passed in the first two hours, like the urea, still it did not fall when the temperature began to rise a second time, but continued rising throughout; just before the cold stage it reached its highest point, arriving at the very unusual quantity of 1.080 grm.

APRIL 12TH

Phenomena of a Fit modified by Quinine.



After this it corresponded in its variations to the urea, except in the second hour of the sweating stage, when, instead of continuing to fall, it rose considerably. The rise in the hour subsequent to the oscillation in the temperature was very extensive.

Considering the fit to have commenced on this day, at 7:15 a.m., the amount for each degree was 0.396, a larger quantity (as is also the case with the urea) than occurred on other days; but as the amount passed for some hours before the fit was unusually large, it is probable that the quantity normal for that day was in excess.

The water in its variations corresponded to the urea, except at its very termination (that is to say, the second hour of the sweating stage), when it rose considerably.

The amount for each degree was 105 c.c. It is rendered most highly probable that the reason for the amount for each degree being greater than usual in urea, salt, and water, is, that the normal quantity was in excess on this day, for it is found that the increase in each is proportionate.

The quantity he drank was not taken down on this day. Thus the only influence that can be ascribed to the quinine is the lowering of the temperature and the postponement of the fit.

Examination of the Urine on April 13th, 3ij of Quinine having been taken the day before, the fit being entirely absent.

On the 13th no fit occurred. The patient took, without leave, another scruple of quinine the evening before. The temperature remained at 97° the entire day, with very slight variations, never amounting to more than $\frac{1}{2}^{\circ}$. But, notwithstanding this equality of temperature, the urea and chloride of sodium underwent an increase. The urine was collected through the night.

UREA.

CHLORIDE OF SODIUM.

Hour.	Quantity per hour.	Hour.	Quantity per hour.
	Grammes.		Grammes.
3-30 p.m. to 6-30 a.m.	0.610	3-30 p.m. to 6-30 a.m.	0.657
6-30 to 7-30 a.m.	1.340	6-30 to 7-30 a.m.	0.660
7-30 to 8-30 a.m.	1.122	7-30 to 8-30 a.m.	0.682
8-30 to 9-30 a.m.	1.150	8-30 to 9-30 a.m.	0.696
9-30 to 10-30 a.m.	1.412	9-30 to 10-30 a.m.	0.138
10-30 to 11-30 a.m.	0.803	10-30 to 11-30 a.m.	0.086
11-30 to 12-30 p.m.	0.999	11-30 to 12-30 p.m.	0.091
12-30 to 1-30 p.m.	0.606	12-30 to 1-30 p.m.	0.027

Thus, though the patient experienced no change as regards his sensations, and his temperature remained the same throughout the day, a similar change to that occurring on the previous days was observed in the urea and chloride of sodium. The fit, as judged by the urine, commenced at 6-30 a.m. The urea then fell somewhat during the next hour, rising again steadily up to 10-30 a.m., when it had reached its highest point. It then fell greatly, and this was followed by a slight rise, and then again fell to the amount normal to this man. The fall, immediately after the first rise, probably corresponded to the termination of the period prior to the cold or the commencement of the cold stage; and had the temperature risen, it would probably at this point have either oscillated or remained permanent for some time.

The next great fall, at 11-30 a.m., corresponded to the sweating stage, and would have been marked in the temperature by a great fall, and then, probably, the temperature would have remained stationary for half an hour, and not have risen, as the subsequent rise in the urea is small in amount. The urea after this small rise again falls to the normal amount.

The chloride also strictly followed the principles laid down, for, as has been shown, it has a tendency to rise continuously, an equal amount of urine being taken each hour, though, if the urine be much altered in

quantity, this salt also falls and rises. Thus, the chloride rises continuously, and is marked by no fall after the first hour, but, corresponding to the great fall of urea at 11-30, it also falls and again rises slightly in the next hour, and then again falls to the amount normal.

The urea and chloride increase *pari passu*, and each reaches its height at the same hour.

The amount of urine, again, corresponded to the urea and chloride of sodium; it fell slightly after the first hour, then rose, reaching its highest point at the same time as the urea; it then fell, but again rose somewhat, and then permanently fell.

3-30 p.m. to 6-30 a.m.	40 c.c.
6-30 a.m. to 7-30 "	50 "
7-30 " 8-30 "	46 "
8-30 " 9-30 "	46 "
9-30 " 10-30 "	66 "
10-30 " 11-30 "	41 "
11-30 " 12-30 p.m.	51 "
12-30 p.m. to 1-30 "	30 "

Taking the mean quantity of grammes corresponding to a single degree on other days, the increase of urea would correspond to a rise of 54° in temperature. Thus, as far as the urea, &c., are concerned, the fit was as severe as on previous days.

The urine was again collected hourly during the 14th.

Here the urea and chloride of sodium were again in great excess during the whole day, and also during the night previous.

Hour.	Urea per hour.	Chloride per hour.	Water per hour.
1-30 p.m. to 6 a.m.	1.620	0.168	85 c.c.
6 to 7 a.m.	2.715	0.585	150 c.c.
7 to 8 a.m.	1.332	0.405	90 c.c.
8 to 9 a.m.	2.545	0.877	172 c.c.
9 to 10-30 a.m.	1.106	0.320	67 c.c.
10-30 to 11-30 a.m.	1.197	0.421	78 c.c.
11-30 a.m. to 1-30 p.m.	1.118	0.312	60 c.c.
1-30 to 3-30 p.m.	1.180	0.254	77 c.c.

On the second and fourth hours the urea rose to nearly double the quantity of other hours. After the second rise it remained stationary for two hours, and then rose. This corresponds to some extent with the urea the day before; but after the second rise the fall was not so great, and the subsequent rise was very small in amount.

The chloride more closely resembled the day before. Like the urea of the same day, it rose twice, but the last rise exceeded the first, and was followed by a considerable fall, and this again by a rise of some extent, and then it again fell. The second rise and fall corresponding to the commencement of the sweating stage, the first fall probably would have corresponded (had the temperature risen) to an oscillation or a permanency at the termination of the period prior to the cold stage.

The water closely corresponded to the chloride.

Detracting 1.000 gramme from each hour, that being the necessary quantity to reduce the urea in the night urine to the amount normal to this man, and again, as on the day before, calculating what height the temperature would have reached had it risen, it is found to be 3° .

This fit was therefore less severe than that on the previous day. It thus appears that *variations in the urea and chloride of sodium continue to occur at those periods when, if the fit had continued, the temperature would have risen.* The same fact has been noted by Redenbacher; and it would show that the cure of the fit by quinine, in this man, was followed by a much larger excretion of urea during what would have been the apyretic hours than had been noted at the corresponding time when he was suffering from the disease.

CASE 2.—*Tertian ague.*—The following case of ague occurred in a boy, æt. 19, strong, and in every respect healthy with exception of the attack of ague. The boy had not been out of London for twelve months before, but at that time came over from Dantzic, having been there for some months.

He was admitted into the hospital with the second fit, which was very slightly marked, the fit increasing subsequently in severity till he took four grains of quinine every two hours, which prevented its continuance. It was tertian in type.

Everything was conducted exactly after the manner of the former case. The urine, however, was collected (on one day only) by the stage, and not by the hour. His pulse was taken each quarter of an hour, with the temperature.

The two following charts of the temperature will be found to correspond closely with those given before, the correspondence holding mostly with the severer fits. In the first, the temperature rises slowly up to the cold stage; it then rises rapidly, running up continuously to the commencement of the hot stage, at which point it remains stationary for half an hour; it then runs up to its highest point, remains stationary for a short time, and sinks till the sweating stage commences, when, instead of oscillating, it remains stationary for half an hour, then falls and oscillates, but remains high even at 9 p.m., thus differing from the former and subsequent tables.

This further illustrates the slow rise of the stage previous to the cold one; then its rapid rise, the rise being here also greater than at any other period; also its tendency to be affected first at either termination as the fit becomes less severe. It shows that the temperature falls before the commencement of the sweating stage, and that at this point it either oscillates or remains stationary; and in the severe fit, that the temperature has a tendency to remain stationary at its highest point.

The next chart (that of April 4th) shows the temperature on the day the fit was severer, and consequently we find indications of this in the character and fall of the temperature. It shows at the commencement of the cold stage a tendency to be stationary, but after this it runs up continuously with no permanent period at the termination

of the cold stage, though an approximation is seen in the rise at this point being less than the previous ones. It then remains permanent for some time, and then very slowly sinks, remaining permanent for half an hour just where the sweating commences; the temperature here also falling before the sweating stage commenced. Thus, the continuous rise, the permanency when at its height, and the slow and gradual fall, all indicate the severity of the fit, besides the great height to which the temperature reaches.

The urea, here only estimated for each stage, corresponds to the results before stated. Taking the average amount per hour in the sweating stage as the nearest approach to the quantity normal to the boy, it is found that there is an increase in the period previous to the fit; this continues increasing, is highest during the cold stage, then falls somewhat in the hot, and again falls still lower in the sweating.¹

The same is exactly the case with the chloride of sodium and water.²

The patient drank—

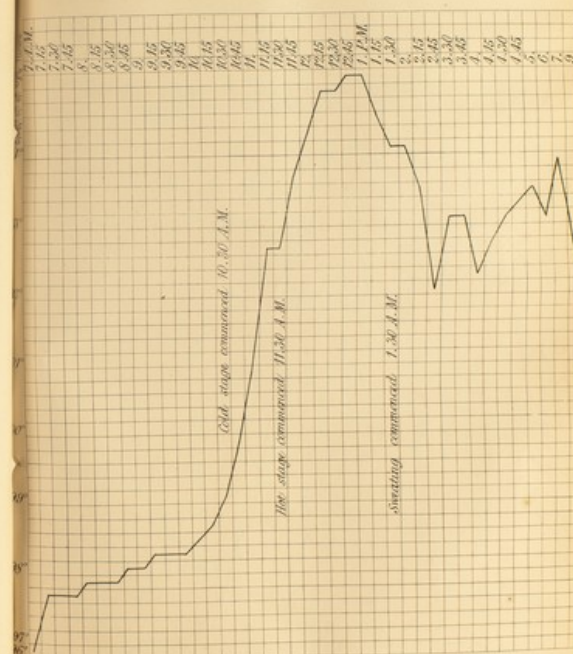
At breakfast.....	285 c.c.
Before cold stage.....	200 "
Cold stage.....	200 "
Hot stage.....	700 "
Sweating stage.....	500 "

He passed the following quantity of urine:

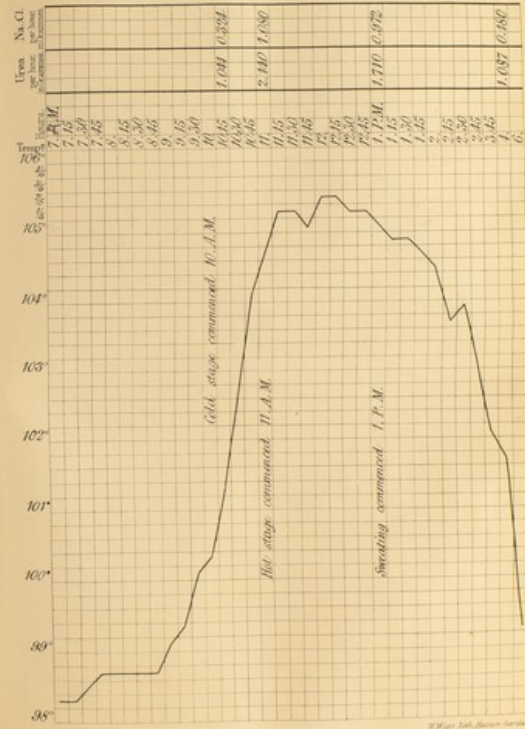
Before fit (3 hours).....	162 c.c. = 54 c.c. per hour.
Cold stage (1 hour).....	100 " = 100 "
Hot stage (2 hours).....	180 " = 90 "
Sweating stage (4 hours) ..	150 " = 37½ "

¹ The quantity of urea to each degree was 1.977 grm., showing a close correspondence to the amount for each degree in the former case. The correspondence would probably be closer still if the normal amount of this patient was reduced to that of the former.

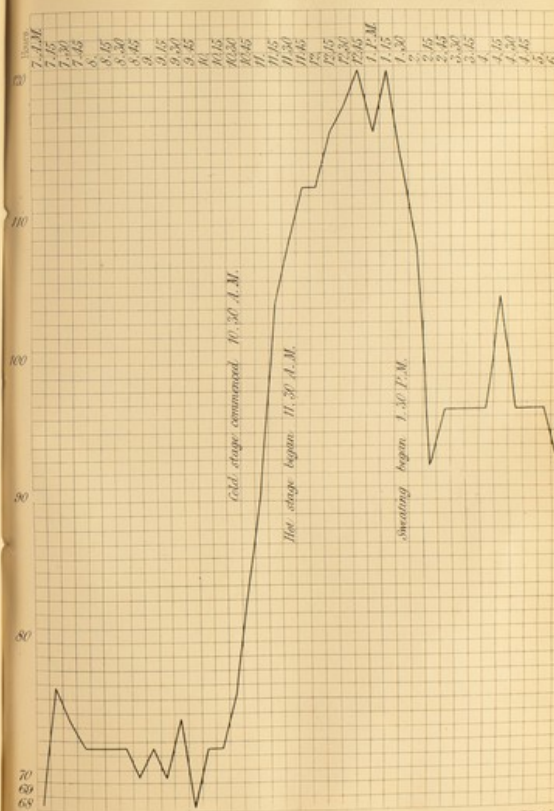
² The amount of chloride of sodium corresponding to each degree was 0.502.

APRIL 2ND

APRIL 4TH

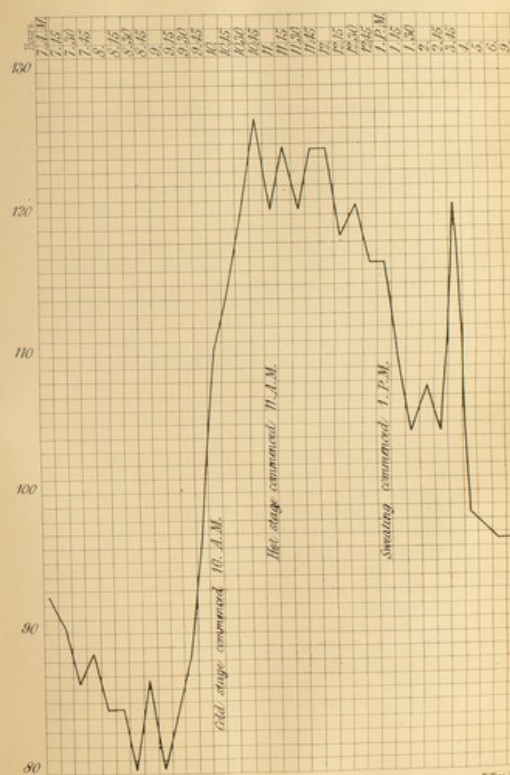


Pulse.



APRIL 4TH

Pulse.



Thus, during the period before the cold stage he drank 323 c.c. in excess over the quantity of urine passed.

Cold stage, he drank	100 c.c. more than he passed ;
Hot stage, " "	520 " "
Sweating, " "	350 " "

making a total of 1293 c.c. in excess of the total amount of urine voided. In this respect, again, he corresponds to the previous patient.

The pulse was taken every quarter of an hour with the temperature in the two fits, and the result is shown in the two following charts. In the first the pulse at the very commencement falls somewhat, and then oscillates. Just before the commencement of the cold stage it commences rising, and then remains stationary for half an hour; then runs up enormously and rapidly during the whole of the cold stage, and at its termination remains again stationary for half an hour; then runs up slightly, oscillates when it has reached its highest point, and immediately on the sweating stage setting in it falls rapidly. In the second chart, at the very commencement, the temperature falls more than it did in the previous one; in the stage previous to the cold it rises more rapidly than in the previous one; runs up still more rapidly during the cold stage, reaches its highest point, and then immediately oscillating, before the sweating stage commences; after oscillating for some time, it falls, and at the commencement of the sweating stage it stands stationary for half an hour, and then falls greatly. During the entire rise there is no tendency for it to remain stationary, still less to oscillate.

This last fit was more severe than the former.

Thus, on both days the pulse commences by falling somewhat, then on both it rises before the fit commences, thus corresponding to the urine and its constituents and also to the temperature. At the commencement of the cold stage, in the less severe fit, the pulse remained stationary for half an hour, and again at the termination of the same stage, whilst nothing of this is seen in the severer one, thus cor-

Thus, a close correspondence exists between the pulse and the temperature. Like it, the character of the rise varies with the intensity of the fit; it rises before the cold stage commences; then, if the fit is not very severe, remains stationary; again rises through the entire cold stage, the rise being more considerable than during any other period; then falls before the sweating stage, and just at the commencement of this stage stands still for half an hour in the severer cases, and falls subsequently slower than in the less severe cases, in which also there is no stationary period.

It occurred in a phthisical patient, set. 45, under Dr. Walshe. It was particularly obstinate in resisting all measures for its removal, and was also peculiar in its occurring always during the day, and its very long continuance, as it commenced at 7 a.m., and continued to 5 p.m. All the stages were unusually well marked, consisting in severe rigors, accompanied with great pain about the loins and legs, the rigors coming on gradually, the sensation of cold being first observed about the feet; the hot and especially the sweating stages followed regularly. The observations were conducted in all respects after the manner of those already detailed.

Here, as in the other cases, the temperature began to rise before the rigors set in, ran up slowly through the

Time	Temp.	Urea	NaCl	Glucose	Cholesterol	Protein
7:30 A.M.	97.0	1.663	0.152			
8:15	97.15					
8:45	97.45					
9:15	97.65	1.144	0.058			
9:45	97.85	0.633	0.357			
10:15	98.15	1.141	0.651			
10:45	98.45					
11:15	98.45	1.231	0.819			
11:45	98.75					
12:30	99.00					
1:15 P.M.	99.35	1.559	1.181			
1:45 P.M.	99.50	1.170	1.343			
2:15	99.75					
2:45	99.80	0.734	0.121			
3:30	99.30	0.980	0.140			
4:15	99.45					
5:15	99.75	1.730	1.170			
6:45	99.85	1.188	0.892			
7:15	99.75					

whole of the cold stage, at its termination remained stationary for half an hour, then again ran up slowly, remaining stationary once, till it reached its highest point, when it oscillated, the rise in the oscillation occupying half an hour, then slowly and gradually fell, every now and then remaining stationary. A close connexion exists between this temperature and those given formerly; the rise, however, before the shivering set in was peculiarly great, and the cold stage very short, the rise in this stage not being so much in excess over the other stages as in the cases of ague.

Urea.

Unlike the cases of ague, the urea falls during the period preceding the rigors, and reaches its minimum immediately they set in; the quantity then rises rapidly, and becomes greatest during the hour immediately preceding the sweating stage. The quantity then falls rapidly through the sweating stage, reaching its minimum when the sweating is most severe; then rises again even higher than it was during the hot stage, and after that falls to its normal amount.

Chloride of sodium.

This corresponds to the urea in all respects, except that the rise at the termination of the fit does not equal the amount secreted during the hot stage.

Water

Again corresponds to both the above, except that its greatest quantity was poured out an hour later than the greatest amount of urea and chloride of sodium (showing the independence of these substances one of the other).

The patient was permitted to drink *ad libitum*; the quantity, however, was not measured.

The following table shows the variations in the amount of urinary water:

Hour.	Amount of Water.
7:30 a.m.	93 c.c.
8:30 "	52 "
9:15 "	35 "
10:15 "	58 "
11:15 "	71 "
12:30 p.m.	97 "
1:30 "	104 "
2:30 "	39 "
3:30 "	50 "
5 "	100 "
6:45 "	70 "

The pulse in this patient remained about 80 throughout the day.

GENERAL CONCLUSIONS.

The following are the conclusions deducible from the facts noted in the cases of ague.

Temperature.

1. This rises before the commencement of the subjective fit.
2. The time before the cold stage at which the rise commences varies.
3. It continues to rise during the entire cold stage.
4. The rise during this stage is greater than during any other.
5. It reaches its highest point during the hot stage, but falls again before the sweating stage, the fall being gradual.
6. The fall is more rapid during the sweating stage. The rapidity is in proportion to the slowness of the fit.
7. Definite variations occur in the rise and fall of the temperature indicative of alterations in the severity of the fit.
8. These variations first appear at the commencement and termination of the cold stage.
9. Their earliest indication is seen in a tendency for the later rises in the cold stage to become less extensive.

10. The temperature next becomes stationary for a variable time at either or both extremities of the cold stage.

11. Should the fit become still less severe, the temperature oscillates at these points.

12. An oscillation in the middle of the cold stage indicates a still further diminution in the severity of the fit.

13. Variations also occur during the hot stage.

14. In the severest fits there is a tendency for the temperature to remain stationary at its highest point.

15. If less severe, the temperature immediately falls on reaching its extreme height.

16. When the fits become still less severe the temperature next fails to reach to such a height as previously, but at this point again has a tendency to be stationary.

17. Should the severity of the fit be still less, it just touches the extreme point, and immediately falls.

18. Variations are also observed during the fall of the temperature.

19. Thus, at the junction of the hot and sweating stage, an oscillation, or a tendency to remain permanent, always occurs.

20. In continuing to fall, the temperature either falls gradually and continuously; this occurs in the severe fit—or,

21. Remains stationary every now and then; this indicates a somewhat less intensity of the fit—or,

22. It oscillates; this accompanies the most rapid fall, and occurs during the least severe fit.

23. After sinking to its extreme point, the temperature has a tendency to rise again, the rise being often considerable.

Urea, Chloride of Sodium, and Water.

24. The urea, chloride of sodium, and water, also begin to increase in quantity before the commencement of the cold stage.

25. They continue to rise rapidly, and become most

abundant either at the termination of the cold or the commencement of the hot stage.

26. These urinary constituents commence to fall in amount before the temperature reaches its highest point.

27. During the latter part of the hot stage they decrease in amount slowly.

28. They fall rapidly during the sweating stage, the rapidity of the fall being proportionate to the slowness of the fit.

29. These constituents exhibit variations corresponding to the variations in the temperature.

30. Thus, when the temperature remains stationary at either end of the cold stage, these urinary constituents also remain stationary, or fall somewhat. When the temperature oscillates, the fall in these constituents is proportionately greater.

31. A fall occurs in these constituents corresponding to the oscillation in the temperature at the termination of the hot stage, the fall in their amount corresponding to the depth of the oscillation.

32. A greater diminution in the amount of these constituents is observed when an oscillation occurs, than when the temperature decreases gradually, or tends to remain stationary.

33. The increase in the urea and water is definite, the same amount of increase corresponding to a single degree each day.

34. A greater increase in these constituents corresponds to a single degree at a high than at a low temperature.

35. No connexion existed between the various rises of the urea and chloride of sodium when equal periods were compared together.

36. The urea appeared to be independent entirely of the influence of the amount of water.

37. The chloride of sodium also underwent a definite increase, but this increased percentage remained the same whatever the amount of water was, so that the total amount of chloride was greatly under the influence of the urinary water.

38. The chloride rose steadily and constantly, and did not observe the variations corresponding to temperature which were seen in the urea, but the water varying with the urea and influencing the chloride, as seen above, caused similar variations in the hourly amount of the chloride.

39. The quantity of water drunk in no way influenced the total amount excreted.

40. The increase in the above constituents often commenced before a corresponding rise in the temperature occurred.

41. The variations in the temperature above described often followed similar variations in the urea, but never preceded them.

42. Quinine given in a single, but large dose, when the temperature commenced to rise, lowered the temperature and postponed the fit for an hour, but had no other effect on that fit, though it prevented its recurrence next day, another scruple having been taken.

43. The pulse corresponded closely with the temperature.

CASE OF
ANEURISM OF THE THORACIC AORTA,
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TRACHEA AND LEFT BRONCHUS;
AND IN WHICH
HEMOPTYSIS OCCURRED FOUR YEARS AND EIGHT MONTHS
BEFORE DEATH:

WITH
REMARKS ON THE CIRCUMSTANCES ATTENDING THE RUPTURE OF
ANEURISMS, ESPECIALLY ON MUCOUS SURFACES.

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Received April 25d.—Read April 26th, 1859.

THE subject of the following observations was Mr. J. B—, a merchant, of a robust frame, and more than average intelligence, who, at the period of his death, was about forty years of age. I was first consulted by him on the 9th of March, 1854, and he remained more or less constantly under my observation till his death, which occurred on the

19th of April, 1858. The mass of details regarding his symptoms, which came to my knowledge during this period of four years, would occupy much more space than can be given to them here, and I must therefore bring within very narrow limits the statement of many facts to which the intelligence of the patient, and the anxieties of his relatives, lent much interest at the time of their occurrence.

From many conversations with the patient, I am enabled to carry back the history of his complaint to nearly six years before the date of my first visit to him, *i.e.* to ten years before his death. The earliest symptoms were pains in the left side and shoulder, sometimes pretty severe, but without any distress of breathing or palpitation. He was at this time accustomed to take a good deal of exercise, and used to experience relief from his pains on walking till he was thoroughly heated. He also found that the use of stimulants frequently removed the pain, which was always lessened after a free perspiration; and partly, no doubt, on this account, the pains were regarded as "rheumatic." He afterwards became subject to more or less of difficulty of breathing, and, under the advice of his ordinary medical attendant, underwent a considerable amount of active treatment.

In October, 1852, it was for the first time intimated to the patient that there was a fault in the great vessels, indicated by a murmur, of the precise character and site of which I have, however, no information. He was forbidden to use severe exertion, which up to this time he had permitted himself without restraint; he was also directed to use palliative remedies, and was allowed moderately good diet, with a proportion of stimulants. Previous to this time, but at what precise period in the history of the case I am unable to state, he was sensible of two distinct aggravations in his complaint, one after a fall in shooting, the other connected with an injury which he received while descending from the top of a stage-coach. After this he always acted on the advice he had received, and was very chary of exposing himself to the least risk of injury.

In March, 1853, a pulsation became apparent in the upper part of the left front, and in the course of the summer he became subject to pretty severe cough. With the cough came a mucous expectoration, and though nothing that was distinctly of blood colour was observed, the sputum was slightly coloured, or "bilious" as he called it.

It was in August, 1853, that the first gush of blood occurred. The circumstances were remarkable, and served to make known Mr. J. B.—'s precarious state of health to a wide circle of his townsmen. He was giving evidence as a witness in a court of justice, under some degree of mental excitement, when, without the least warning, his mouth suddenly filled with blood; and apparently without either coughing or vomiting he brought up such a quantity of pure blood as made him quite faint. He was immediately conveyed home, and there the hæmorrhage was repeated to nearly the same extent as before. For several weeks after this occurrence Mr. J. B.— was confined entirely to his room, and for the most part to the sofa; he also took sedative remedies to a considerable extent. After a time, however, being dissatisfied with the results of treatment, he changed his medical advisers more than once; and ultimately placed himself under my care, as already stated, in the spring of 1854, about six months after the first hæmorrhage, and rather more than four years before his death.

It is unnecessary to go in detail into the history of the case after this. I found the patient with every sign of a large aneurism of the aorta presenting itself in the left front immediately below the clavicle, and passing backwards and upwards so as slightly to involve the left subclavian artery. The radial pulse on the left side was a little weaker than on the right (this sign became afterwards much more distinct, the pulse being ultimately almost entirely suppressed). There was less of dullness on percussion than of tumour and impulse; there was only a trace of the murmur formerly heard. For the rest, the patient was in tolerable condition, sleeping better than he had done for some time after the serious accident of the preceding August; suffering

little positive pain, and having had no considerable expectoration of blood, although he had had a slight cough, with occasional coloured sputa. The pulse was ordinarily 70 to 80. He at this time took some exercise, but as yet very little. His hopes of life had obviously been deeply shaken, and his behaviour was that of a man doomed to a speedy and probably a sudden death.

I directed him to take gentle exercise, and after a time to go for two hours daily to his place of business, but not to lean over the desk; to take light, nourishing, solid food; to abstain from medicine; now and then, when pain was severe, to put one or two leeches over the tumour; and in general to think as little as possible about his complaint, except in the way of caution against violent movement. Under this regimen his condition improved considerably. In the beginning of May he removed to the seaside, and before the end of the same month he took lodgings at a quiet railway station on the Gala Water, on the banks of which he spent the summer with very considerable enjoyment. He became a brother of the angle, and with the help of his wife and a light rod, managed to do a considerable amount of execution among the Gala trout. His mind, too, found great relief from suffering in a resigned and intelligent view of his condition. From having abandoned all idea of an ultimate cure he seemed to derive decided comfort as regards the present no less than the future.

But in the midst of this improved state of health, he continued occasionally to bring up a more or less tinged expectoration; sometimes rusty, sometimes purple, almost never of anything approaching pure blood. This, I believe, continued to be the case, with intermissions of, at most, a few weeks, during the remainder of his life. The two succeeding summers were spent, in part at least, on the Gala; but, although the progress of the disease was slow, I had no difficulty in recognising a distinct progress, chiefly in the direction of the left lung, of which the sounds gradually became more and more impaired. Occasionally he had attacks of severe pain, and now and then paroxysms closely

resembling *angina pectoris*. He had also one or two attacks during the night, which, according to the description, I believe to have been of an epileptic character, but which lasted only a few minutes, and were not accompanied by marked convulsions. In the winter of 1857-8 it was evident to me that the end was approaching; the patient was worn and haggard in appearance; he had lost flesh to a great extent; he breathed with difficulty, and had a somewhat hoarse, rather laryngeal, inspiration, and an altered voice; he occasionally complained of difficulty in swallowing; he frequently brought up blood rather more copiously than he had done since the first attack of hæmoptysis, but never in large quantity, or pure. He also had sickness of stomach to a distressing extent, often lost his sleep at night, and was altogether in a pitiable condition. At length the respiration of the left lung became completely suppressed, the percussion at the same time becoming dull all over the left side; and on the 19th of April, 1858, a small gush of blood, probably not exceeding eight or ten ounces, occurred, which terminated life by suffocation in a few minutes.

A careful post-mortem examination was performed by Dr. Haldane; and the result of it, so far as the interest of the preceding facts extends, is now before the Society. The aneurism involves the descending aorta, from the left subclavian, which is barely free of the sac, to several inches lower down. The sac rests on the vertebrae behind, on the ribs above, on the left lung below and in front. To the left lung it is firmly adherent over a space of many inches square. The left bronchus is stretched over the sac, and has its posterior wall absorbed throughout its whole length. The sac is filled with firm coagulum, which is freely exposed to view from the interior of the bronchus. Exactly at the bifurcation of the trachea there exists another opening into the sac; it is not larger than will admit a probe, and from its smooth rounded edges has evidently been a long time present. This opening, too, rests upon a firm, solid, laminated clot. The left pneumogastric nerve, and the corresponding recurrent, are deeply involved in the wall of the

sac. The left lung is much collapsed, and infiltrated with a considerable amount of purple blood. The right lung was quite normal, and has not been preserved.

I am led to believe the preceding case not unworthy of the attention of the Medical and Chirurgical Society—1st, because of the very long period which existed between the occurrence of rupture of the sac and the ultimate fatal event; and, 2dly, because of the occurrence of hæmorrhage, in a modified form, at intervals during the whole of that very long period. There is not much room for remark on the diagnosis, which, from the time that I first saw the case, was too clear to admit of a doubt. Nor did the case illustrate any new symptom or combination of symptoms in thoracic aneurism, such as to call for remark. But as it presents an undoubtedly very rare, if not unique, example of the prolongation of life after rupture of the sac, and as this particular class of cases has not been made the subject of any very formal investigation, though incidentally discussed by most authorities, I have thought it desirable to accompany the statement of the facts with a short commentary, illustrative of some points which may have escaped the attention of members of the Society.

It is well known that sudden death by hæmorrhage in the case of aneurisms of the thorax and abdomen, opening on mucous surfaces, is very generally preceded by minor discharges of blood, and sometimes by more or less considerable gushes, occurring many hours, and sometimes many days or weeks, before the fatal event. This fact, indeed, though apparent to all who have studied the records of aneurism, was not duly appreciated until a comparatively recent period; these slight hæmorrhages being often either allowed to pass unnoticed, or being referred to some other cause than the true one. The celebrated case of Mr. Liston probably did much to diffuse among the members of his own profession a knowledge of the variable character of the hæmorrhage from aneurismal sacs, and of the remarkable intermissions to which that symptom is occasionally subject. It is well known that between July, 1847, when

hæmoptysis occurred to the extent of many ounces, and October of the same year, when cough recurred, accompanied with a rust-coloured expectoration, Mr. Liston enjoyed an almost complete immunity from symptoms; an immunity so remarkable, that all the warnings of his medical advisers were inadequate to lead him to consider himself an invalid. Mr. Liston died in December, 1847, five months after the first gush of arterial blood, and without any new considerable hæmorrhage. The trachea was found perforated in three or four places, and portions of the aneurismal clot were discovered projecting through the openings, and partially blocking them up. It is evident, therefore, that in Mr. Liston's case the rupture of the aneurismal sac had actually taken place at least five months before death; and that, notwithstanding the unclosed openings, no discharge of blood such as to cause serious alarm to the patient himself had occurred in the interval.¹

Mr. Liston's case was probably not unique even at the time at which it was published. Very few recorded cases, however, exist in the literature of medicine, in which death was postponed so long as five months after a serious hæmorrhage from a mucous surface in aneurism. In proof of this assertion, I may mention that Dr. Edwards Crisp's laborious collection of cases, added to my own reading, and to a list with which I have been kindly furnished from the manuscripts of Dr. Sibson, whose very elaborate and careful study of this subject is known to the members of the Society, furnish in all only nine, or perhaps ten, instances in which an interval of a month or more existed between hæmoptysis and death, and in very few of these was there an interval nearly so long as in Mr. Liston's case between a considerable bleeding and the fatal event. In one case, indeed, pointed out to me by Dr. Sibson's manuscript,² hæmoptysis

¹ See 'Lancet' for December, 1847, p. 633.

² Guy's Hospital Museum, Preparation 1489²⁶. I have had an opportunity to-day (April 25th) of inspecting this preparation, along with Dr. Sibson. The aneurism forms a three-fold dilatation, involving the greater part of the arch, and communicating by separate openings with

is stated to have taken place seven years before death; the connexion of the hæmorrhage with the aneurism, however, does not appear to be clearly made out by the history. Of all intervals less than a month the examples appear to be numerous enough.

It has been my fortune to observe and describe two cases of aneurism in which copious bleeding occurred on a mucous surface, and in which the interval between the first bleeding and death greatly exceeded that observed in the case of Mr. Liston. One of these cases is the one just narrated; the other was a case of aneurism of the superior mesenteric artery, remarkable as a specimen of a rare disease, and a curious, though incorrect, diagnosis, but still more remarkable as showing a minute opening (from the aneurismal sac into the duodenum), which was nearly closed at the time of the patient's death, but had led to profuse hæmorrhage twenty-two months before the fatal rupture into the peritoneum. The whole facts connected with this case are of great interest; but I cannot venture on an abstract of them in the present paper. They will be found in the 'Monthly Journal of Medical Science,' vol. x (for 1850), p. 83.

When aneurisms open into serous cavities it is rare to find death long delayed. Dr. Stokes, however, has referred to one case, where some days probably intervened between rupture into the pericardium and death. I have seen one instance in which "hæmorrhagic pericarditis" was caused by aneurism, and where I was led at the time very strongly to suspect that what appeared to be an inflammatory effusion was in reality a hæmorrhage, the blood having been churned about in the course of the movements of the heart so as to the ascending and the descending aorta. The anterior part of the aneurism is extensively adherent to the right lung, while the posterior sacculated dilatation communicates with the left bronchus. The patient was a woman of fifty-four years of age. She had suffered for some months from palpitation, with pain between the shoulders, at times extending down the left arm; but "it was ascertained after death that she had been the subject of hæmoptysis six or seven years before." She died of hæmoptysis shortly after admission.

decolorize its fibrin. In a case of aneurism fatal by rupture into the peritoneum, also, it has occurred to me to observe adhesions which appeared to have confined the hæmorrhage, during some days at least, to the posterior part of the sac. But these cases are extremely few.

Several cases have been recorded in which, in aneurisms opening on the external surface of the body, a considerable interval existed between the opening and the fatal event. One of the most curious of these is a case communicated by Mr. Ramsay, surgeon at Broughty Ferry, to Mr. Syme.¹ A man affected with aneurism of the arch and of the innominate artery, lost a very large quantity of blood from a rupture opposite the cartilage of the third rib. The stream of blood is reported as being "somewhat larger than a quill;" and, strange to say, the patient, "nothing alarmed, got hold of a bowl, and held it at arm's length to receive the red arch, which he supposed was the contents of a 'bloody boil,' pressing the tumour with his chin to effect a more speedy clearance. After about a quart of blood had gushed out, he fainted, and the bleeding stopped." The patient lived for four months, without any new bleeding, and died in the end, not of the aneurism, but of "typhus fever." Not less curious is a case communicated by Dr. Neligan to Dr. Stokes.² A ship-carpenter, aged 56 years, had all the signs and symptoms of an aneurism of the aorta, opening externally about the second rib on the right side, in front. For more than a year the tumour discharged at intervals, sometimes copiously and in a continuous stream, a quantity of blood sufficient to cause alarm, and occasionally arrested with difficulty. Two of these hæmorrhages occurred under Dr. Neligan's observation. After the first of them, which was by far the most considerable, the tumour diminished considerably in size, and became much more dense, losing the fluctuating character it had previously had. The cough and dyspnoea also sub-

¹ 'Monthly Journal of Medical Science,' vol. x, p. 89.

² 'Diseases of the Heart and Aorta,' p. 582.

sided, and three weeks after the last hæmorrhage the patient left the hospital, "stating that he felt quite well." This is assuredly one of the most remarkable cases on record. The only flaw in it, as an observation, arises from the very circumstance that makes it so interesting, viz., that no opportunity occurred of ascertaining, with absolute certainty, that it was an aortic aneurism. I agree, however, with Dr. Stokes, in thinking that the early signs and symptoms leave no reasonable doubt of this diagnosis. And it is possible, therefore, though perhaps hardly probable, that this case may have ended in a more or less permanent cure of the disease; or, at all events, in long-continued exemption from external hæmorrhage, and death from some other cause.

Cases are not very uncommon, in which aneurisms of the aorta, after opening on one mucous surface and leading to hæmorrhage, are actually fatal by hæmorrhage in another direction, or by some other complication. I have several times seen an aneurism open nearly simultaneously into the trachea and œsophagus; the fatal event having been probably delayed for days after both openings. Similar cases have been recorded, and preparations illustrative of such double openings will be found in many museums. Rupture of an aneurism into the œsophagus, and into the alimentary canal generally, is probably rarely detected until the hæmorrhage is very large, indeed fatally large; because small bleedings, discharged downwards into the stomach and intestines, are almost sure to pass unnoticed. Hence we rarely obtain the opportunity of observing closely the process of rupture on the œsophageal mucous membrane. In the air-passages it is different; very small discharges of blood being here easy of detection. Sometimes, indeed, the true nature of the disease is overlooked, and the hæmorrhage is ascribed to pneumonia, pulmonary hæmorrhagic condensation, malignant disease of the lung, or, perhaps quite as commonly, to ordinary tubercular phthisis. I have seen each of these mistakes made by physicians nowise incompetent or inattentive; indeed, where large gushes of

blood occur in connexion with obscure physical signs, it is not unfrequently all but impossible to decide between aneurism and phthisis, unless the history, age, and appearance of the patient constitute a ground of decision. Even these grounds of diagnosis, carefully investigated, sometimes fail; and I have notes of one case, where large quantities of cod-liver oil were ordered, not unreasonably, nor without benefit, to an aneurismal patient, under the idea of his being consumptive, for several months before the true diagnosis was made.

But the cases in which aneurism is most apt to be overlooked after rupture of the sac are those in which no large hæmorrhage occurs; but in which, for weeks together, perhaps for months, an inconsiderable leakage occurs into the air-passages, assuming the form of—1st, a frothy bronchitic sputum *streaked* with blood; 2d, a rusty sputum very like that of pneumonia, but usually more abundant, more frothy, and less viscid; 3d, a deeply dyed purple or brownish-purple sputum, like the so-called "prune-juice" expectoration, characteristic of the third stage of pneumonia, and of certain forms of pulmonary hæmorrhagic condensation from valvular disease of the heart; 4th, any of the preceding, alternating with small discharges of pure, unmixed, but generally imperfectly coagulated blood. All of these forms of expectoration I have seen on several occasions; most of them occurred at different periods in the preceding case. In general, it may be remarked, that the bronchitic varieties of sputum, either stained or streaked in different proportions with blood, occur chiefly in tumours pressing directly on the trachea and larger bronchi, and not producing consolidation of any part of the lung; while the "prune-juice" sputum, and the varieties more truly resembling the expectoration of pneumonia, occur when the lung is directly involved in the tumour; or when by long-continued flattening of a bronchus, perhaps with extensive ulceration of its mucous membrane, and necrosis of its cartilages, secondary changes have been induced in the pulmonary texture. Such changes are rarely of the nature of inflammation, at least in the first instance. They partake more of the nature of collapse of the air-cells, which is some-

times the result of mere pressure on a bronchus, producing obstruction to the evacuation of the bronchial and pulmonary excretions; while occasionally we find a chronic and continuous infiltration of the lung with blood, either from the aneurismal sac through the bronchus, or from rupture of the aneurism directly into the pulmonary air-cells. The "lobular" character of these lesions is often very clearly demonstrable, and shows that they spring, not from inflammation proper, but from some derangement of the mechanism of the air-passages. In the more advanced stages, however, inflammatory changes are apt to occur; and I have repeatedly seen an entire lung, or some considerable portion of it, ulcerate and break up into suppurating cavities, under the continued pressure of an aneurism upon the bronchus. In one or two cases this has been attended with many of the symptoms and signs of tubercular ulceration, and the morbid appearances after death have also to a remarkable degree resembled those of softened tubercle; confined, however, absolutely to the side on which the aneurismal pressure took place. Possibly some of the cases alleged to be "aneurism associated with phthisis," may have been of this kind.

In systematic works, it is customary to ascribe the minor hæmorrhages to which I am now alluding, not to rupture of the sac, but to "congestion of the lung" from pressure on the veins, and consequent impediment to the return of blood. I am certainly not prepared to maintain that the pressure of aneurisms on the pulmonary veins never causes hæmorrhage. But that this is the chief cause of minor hæmorrhages cannot, I think, be admitted; 1st, because even in cases in which pressure on the veins may have occurred, hæmorrhage (at least continuous or repeated hæmorrhage) is almost always associated either with direct pressure of the sac upon the lung, or upon an ulcerated bronchus; 2dly, because some of the most characteristic cases I have observed of slight and continuous hæmorrhage have been from aneurisms in which no pressure on the pulmonary veins was possible; but in which there was undoubtedly pressure upon, and opening of the sac into, the trachea. I may refer in

particular to two cases of aneurism with laryngeal symptoms, which I recorded some years ago with a view chiefly to other points of interest, but in one of which the patient died of dyspnoea after weeks of constant though inconsiderable hæmorrhage; while in the other a fatal hæmorrhage occurred after many days of very slight tinging of the sputum. In both these cases there was no reasonable doubt, from the condition of the mucous membrane of the trachea, that the blood must have come from the sac.¹

While, therefore, I will not venture to say that blood in the discharges of a patient affected with aneurism *always* indicates the communication of the sac with a mucous membrane, I believe it *generally* does so; and more especially is this the case in aneurisms accompanied by hæmoptysis, if the pressure of the tumour be on the trachea, and if it be unaccompanied by the indications of pulmonary change. The importance of this view, if correct, both as regards the diagnosis of obscure cases of aneurism, and the prognosis of this disease in well-marked cases, it is unnecessary to point out at length. I may be permitted, however, to detain the Society over one view of diagnosis, which, if it be as generally applicable as my own experience would lead me to affirm, must be one of considerable importance. In *aneurisms* characterised chiefly or exclusively by laryngeal symptoms, it is often extremely difficult to arrive at a satisfactory conclusion as to the cause of the very distressing dyspnoea. Now, it is precisely in this class of cases that the repeated presence of even small quantities of blood in the sputum becomes a most valuable means of diagnosis. For, if there be laryngeal dyspnoea and stridulous respiration (which are seldom present to any marked extent in mere laryngeal phthisis); if the epiglottis be not thickened; if the mucous membrane of the larynx, in so far as it is within reach of the finger, be sound; and if, with these signs, positive and nega-

Cases

¹ See 'Monthly Journal of Medical Science,' vol. xiii, p. 137; and vol. xvi, p. 116.

tive, there be a persistent tendency to even the slightest amount of blood in the sputum, while auscultation and percussion give negative results both as regards the lungs and heart, I believe that aneurism may be predicated with as near an approach to certainty as is possible without the physical signs of tumour; and further, the aneurism will be small; it will arise from the back part of the arch, or from the commencement of the innominate artery; and it will be so placed as to entangle either the left or the right recurrent nerve. These considerations have more than once led me to the diagnosis of aneurism under circumstances where, without them, it would have been impossible to give a decided opinion; and hitherto they have not led me wrong. In fact, there is but one form of disease which, in any considerable number of instances, leads to laryngeal stridor and to hæmoptysis, without positive ulceration of the larynx. Malignant tumours very closely resemble aneurism in their diagnosis in these respects. But I have never yet seen malignant disease of the chest leading to hæmoptysis, without manifest physical signs of disease in one or other lung. So that I am inclined to believe that the rules of diagnosis mentioned above, will hold good in the great majority of instances.

A reference to a drawing¹ illustrating the usual mode of opening of an aneurism upon a mucous membrane may close this rather desultory paper. It is from the case of a man who died, not of hæmorrhage, but of suffocation; but in whom, nevertheless, an opening into the trachea existed, which had yielded blood in small quantities for some time before the fatal event. The rupture has been arrested by death at its earliest stage; and it will be observed that while five or six minute papillary eminences, with distinct pale apices on a congested membrane, are to be seen, only one of these has been perforated; the actual opening not exceeding

¹ This drawing was exhibited at the meeting of the Society at which this paper was read.

the size of a pinhole. The mucous membrane as viewed from within, opposite a necrosed cartilaginous ring, is puffy and thin; and in all probability a larger rent would ere long have formed there. But it is very easy to understand on looking at this drawing, how these small pinhole openings should sometimes heal up; especially after a sudden removal of pressure, such as occurs after a copious hæmorrhage. Such would appear to have been the course of events in the case of Mr. J. B—, and also in that of Mr. Liston; as well as in the case of aneurism of the superior mesenteric artery referred to above.

THE
ACTION AND SOUNDS
OF
THE HEART;
A
PHYSIOLOGICAL ESSAY.

BY
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ETC. ETC.



LONDON:
JOHN CHURCHILL, NEW BURLINGTON STREET.
MDCCCLX.

PREFACE.

THE reader will find in the following pages considerable extracts from the writings of Billing, Bryan, and Brakyn, and at the end of the work a few Notes recording the opinions of trustworthy observers and writers on the subject under examination; the author's wish being to give his fellow-labourers all the merit that is their due; for he holds it to be expedient for the advancement of knowledge that each man of science should fairly acknowledge whatever help his mind has received from others.

*London, 17, Victoria Square, S. W.
19th December, 1859.*

PART I.

THE ACTION OF THE HEART.

It is singular that all previous inquirers into the subject of the Heart's movements, from Harvey downwards, have first removed the pericardium, and then made their observations. By this proceeding they have been confused, for before the removal of the pericardium the motion of the entire organ is natural, *i. e.* peristaltic; but after it loses its investing membrane it may truly be said to wobble, *i. e.* to have an uncertain or unequal motion. This explains Cruveilhier's description of the heart's movements, as detailed in the *Gazette Medicale* of August 7th, 1851. This eminent anatomist hastened to view these movements in a case of Ectopia of the Heart, and he has minutely recorded them; but he seems to have forgotten that he was beholding a *lusus nature*, not destined, because not perfected, for more than a few hours' fruitless struggles, for he tells us that the heart had *no pericardium*; the result of which was, that the heart under observation had no support, but by its own gravity made its fixed point where in the natural heart there is most movement, *viz.* at its connection with the aorta and pulmonary artery.

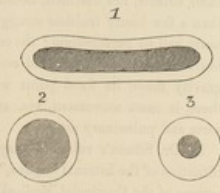
In Dr. Sibson's valuable essay "On the Changes in the Situation of the Internal Organs," published in "The Transactions of the Provincial Medical and Surgical Association,"

vol xii. 1844, the heart's action is most minutely described; but, unfortunately, the pericardium was first removed,—a circumstance to be regretted, for from my own knowledge of the subject I am sure that the observations were most carefully made, and what was seen accurately recorded. Dr. Sibson's essay will well repay the reader's perusal.

I will first introduce to the reader's notice the substance of a paper to be found in "The Lancet" of February 8th, 1834, by Mr. Bryan, then of Stowmarket, now, I believe, residing at Cambridge. This paper is very remarkable, being purely theoretical, its author never having seen the heart act; but it may be safely taken, with a little modification and extension, as a key to comprehending a series of movements so rapid and apparently so confused as to have hitherto baffled the most eminent in anatomy, but yet so truly simple that any one acquainted with the structure of the organ may, by a careful perusal of Bryan's remarks, and comparing these with the results of my own repeated observations, at once see the order in which the several movements occur, and comprehend the necessity for the order so set forth. Bryan says—

As the heart consists of two sets of hollow muscles, alternately dilated by and contracting upon the blood, the investigation of its action necessarily requires some previous consideration of the physical state of hollow bodies distended by or contracting upon a fluid.

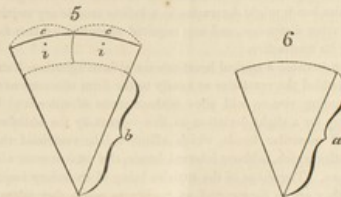
Let Fig. 1 represent the section of a hollow muscle in a state of relaxation, and having parietes of equal thickness throughout. By



a well-known law of physics it would, when distended by fluid, assume a spherical form, Fig. 2; and in contracting and expelling a portion of its contents it would, of course, become a smaller sphere, as Fig. 3.

For the sake of elucidation, let us conceive a heart of the above simple construction. Let *a*, Fig. 4, represent the ventricles, *b* the auricles. The ventricles, *a*, in contracting would become a smaller sphere, as *c*, and, consequently, the parts in contact with their outer surface would be alternately dragged into, and pushed from, the space *ii* at each systole and diastole. Similar phenomena would attend the action of the auricles. The parts in contact with such a heart would be injured by its movements.

Let the strong lines, Fig. 5, represent the outline of a hollow, distended, muscular cone. In contracting it would become a

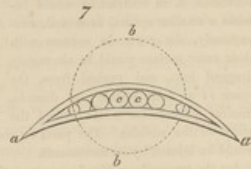


smaller cone, as Fig. 6. It is evident the whole surface *a* of the lessened cone would be exactly adapted for the space *b* previously occupied by the lower portion of the cone in its distended state; and if auricles were situated at the base, as *c*, they might, by distending in proportion as the ventricles contracted, fill the space *ii* from which the base of the cone had receded. From

this it may be seen that conical ventricles, with auricles at their base, might contract powerfully without altering the form of the pericardium containing them, and, consequently, without agitation of the surrounding parts.

I would here wish the reader to consider what might determine the form of a hollow body during its distension by, or contraction upon, a fluid. Suppose that *a a*, Fig. 7, represents the section of a hollow muscle containing a fluid. When contracting upon its contents it would (as explained in connection with Figs. 1, 2, 3,) spring into the spherical form, as *b b*; but if it possessed bands stretched across its cavity, as *c c*, &c., it might, during distension and contraction, retain its original form. Different modification and arrangement of similar bands might determine in a hollow muscle any required form during distension, and any required alteration of that form during its contraction.

Then, because a conical heart can act without agitation, Nature has moulded the ventricles as nearly to the form of a cone as the surrounding viscera will allow without waste of space, and has made up for a slight deviation in this respect by the addition of internal muscular bands, which afford to the ventricles those properties which, without internal bands, the perfect cone alone possesses. The shape of the auricles being of secondary importance, they are so constructed as readily to adapt themselves to the varying form of the parts in contact with them; and, as the shape of their loose portions—the appendices—is extremely irregular, numerous internal bands become indispensable to their smooth and tranquil action. These muscular bands of the auricles and ventricles have much engaged the attention of physiologists, and various and unsatisfactory have been their opinions concerning them. They enable the heart to act smoothly.



Ventricles, without them, unless their figure were that of a perfect cone, would be a torment to the being to whom they belonged.

From what has been advanced I have been led to believe that the heart moves as exemplified in the following diagram.

Let Fig. 8 represent the heart *a*, ventricles *b b*, auricles contained in the pericardium *c*. Suppose the ventricles to have arrived at their extreme distension; the instant contraction commences the auricular valves are closed by the blood, but no important alteration in the size or form of the ventricles takes place until the pressure of the blood contained in their cavities becomes greater than that in the arteries, when the basilar portions sink towards the apex, as from the line below *b* (on the right) to the dotted line below *i*, occasioning partial or perhaps total obliteration of their cavities. While this depression of the base of the ventricles is taking place, the auricles *b b* swell into the space *i i* receded from, thus rendering it unnecessary for the pericardium, and, consequently, the parts in contact with it, to close in upon the contracted ventricles. During the diastole of the ventricles the reverse of this movement takes place, *i. e.* the bases of the ventricles rise into the space *i i*, from which the auricles withdraw.

It will naturally be questioned by what power the ventricles recede towards their apex, when, from their attachment to the aorta and pulmonary artery, one would be led to conclude that the apex and the other portions of the ventricles would be drawn towards the orifices of those vessels. Let us for a moment reflect upon what would be the movement of the ventricles if they were quite free. It is certain that, in ejecting their contents by the arterial orifices at their base, they would recede in the opposite direction—that of their apex. This motion would be serviceable with respect to the heart's minor function, that of receiving

Fig. 8.



the blood, for by abstracting pressure from the auricles it would induce the flow of blood into them by the force commonly called suction. Could it for a moment be supposed that nature would use means to oppose this natural and useful movement of the ventricles, in order to establish a movement in the opposite direction, *i. e.* that of the arterial orifices, which must tend to trammel the dilatation of the auricles, causing them to swell in a direction upwards and backwards, while the ventricles, contracting towards the above points, must alternately drag towards and push from these points the lungs, &c. in contact with them. If the orifices of the aorta and pulmonary artery were the fixed points towards which the muscular fibres of the ventricles contracted, the heart at each beat would move in the manner of the simple heart represented by Fig. 4, produce much agitation in the chest, and occasion a *bruit de soufflet*, by causing the entrance of air into that portion of the lungs which is suddenly dragged into that space from which the substance of the ventricles must unquestionably recede in tending towards the above points. Such action as this is highly inconsistent with that delicacy and precision which nature displays in all her works. The obliquity with which the aorta and pulmonary artery arise from the ventricles would permit a slight descent of their orifices towards the apex, and the necessity for much movement of these parts might be, and I believe is, obviated by the posterior and lateral portions only of the base descending.

The last few lines prove that Mr. Bryan never saw the heart act, else he would not have made this reservation of the descent of the origin of the large vessels to his theory; for, as will be seen further on, they descend freely, this movement being favoured by the perfect elasticity of the arterial coats.

In prosecuting my own inquiries, asses and dogs have been the subjects of experiment, and the following method has been adopted: chloroform was first administered to produce insensibility, then the trachea divided, and the nozzle

of a bellows inserted into the lower portion, and artificial respiration maintained; the anterior portion of the thorax, viz., sternum, the cartilages of the ribs, and sometimes parts of the ribs themselves, according to the depth of the thorax, removed or turned upwards. In this process it is necessary, occasionally, to drop some chloroform into the valve of the bellows, otherwise the animal may become conscious, and disturb the operations.

Observation 1.—When the heart is thus exposed, the movements of the whole viscus, and alternately of its separate parts, can be studied through the more or less transparent pericardium. (Should the pericardium be loaded with fat, it can easily be dissected off without piercing the membrane.) The artificial respiration being properly and rhythmically kept up, the facility and smoothness of the heart's action strikes the observer, any impulse or beat of the heart not entering into the mind. The point of the finger coming in contact with the ventricles, immediately dispels the deception, and the *apparent* blow is distinctly felt. (1) A stethoscope, lightly applied, is raised against the ear, and the first and second sounds are distinctly heard.

Observation 2.—When the pericardium is removed, and its contained fluid consequently lost, the heart's action becomes immediately tumultuous; the contractions appear more forcible, and the sounds are much louder; and this is the character, more or less intense, of its action ever after. The pericardium, therefore, regulates the heart's movements.

Observation 3.—The first sound occurs during the contraction of the ventricles, and the second immediately follows it; that is, whilst the auricles are being filled with blood and no muscular action is going on.

It is necessary now to bear in mind, that the ascending

portion of the arch of the aorta and the commencement of the pulmonary artery must be looked upon as heart when speaking of the movements of the latter; for they are equally contained within the pericardium.

Observation 4.—During the systole, or contraction of the ventricles, the base of the heart approaches the apex; the latter at the same time is pressed downwards, backwards, and from right to left, describing part of a circle; the ventricles, assuming a contracted globular form, descend, describing also part of a circle, but passing forwards and from right to left. There is also a forward movement of the left ventricle above the apex from left to right. These minor movements must be seen, they cannot well be described; but the facts of importance to be remembered are, that, when the ventricles contract, their bases descend towards the apex, and the latter is not tilted forward so as to give any blow to the thoracic walls, but is pressed downwards, its extremity being directed backwards and from right to left.

Observation 5.—The ventricles contracting, the auricles simultaneously receive blood and then occupy part of the space within the pericardium previously taken up by the ventricles in their relaxed distended state. (2)

Observation 6.—With each contraction of the ventricles and descent of their bases, the pericardial portions of the aorta and pulmonary artery become suddenly greatly elongated and distended, and as suddenly, by their elasticity, react upon their contents, becoming of uniform calibre, but visibly distending the sinuses of Valsalva. (3) The venæ cavae are similarly distended during the contraction of the auricles.

Observation 7.—The finger and thumb spanning the diameter of the ventricles are perceptibly further separated during the contraction of the ventricles, and approximated during their relaxation.

Observation 8.—The same apparent blow is felt over every part of the ventricles during their contraction, but more forcibly over the centre. The impulse at the ribs is most probably given by the fibres just above the apex.

Observation 9.—The apex is the most fixed part of the heart. Great pains were taken to ascertain whether the eye was deceived or not. A piece of string was stretched tightly across the thorax, and a pin thrust through it into the substance of the heart. The string acting as the fixed point, the oscillations of the pin corresponded to the movements of the part into which it was inserted. Thus, when inserted about the centre of the sulcus separating the ventricles anteriorly, the head of the pin moved upwards during the contraction of the ventricles, and downwards during contraction of the auricles, that is, in a reverse direction to that of the heart and the pin's point inserted into it. In this situation the pin moved longitudinally along a line equal to $\frac{1}{2}$, whereas when inserted into the apex the distance did not exceed $\frac{1}{4}$. The same was tried over other parts of the heart, and always with the same result, viz. that the apex was more fixed than any other part. (4)

A careful consideration of these observations shows that the heart's action is extremely simple; that there exists a reciprocity or compensation between the auricles and ventricles, and that, although the chambers of the heart are continually receiving and discharging their contents, these actions are so regulated that the bulk of the contents of the pericardium is always the same. For instance, when the ventricles contract, the loss of the bulk of the fluid they have ejected is made up, or compensated for, by the bulk of the fluid pouring into the auricles, and by the increased

bulk of the pericardial portions of the aorta and pulmonary artery. This is shown in figs. 9 and 10.

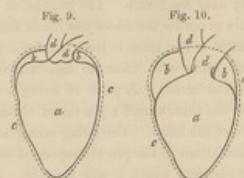


Fig. 9.—*a*, ventricles relaxed and full; *b*, *b*, auricles contracted and empty; *d*, *d*, aorta and pulmonary artery full, but not distended, the semilunar valves being shut down; *c*, pericardium.

Fig. 10.—*a*, ventricles contracted and empty; *b*, *b*, auricles relaxed and distending (they do not become fully distended until the aorta and pulmonary artery have, by their elasticity, reacted upon their contents, and have become of diminished calibre, the auricles still filling after the second sound is heard); *d*, *d*, the aorta and pulmonary artery greatly distended and lengthened, for observe, that the bases of the ventricles, from which these vessels spring, are lower than in fig. 9; *c*, *c*, the pericardium.

This uniform shape of the pericardium is however departed from at the instant of the ventricular contraction, the change of form causing the impulse which has erroneously been attributed to the tilting forward of the apex, whereas it may be caused by any portion of the ventricles in contact with the thorax or diaphragm. Were there no opposing column of blood pressing upon the upper surface of the semilunar valves, there would be no blow or impulse caused by the ventricular systole. How much this impulse is augmented and communicated to surrounding parts where diseased valves obstruct the circulation, is too well known for me to enlarge upon here.

We must, then, not forget, in our physical examination of patients, that any part of the ventricles being in contact with the chest-walls will give, during contraction, an impulse.

Let us now turn our attention to what is taking place in the interior of the heart simultaneously with the external conditions we have been noticing.

The cavities of the auricles and ventricles being at one time continuous with, and at another separated from each other, by what means and at what time are these changes effected? In short, what are the functions of the auriculo-ventricular valves, chordæ tendinæ, and muscoli papillares? Also, What share does the blood take in these actions?

To determine these points I adopted the following method. A bullock's heart was obtained, and the auricles cut away nearly as low down as the auriculo-ventricular openings; the cavities of the ventricles were well washed out, and the coagula carefully removed. A vulcanised india-rubber tube of like diameter with the pulmonary artery was then attached by one extremity to the vessel, and by the other to a common forcing-pump; water was then thrown into the pulmonary artery, and the semilunar valves tightly shut down, gentle pressure being maintained, in imitation of what takes place during life. The right ventricle, being empty, was in the same state as when the auricle is about to inject it. On pouring water into the ventricle the flaps of the auriculo-ventricular valve rose upon the surface of the fluid, until (the ventricle becoming fully distended) the valve formed a perfect septum between it and the auricle. The left side of the heart was tested in the same manner, and with results perfectly the same, notwithstanding the greater thickness of the valve, the larger size of the muscoli papillares, and the stronger chordæ tendinæ. Fig. 11 represents the right auriculo-ventricular, and Fig. 12 the left auriculo-ventricular, valves closed without the least pressure on the sides of the ventricles.

The action of these valves has been previously shown to

Fig. 11.

Fig. 12.



be passive. I find Valentin (5) quotes some experiments by Fick. He says, "These peculiar structures are best shown by pouring in water from the pulmonary artery and aorta, until it reaches to a point in these vessels which is somewhat higher than the region of the auriculo-ventricular valves." This, however, is a very different proceeding from that adopted by myself—the principle is different; nevertheless the true action of the valve is thus stated by Valentin further on: "The slightest pressure suffices to close the auriculo-ventricular valves—a very weak contraction of the walls of the ventricle—indeed, the mere force with which the blood is expelled from the auricle, and rebounds or is rejected by the elasticity of the ventricular walls, completely suffices for this purpose."

Dr. Hamernik (6), recapitulating at some length the opinions of Dr. Baumgarten, says,—

That the auriculo-ventricular valves are closed by the counter-pressure of the ventricular blood, suddenly developed by the contraction of the auricles; that the cavities of the auricles and ventricles during the heart's diastole are distended by the continuous current from the veins, while at this period the valves are to be found floating in the blood in the form of a funnel; that the object of the auricular systole is to induce such an amount of tension in the contents of the ventricles, and of course in the blood surrounding the funnel-shaped arrangement of the valves, as to cause their rapid closure; and in this way only can regurgitation be prevented.

Now, I grant that Dr. Hamernik is perfectly correct when he states that "the auriculo-ventricular valves are closed by the counter-pressure of the ventricular blood suddenly developed by the contraction of the auricles;" but he is incorrect when he says that "the cavities of the auricles and ventricles are distended during the heart's diastole;" for, as I have shown by the diagrams Fig. 9 and Fig. 10, there cannot be full auricles and full ventricles at the same time within the pericardium. Nevertheless the truth came out of these experiments, as witness the remark of the Editor of "The Monthly Retrospect:"—"The experiment by which it is shown that the auriculo-ventricular valves are closed before and independently of the ventricular systole is very easy of performance."

Independently of the above authorities, I was led from experiment to treat of the closure of these valves as follows:—*The auricles contracting on the blood, the force of their contraction is transmitted by the blood in all directions, separating the flaps of the valves, distending the ventricles, and (the semilunar valves being shut down) pressing as much upwards and backwards, as downwards and onwards. The force, not being sufficient to raise the semilunar valves, is expended in distending the ventricles, and raising and closing the auriculo-ventricular valves.*

Let the fact of there being a perfect septum between auricle and ventricle, previous to the contraction of the latter, be taken in connection with the fact (as stated in Observation 4, and as shown in Fig. 10) of the descent of the bases of the ventricles during their systole, and we arrive at a certain conclusion respecting the functions of the muscoli papillares. Seeing then that the valve forms a perfect septum as above, when the ventricle contracts as from *a*, Fig. 9, to *a*, Fig. 10, these muscles also contract in

B

proportion as the base of the ventricle descends, and their tendons (chordæ tendinæ) being inserted into or connected with the under surface of the flaps of the valve, from apices to circumference, maintain the proper tension of the valve till the whole of the blood is expelled from the ventricle.

The auriculo-ventricular valve forms a perfect septum between the auricle and the ventricle as soon as the latter becomes distended, and is kept in this position during the whole ventricular systole by its own proper muscles (musculi papillares), which, by contracting simultaneously with the descent of the base of the ventricle, prevent the eversion of the valve into the auricle. (7).

The following Table will facilitate the comprehension of the heart's action, the time, and, as will be shown further on, the cause of its sounds (see opposite page):—

We now pass to the more difficult part of our inquiry, viz. The Cause of the Sounds of the Heart.

ANALYSIS OF ONE COMPLETE ACTION OF THE HEART.

Time occupied, say, one second.

Rhythm.	Time.	Order of Occurrences.	Muscular Action taking place.
LAST PART OF PAUSE.	↓	{ Auricles contracting. Ventricles distended with blood by the force of the Auricles. Auriculo-ventricular valves closed.	CONTRACTION OF AURICLES.
FIRST SOUND AND IMPULSE.	↓	{ Ventricles contracting on their contents; tension of the auriculo-ventricular valves and chordæ tendinæ; blood passing into and increasing the calibre of the Aorta and Pulmonary Artery. (The ventricles contract, permitting the flow of blood into them from the vena cava and pulmonary veins.)	CONTRACTION OF VENTRICLES.
SECOND SOUND.	↓	{ Ventricles relaxing; auriculo-ventricular valves and chordæ tendinæ no longer tense; small quantity of blood passing into Ventricles. Auricles fill with blood. The tension of the Aorta and Pulmonary Artery diminished. The tension of the walls of their coats on the contained blood, producing forcible closure and tension of the semilunar valves.	NONE.
FIRST PART OF PAUSE.	↓	{ Ventricular fibres, auriculo-ventricular valves, and chordæ tendinæ, fully relaxed. The tension of both Auricles and Ventricles is arrived at for a new act of action.	NONE.

N.B. The above Table shows the contents of the Pericardium, from the Apex of the Heart to its connection with the Aorta and Pulmonary Artery, always to be the same.

PART II.

THE SOUNDS OF THE HEART.

THE second sound is almost unanimously believed to be due to the tension of the semilunar valves of the aorta and pulmonary artery. The cause of the first sound, however, is differently stated by physiologists, inasmuch as up to the present time at least thirty explanations have been offered of its production. Need I say how detrimental to the study, and discouraging to the student, of medicine have been this uncertainty and perplexity? The subject has by its occultness been peculiarly enticing to the speculative and fanciful; but it must not be left in such hands, it must be brought back and tested, both theoretically and experimentally.

Dr. Billing, in the preface to the second edition of his "First Principles of Medicine," thus speaks of the Sounds of the Heart:—

In an essay read at the anniversary meeting of the Hunterian Society (February 9, 1832, and reported in "The Lancet," May 19, 1832), I first stated publicly that "the push or beat felt at the side is caused by the ventricular muscles, in their systole to expel the blood. The first sound is caused by the tension produced in the shutting of the auriculo-ventricular valves, and the second sound is caused by the tension produced in the shutting of the ventriculo-arterial valves, &c. . . . This is a simple, unsophisticated explanation of the causes of the beat and sounds of the heart; and you will find that the morbid signs are all explicable as alterations of these." Subsequently Rouanet brought

forward the same explanation in his thesis, which was noticed in the "Journal Hebdomadaire" (September, 1832), and copied into the "Medico-Chirurgical Review" (April, 1833), as well as an extract from my essay; in which I had advanced, as I thought, sufficient to enable any practised physiologist to confirm my positions by the suggestions of his own mind. Finding, however, from some observations, that this did not appear to be the case, I published the following additional remarks in "The Lancet" (November 30, 1833):—"The succession of phenomena of the heart's action is as follows:—First, the auricle contracts, then the ventricle; by the action of which latter the auriculo-ventricular valves are shut by the pressure of the blood against them (8). Upon the relaxation of the ventricle, the semilunar valves are shut by the backward pressure of the blood in the artery. The first sound takes place exactly synchronous with the impulsion and action of the ventricle; hence it might be supposed that the action of the muscle (as averred by some) produces the first sound. But the second sound takes place at a time when there is no action of the heart going forward; and this is peculiarly evident when there is an intermitting pulse, as there is then a marked pause after the second sound; so that, in fact, there is nothing but the semilunar valves in operation to produce sound at the instant." I have thus proved that the second sound can be produced by nothing but the valves; and I have therefore shown the tension of the valves to be a sufficient cause for the first sound; and as *nil frustra natura facit*, according to the laws of reasoning in physics, more causes than are true or sufficient are not to be assigned (Newt. Princip. lib. iii. Reg. Phil. 1), so I discard muscular action as the cause of the first sound.

Speaking of the conclusions arrived at by the Committee of the British Association, appointed to inquire into the cause of the first sound, he further says:—

Now, as to their experimentum crucis (to show that muscle produced the first sound), of putting the finger into the heart after the valves were destroyed, and their hearing a sound proceeding from the contraction of the heart, with air, comes

columnae, and bloody moisture in it, without the fingers,—doubtless there was a sound produced independent of the valves, but not the sound of the heart. I say my proof is legitimate—their assertion a sophism of *non causa pro causa*. It is the tympanic sound of the membranous valves which, with the time of the beats, produces the rhythm; and we judge of the existence of certain states of disease by the degree and manner in which they are out of tune. Looking at the subject physiologically and pathologically, valvular sound is the one we have to depend upon; for granting, for argument's sake, even that any other exists as a normal accompaniment, it has no more to do with the sound than the drone of a bagpipe has to do with the tune.

Ignorant of Dr. Billing's priority, Mr. Bryan thus wrote ("Lancet," January, 1833):—

I shall content myself with offering an explanation of the first sound, founded on a simple law of physics, examples of which are so frequently before us as to have rendered it familiar to every one.

Any flexible solid, suddenly brought from a state of relaxation to a state of tension, vibrates, and its vibrations are sonorous or not, *i. e.* audible or not, according to its physical structure.

At the commencement of the systole of the ventricles, their auricular valves are flapped into play, and at the instant of their closure the whole substance of the ventricles and the valves is suddenly brought to a state of tension, and then consequently vibrates. I leave it to the reader to determine according to the laws of physics whether the vibrations of the valve floating freely in fluid, or the muscular substance of the ventricles trammelled by the contact of surrounding solids, would most contribute to the formation of the first sound.

Notwithstanding the above ably-written theories, the valvular origin of the first sound had very few supporters.

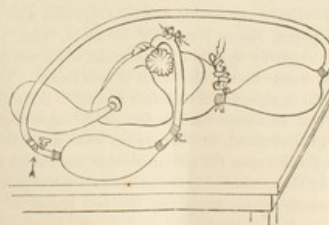
We now come to the experimental part of the inquiry.

In order to prove that *tension* of the auriculo-ventricular valves is sufficient to produce the first sound of the heart, Mr. Brakyn of Dublin devised an ingenious experiment,

the details of which I extract from "The Lancet," November, 24, 1849:—

The apparatus used consists of an ox's heart carefully dissected from the animal, so as to avoid injury to any of the appendages. To this I attached an apparatus consisting of a flanged tube, attached to the middle of the left ventricle, and piercing its wall, introduced through the auriculo-ventricular opening, to which was screwed externally another tube, with a flange also, so as to grasp the wall of the ventricle all round the tube, and render the junction air-tight; to the outer tube a bladder is tied. A free communication is thus established between the bladder externally and the cavity of the ventricle within. To the left auricle, a similar apparatus, but without flanges, was then attached by one trunk of the pulmonary veins, the rest being tied. Then, having tied all the offsets of the aorta, I tied a tube and bladder to its abdominal extremity; to the distal end of this a small stopcock was then tied, into which a brass pipe, fastened to the end of an india-rubber one, can be wedged; the other extremity of the caoutchouc pipe is finally attached to the distal extremity of the auricular bladder. There is thus completed an apparatus, permitting a mimic circulation through the left heart (it being sufficient for illustration), which may be conducted with perfect ease in the following mode:—

Fig. 13.



Let the system be inflated with air through the orifice of the elastic tube next the stop-cock (marked by an arrow in the diagram); when, having wedged back the stop-cock into the pipe and opened the cock, a rhythmical circulation may be carried on by alternating manual pressure applied to each of the three bladders in succession (without removing any of the three hands applied), thus representing the successive contractions of auricle, ventricle, and aorta, with the natural attempts at regurgitation, which close both sets of valves in succession. Hereby a complete imitation of the normal sounds may be produced on either a very magnified or diminished scale, according to the force used in propelling the air.

These sounds, being produced without any muscular contraction or rush of blood, &c. must evidently be valvular, which can be further demonstrated by removing part of the apparatus (the auricular) so as to show the mitral valves in action synchronously with the first sound, or by introducing a wire cage prevent them closing on regurgitation when no sound follows; above all, the first sound is as perfect as the second, the valvular origin of which is I believe undisputed. In fine, the illustration, though conducted with air, ought to be conclusive, inasmuch as a suddenly strained membrane, which gives a tympanic sound in air, will do the same in water also, as I have tried, but not so loudly.

I had the good fortune to witness and assist at this experiment, when a pupil of the Grosvenor Place School of Medicine. Mr. Brakyn, going abroad, left the apparatus with two of the pupils, Messrs. Waters and Sercombe, who, with myself, frequently repeated the experiment, which was quite conclusive as to the valvular origin of both sounds. There being still an objection in the minds of some to this experiment, and an almost total neglect of Billing's original theory, I determined, in 1851, to see what would be the effect of fluids upon the valves; whether the tension produced by their resistance to compressed fluid would produce sound—such sounds resembling the two sounds of the heart.

It is evident that, to determine so delicate a point as the cause of the first sound of the heart, there must not be (as in the experiments of Hope, Williams, and the Committee of the British Association) any rude interference with the mechanism of the heart's action; its cavities must remain untouched; there must be no thrusting fingers into auricle or ventricle (as in theirs); no hooking back of valves (as in theirs); in fact, not one source of sound substituted for another, nor two or more brought together to discover one. My proceedings were as follow.

Large dogs were obtained, and, as in my preceding experiments, the heart was exposed, and the circulation kept up with artificial respiration. A stethoscope being applied to the organ, the sounds were distinctly heard. The superior and inferior vena cava were now compressed with bull-dog forceps, and the pulmonary veins by the finger and thumb; the heart continuing its action, a stethoscope was again applied, and neither first nor second sound was heard. After a short space of time the veins were allowed to pour their contents into both sides of the heart, and both sounds were instantly reproduced. The veins being again compressed, all sound was extinguished, notwithstanding that the heart contracted vigorously. Blood was again let in, and both sounds were restored.

I have thus frequently *interrogated* the same heart for upwards of an hour, and always with the like result.

This experiment (which was, at the time of my first performing it, in 1851, carefully examined, and verified by the late Dr. Marshall Hall and Messrs. Lane and Blenkins; more recently, and during the quiet of the night, by Messrs. Bickersteth, Waters, Fletcher, Grimsdale, M'Cheene, and Webster, of Liverpool; and lastly, for his own especial conviction, by Dr. Quain, of the Brompton Hospital, and

again, more recently, by many others of repute) reduces the origin of the sounds either to the vibrations of the valves or to the rush of blood into and from the heart's cavities. That they need not be referable to the blood (that is, not generated in the fluid) is rendered certain by Brakyn's experiment, in which there was nothing but the *tension* of the valves to produce sound; and I will not admit that the flow of blood either into or out of the heart (in health) can produce sound; for, if such were the fact, sound should be developed during the filling of the auricles, and subsequent injection and distension of the ventricles, that is, during the whole pause, which is not the case. But ascribing, as I do, with Billing, Rouanet, Bryan, Valentin, Baumgarten, and others, the sounds to valvular tension alone, it becomes easily explicable why the first sound should be more prolonged than the second; for, although the tension of the auriculo-ventricular valves is instantaneous, the vibrations of their flaps and of the chordae tendineae, by reason of their unequal sizes and attachments, are continuous; whereas, from the similarity of size and attachment of the semilunar valves, the sound resulting from their tension is limited.

That the blood closes both sets of valves is beyond dispute; that the force of the ventricular contraction is imparted to the blood, and by it transmitted in all directions, must also be admitted. The valves between the auricles and ventricles being, as we have before seen, already in position, their resistance to the ventricular contraction must necessarily be accompanied by extreme tension and vibration of their flaps and tendons (*first sound*). At the same time the semilunar valves give way, and the arterial trunks become greatly distended. Then, the ventricular systole ceasing, this distension of the vessels is overcome by the

elasticity of their coats, which second power, reacting upon the blood, again impels it, its backward progress, however, being effectually impeded by the semilunar valves (*second sound*). Whereas no sound is heard when, as in the last experiment, the blood is prevented entering the heart; inasmuch as the valves cannot be acted upon, the flaps of the auriculo-ventricular valves lying side by side, and the semilunar being immovably closed by the pressure of the blood upon their upper surfaces.

I contend, therefore, that the fact of both sounds being destroyed and reproduced by the same means is the greatest proof that they depend upon the same cause, which is simply the vibrations of the valves produced by the backward pressure of the blood, first against the auriculo-ventricular (*first sound*), and secondly against the ventriculo-arterial valves (*second sound*).

Are there any other means left of confirming my opinions? I crave the reader's attention.

It has been all through stated that valvular tension was the cause of the heart's sounds; that the difference between the first and second sound depended upon the difference in size, shape, and structure of the valves separating the ventricles from the auricles on the one side and from the pulmonary artery and aorta on the other. From this it results that all animals with hearts constructed as our own should give out sounds similar to our own; and upon inquiry we find that it is so, that the sounds heard in the ox, horse, ass, dog, cat, rabbit, &c., differ only in degree, but not at all in character, from the sounds heard in man.

Again it should follow that animals with hearts constructed differently from our own should give out sounds differing from our own; and again upon inquiry we find it is so; for,

In birds there is no perceptible difference between their first and second heart sounds. Two sounds are heard, but they are so similar as to appear like one continuously intermitting sound. And why is this? Because the area of vibration for the production of both first and second sound is about equal, *i. e.*, a bird, in comparison with a mammal, has only half an element for the production of the first sound, only one *tendinous* valve separating the auricles from the ventricles (that being between the left auricle and the left ventricle); whilst in the right ventricle the valve corresponding to the tricuspid of mammals is absent, its place being supplied by a triangular layer of *muscular fibres* connected to the margin of the auriculo-ventricular opening, but which, however accurately it may close the opening during the ventricular systole, must in the production of sound be taken at *nil*.

But in birds, as in mammals, the orifices of the aorta and pulmonary artery are both furnished with three semi-lunar valves, consequently there is no difference between the second sound of the heart of an eagle and of a child, of a fowl and of a dog; but the heart of the eagle and of the fowl, wanting, as it does, the *tendinous* tricuspid of the right side, gives out only half the intensity of the first sound elicited from the heart of the child and of the dog. I repeat, therefore, that the sounds of the heart have their origin in valvular tension alone. Q. E. D.

Fig. 14.

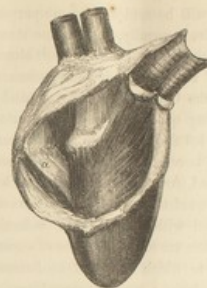
Heart of Bird. Right ventricle laid open to show, *a*, the muscular valve.

Fig. 15.

Left ventricle of heart of Bird, laid open to show, *a*, *a*, the tendinous valve.

But to pass, if possible, to further proof.

There is a Family of Birds, the *Bucconidae*, including the Ostrich and the Emeu, characterised by peculiarities of

structure approximating its members in some degree to the mammalia. It will be sufficient for my purpose to refer to these peculiarities generally, but to the structure of the heart more particularly. In Knight's "Museum of Animated Nature" the Ostrich is thus spoken of:—"The resemblance of the Ostrich in many structural peculiarities to the ruminating quadrupeds was not overlooked by the ancients, which led them to assign to it the name of camel-bird, in allusion to certain points of analogy between it and the camel: indeed, Aristotle asserts the ostrich to be partly bird and partly quadruped, and Pliny observes that it may be almost classed with beasts. The voluminous thighs divested of feathers are more like those of a quadruped than a bird; added to which the bifid hoof-armed foot, well padded beneath, bears a marked resemblance to that of a camel. In this animal there is a large callous pad on the chest, upon which, when reposing, it throws a great portion of the weight of the body. In the ostrich the sternum, which has no keel, but is simply convex and shield-like, is also covered with a callous pad, or elastic cushion, having a hard rough surface unclothed with feathers, and on which the bird rests while reposing. The eyes, with their long lashes and over-hanging brow, are also camel-like. The vast size and sacculated form of the proventriculus, with its extraordinary apparatus of glands for pouring out a solvent fluid capable of reducing the coarsest vegetable aliment, is not to be overlooked. Nor ought we to pass unnoticed the comparatively developed condition of the diaphragm." Added to which, Cuvier says they have "a vast receptacle in which the urine accumulates, as in a bladder."

We are not surprised, then, to find that the heart participates in this gradual approach to the mammalian type. Instead of there being only one muscular valve separating

the right ventricle from the right auricle, there is added a second, partly tendinous and partly muscular,* both of which diverge from a carnea columna springing from the wall of the ventricle. With the exception of this approach to the mammal, the rest of the ventricle is bird-like. Moreover, around the auriculo-ventricular opening there is a more or less distinct zona tendinea. The left ventricle is five or six times thicker than the right; projecting from its walls are three strong muscular masses, from the blunted extremities of which proceed *enormously strong* chordae tendineae, which are attached to the apices, under-surface, and circumference of an equally powerful tendinous valve, disposed somewhat like that called the mitral in the human. The aortic orifice is guarded by three strong semilunar valves, and the pulmonary artery by three others proportionally weak. The auricles approach also the mammalian type, the left having a well-developed appendix full of muscoli pectenati.

What, then, must be the character of the sounds of the heart of the ostrich?

Look at its enormously strong left auriculo-ventricular valve, resisting the pressure of the equally enormously strong ventricle, which has to send its contents to the greatly elevated head and to the far distant toes. I take not into consideration the more or less sonorous semi-tendinous valve in the right ventricle, when I say that the first sound must differ greatly from the second, and from the same sound heard in dissimilar birds. If I tell the reader that the left ventricle of the emeu (the heart being of about

* This valve is usually described as muscular; but, in fact, it consists of tendinous fibres prolonged downwards from the zona tendinea, coated with muscular fibre. Professor Quekett kindly placed a portion of the valve under the microscope, and confirmed my opinion.

the same size as the human) contains a valve and chordæ tendineæ of twice if not thrice the thickness of those in the human subject, he will be prepared to know that the first sound in the ostrich's heart differs little, if at all, from that of the human, neither does the second sound, but that it is, perhaps, less clear. Of course, I cannot describe sounds; but I have listened repeatedly, and for a long time together, to the heart of the ostrich, and find its sounds approximating to those of the mammal; they may be pretty well imitated by *loob-dub*. The heart beats between seventy and eighty times per minute.

One step further into this inquiry. There is a bird, the Apteryx, whose respiration is alone thoracic; whose diaphragm is so developed as to want but a few fibres to make it perfect; none of whose bones are hollow; whose wings are only rudimentary; and whose habits are more mammalian than bird-like. If, then, the respiratory apparatus (with the development of which I have throughout this essay connected the structure of the heart) differs from that of all other birds, its heart must do so too; and, if it be so, the sounds must likewise differ. Let us see.

Professor Owen, in the second volume of "The Zoological Transactions," in describing the anatomy of the apteryx, says: "The principal deviation from the ornithic type of the structure of the heart is presented in the valve at the entry into the right ventricle. This is characterised in birds by its muscularity and its free semi-lunar margin. In the apteryx it is relatively thinner, and in some parts semi-transparent and nearly membranous; a process, moreover, extends from the middle of its free margin, which process is attached by two or three short chordæ tendineæ to the angle between the free and fixed parietes of the ventricle. We perceive in this mode of connection an approach in the

present bird to the mammalian type of structure analogous to that which the ornithorhynchus, among mammalia, offers, in the structure of the same part, to the class of birds: for the right auriculo-ventricular valve in the ornithorhynchus is partly fleshy and partly membranous." By "membranous" Professor Owen means what we human anatomists commonly call tendinous, as will be seen from his following remarks: "There was nothing worthy of note in the left ventricle, or in the valves interposed between it and the left auricle; the two membranous flaps presented the usual inequality of size characteristic of the mitral valve in birds."

Through the kindness of Professor Quekett, of the Royal College of Surgeons, I have been put in the way of seeing the hearts of nearly all animals; among the rest, that of a small specimen of the apteryx; and, in addition to what has been quoted from Professor Owen's paper, I will add, that from the wall of the right ventricle proceed two distinct carnæ columnæ, from which the chordæ tendineæ pass to the tendinous flaps of the valve. The left ventricle contains carnæ columnæ of the mammalian type and disposition. The left auricular appendage is nearly, if not quite, as large as that of a mammal. In fact, here is a bird with a heart nearly identical in structure with that of a child or of a dog. Then its sounds must be like those of the child and of the dog, and differing greatly from those of the eagle. It is even so, as I have ascertained. If any one will listen to the heart of the apteryx, he will almost think it is the heart of his own little child, or of his favourite dog. Its pulse, I should think, is not above 100 when at rest; as with me under a little excitement it varied from 100 to 120.

Above all, let the reader listen first to the heart of the eagle, then to that of the apteryx, and he will find the differences I have attempted to describe, and at once re-

concile them with the teachings I am inculcating, viz. of their valvular origin alone.

But to anticipate a little. (*See below.*) If either "*Bruit musculaire*," "Impulse of the heart's apex against the ribs," "Rush of blood into and out of the heart's cavities," "Collision of blood against the orifices of the aorta and pulmonary artery," or "The concussion between the active and passive portions of the blood," were the cause of the first sound of the heart, why do the sounds produced by the action of the eagle's heart differ from those produced by the action of the child's heart? Blood is passing into and out of either heart in exactly the same manner; auricles and ventricles are contracting with the same rhythm and comparative force. Why then any difference between the first sound of the heart in the one and in the first sound of the heart in the other? Why, simply because the bird has one vibrating auriculo-ventricular valve at the instant of the first sound, whereas the mammal has two. (9).

It would be impossible, within the limits of this essay, to criticise all the views that have been put forth in explanation of the cause or causes of the heart's sounds. Some of the opinions are well worthy of consideration, and can only be put aside by observation and experiments tending to their contradiction,—others are little else than fancies, directly opposed to known physical laws. The assigned causes, I believe, worthy of investigation are:

1st. *Bruit musculaire*; 2nd. Impulse of heart's apex against the ribs; 3rd. Rush of blood into and out of the heart's cavities.

1st. With respect to *bruit musculaire*.—If it and the first sound were identical phenomena resulting from the same cause, viz. muscular contraction, it should follow that in cases of hypertrophy of the heart, in which the cause is

increased, the effect (the first sound) would be increased also; but the reverse is the fact; therefore, they are not identical phenomena.

Again, that they are not identical is proved by demonstration by my last experiment, in which, although muscular action is going on vigorously, no sound whatever is heard. The explanation of the diminished sounds in hypertrophy is that the sounds, being valvular, are less audible through thicker than through thinner ventricles.

Indeed, much, if not all, of what is usually put down as *bruit musculaire* is nothing more, in my opinion, than the vibrations of the compressed air in the tube of the stethoscope, or in the external auditory meatus, produced by the impulsion of the instrument or some other solid against the ear, or by friction between the two; for a low rumbling sound can be produced by pressing a handkerchief or the corner of the pillow against the ear, or by slightly pressing the tragus into the meatus, and another sharper sound is developed by its sudden disengagement on the removal of the finger. On this subject the late Dr. Bellingham has a very good remark; speaking of the so-called *bruit musculaire* heard at night during the contraction of the masseter and temporal muscles, he says that it is only heard by the ear resting on the pillow, and not at all by the other; a fact I have frequently verified, and which is explicable only upon the supposition that the sound results from the contact of the ear with the pillow. Now, all such adventitious sounds may be developed at the time of the ventricular contraction, or beat of the heart, and the more awkward the auscultator, the more are they likely to arise; but that the first sound of the heart is due to muscular contraction, and identical with *bruit musculaire*, is, I think, logically, experimentally, and anatomically disproved.

2ndly. The striking of the heart's apex against the ribs has been given as the cause of the first sound. But, since the apex is not tilted forward, and inasmuch as the blow is only apparent, and is the result of the noiseless change of form of the ventricles within the pericardium; and, further, since the sounds are more audible after the walls of the chest have been removed than before, this at first sight probable cause becomes in fact no cause at all.

3rdly. The passage of the blood into and out of the heart has been assigned as the cause of one or both sounds of the heart.

Several modifications of this cause have been entertained by different authors, as, for instance, "shock of blood against the ventricular walls;" "collision of blood against the orifice of the aorta and pulmonary artery;" "concussion between the active and passive portions of blood;" "rush of blood," &c. (10).

One author's opinions only I shall examine at some length, for the value of one represents pretty nearly the value of the whole.

The theory of the late Dr. O. B. Bellingham, of Dublin, it will suit my purpose best to criticise; first, because his work is of recent publication, and has, I believe, the confidence of a great portion of the Profession; and, secondly, because the views he there sets forth are diametrically opposed to the valvular theory of the origin of the heart's sounds, which it is my present purpose to establish. He says,—

The first sound of the heart we know is synchronous with the ventricular systole; in this act the blood, compressed by the contraction of the powerful muscular walls of the ventricles, is propelled with considerable force into the aorta and pulmonary artery, the sigmoid and semilunar valves of which are suddenly

elevated. In the rapid passage of blood from a wider to a narrower area, there must be considerable friction between this fluid and the parietes of the arterial orifices, quite sufficient, in my mind, to produce the prolonged first sound of the heart. This sound has necessarily a distinct character from the second sound of the heart, because the resistance to be overcome is so much greater, and the passage of blood through these orifices is more gradual; it is likewise more prolonged, because sound must be developed during the entire period that the blood is passing from the ventricles into the large arteries; and the slower the action of the heart the more prolonged will the sounds be.

To this I object, that it asserts Nature to be at fault, which is impossible. There is no such disproportion (in health) between the orifices of the vessels and the cavities from which they spring; they are gradually continuous one into the other; the aorta and pulmonary artery, by virtue of their perfect elasticity, increasing in area as the capacity of the ventricles is diminished, as shown in Fig. 10, page 14. There is not "considerable friction between the blood and the parietes of the arterial orifices," so long as vessels, valves, and ventricles remain healthy. When either the mobility of the valves or the elasticity of the arterial coats becomes impaired, then, I grant, sound may be, and constantly is, produced. But what can exceed in smoothness the lining membrane of the heart and arteries? There cannot, then, be friction, which would presuppose an error in the mechanism—an exception in the Divine Author's works. Again, the heart has sufficient work to propel the blood to the distant capillaries without meeting with obstruction at the instant of its effort. Lastly, where is the "friction between the fluid and the parietes of the arterial orifices" in Brakyn's experiment? and yet the first sound is heard, resulting from valvular tension alone.

Of the second sound he thus speaks:—

The second sound of the heart we know is synchronous with the ventricular diastole. During this act the muscular fibres of the ventricles are relaxed, the cavity of the ventricle enlarges, and the walls of the ventricles re-expand; the curtains of the auriculo-ventricular valves open, and there is a sudden influx of blood from the auricles through the auriculo-ventricular orifices. It is scarcely necessary to say that it is not the contraction of the auricles which propels the blood into the ventricles at this period of the heart's action; nor is the dilatation of the cavities of the ventricles the result of the entrance of blood from the auricles, as some have supposed. It is not, either, necessary for the production of this sound that the diastole of the ventricles should be an active process like the systole; the ventricles being hollow muscles, the state of relaxation of their muscular fibres is a state of dilatation of their cavities; hence a vacuum would be created in them if the auricles were not at this moment full of blood, ready to supply them; but, as the latter had been filling during the whole period of the ventricular systole, this cannot happen, and the blood passes through the auriculo-ventricular orifices in a full and rapid stream, and with sufficient force to generate sound.

According to Bellingham, then, the second sound is produced by the rapid flow of blood from the auricles into the ventricles, and this blood is drawn into the ventricles by the force commonly called suction, owing to the state of relaxation of their fibres, which, he says, "the ventricles being hollow muscles, is a state of dilatation of their cavities."

In the first place, I object to the term "hollow muscles," as likely to mislead and perplex; for the ventricles are never hollow during life, their inner surfaces being either in contact with each other or with the contained blood. Then as to the state of relaxation of their fibres being the state of dilatation of their cavities, see how this is completely disproved by my last experiment, where, the blood

being shut out of the heart's cavities, the heart continues, nevertheless, to act vigorously. Here no dilatation can take place, for how—when the flow of blood through the heart is stopped, and hence all force from within removed—can the ventricular walls expand, how can the ventricular cavities be dilated? It is a physical impossibility (tending to a vacuum); and yet the globular form of contraction and the lengthened form of relaxation are distinctly seen, but no dilatation can take place. The phenomena are as follow: So soon as the ventricles have ceased their contraction, relaxation of their fibres commences. This occurs previous to the dilatation of their cavities, and must not be confounded with it: for relaxation of the muscular fibres of the ventricles has no more to do with dilatation of their cavities, or with the passage of blood into them, than relaxation of the fibres of the sphincter ani has to do with the expulsion of feces from the bowel; yet in both cases the same passive yielding state of the relaxed muscular fibre is necessary to complete the act. As the abdominal muscles, through the medium of the feces, dilate the sphincters, so do the auricles, through the medium of the blood, dilate the ventricles.

Again, the blood does not, as Bellingham says, pass at the time of the second sound in a full and rapid stream into the ventricles. If it were so, it would follow, that, by the time the auricles next contracted, the ventricles, as well as they, would be full; in fact, there would be full auricles and full ventricles by the end of the diastole; and the next contraction of the auricles (which he admits, with every one else, immediately precedes that of the ventricles) would tend to arrest the systemic and pulmonary circulations, forcing the blood back into the venæ cavae and lungs, for it is certain their force could not be exerted on the already distended ventricles; and it is also equally certain that the

auriculo-ventricular valves could not act otherwise than to force back the blood flowing into the auricles upon the venous and pulmonary systems, seeing, as has before been shown (pages 15-18), these structures form a perfect septum between the auricles and ventricles as soon as the latter become distended. In fact, such a heart as Bellingham describes we meet with when the ventriculo-arterial valves are so diseased as to permit of regurgitation; in which cases, the ventricles being imperfectly emptied, the auricles, which have their supply ready to inject into the ventricles, have part of the blood thrown back upon them, and thence into the venæ cavæ and pulmonary veins, producing those distressing symptoms we are too well acquainted with.

Bellingham is not the only one who questions the function of the auricle being to distend the ventricle. There have been from time to time various modifications of this dilating or self-filling power of the ventricle put forth. It will be worth while, then, to devote a few lines to consider attentively what may be the use of the auricle. (11).

There is no one existing, I suppose, who would deny that the blood returns from all parts of the body by the venæ cavæ to the right auricle, and from the lungs by the pulmonary veins to the left auricle.

No one either can deny that in some animals this return of the blood is effected in a shorter time and by a more rapid current than in others.

It should follow, then, if the use of the auricle be to distend the ventricle, that in animals of slow respiration and circulation the auricles would be large, in order that their muscular power might urge on the sluggish stream into the ventricles. By the same rule it should be, that, in animals with quicker respiration and circulation, the auricles should be smaller; and lastly, that in animals with the most highly

developed respiratory and circulatory systems the auricles should be smallest, requiring to give only a little impetus to the already impetuous fluid.

To sum up, I would state that—

The size and strength of the auricles should be in inverse proportion to the rapidity of the circulation and respiration, and to the consequent muscular energy;

and, as far as I am aware, it is so, for—

In the bird, they are small;

In the mammal, they are larger; and

In the reptile, they are largest, being relatively of double the strength and size of the auricles in either other class.

Fig. 16.



Heart of Domestic Pigeon.

Fig. 17.



Heart of Horse.

Fig. 18.



Heart of Snake.

Of what use, then, can the auricles be, unless to distend the ventricles?

Let us now hear Bellingham's objections to the valvular origin of the first sound. He says: "Now the first sound of the heart cannot have its cause exclusively in the sudden tension of the auriculo-ventricular valves, because this act takes place at the very commencement of the systole; and the first sound is a dull, prolonged sound, which persists during the entire systole."

As this same objection was made by the Committee of the British Association appointed to discover the origin of the sounds of the heart, I cannot do better than answer it in the words of Bryan, when combating the conclusions arrived at by that learned body twenty years ago. On what grounds, he asks, is it stated that the cause of the first sound is of equal duration with the sound itself? The sound of a drum is of longer duration than the blow which caused it; a harp-string vibrates long after the finger has struck it; and, though the shock of the sudden tension of the ventricular valves be instantaneous, yet the vibrations excited by the shock of sudden tension may continue." To this I shall only add, that the unequal lengths and sizes both of the flaps of the valves and of the chordæ tendinæ fully account for the prolongation of the sound.

I have thus shown that Bellingham's theory of the heart's sounds is incompatible with the perfect action of the organ, and also that his objection to the theory of the valvular origin of the heart's sounds is founded upon a total mis-apprehension of the subject, and consequently is of no worth whatever.

There is yet another road open by which inquirers may confirm the truth, viz. post-mortem examination. I would suggest that careful notes be taken of cases of morbus cordis, and the sounds heard during life put side by side with the condition of the valves after death. The heart, however, should not be examined as is usually done; it should be tested hydraulically, as in fact it is during life; and then I feel certain we should soon arrive at a comprehension of all morbid sounds (*bruits*), and at a just estimation of their importance. Any London curator giving his attention to this subject would assist the Profession materially. The following method I would recommend:—

Let the heart be removed with an inch or two of the large vessels, then cut away the auricles almost close down to the auriculo-ventricular openings, carefully take away the clots from the ventricles without injuring the delicate chordæ tendinæ, for if even one of them be injured the perfect adaptation of the flaps of the valve is prevented.

First shut down the semilunar valves with a stream of water, (the coronary arteries at their origin must be tied,) then pour water into the ventricles, and the valves, if healthy, will rise upon the surface of the fluid in proportion as the ventricles become filled, and form a perfect septum between them and the auricles. Should the semilunar valves be so diseased as not to permit of closure by fluid, the aorta or pulmonary artery, as the case may be, should be tied, or a cork inserted into it, before the mitral or tricuspid valve is tested.

The above is the mode adopted by myself; others can of course use their own methods.

In conclusion, I would urge upon the student of Medicine the necessity of thoroughly mastering the mechanism of the heart's action, the time and order of the occurrences constituting the rhythm. Only by so doing will he comprehend the morbid conditions he will be called upon to combat, and the sufferings he will be expected to alleviate. To this end the Table at p. 19 has been drawn up, which I hope will prove useful. As a starting point in the study of both functional and organic diseases of the heart, let the student adopt the following simple exposition of the healthy sounds, and all morbid sounds will quickly become intelligible to him.

When listening to the heart, two sounds are heard, the first long, the second short; they may be tolerably well imitated by the sounds *tub, duc*. When the first sound is

heard the ventricles contract, and the auriculo-ventricular valves are made *tense*, and completely separate the cavities of the ventricles from the auricles; the *tension* of these valves produces the first sound. When the second sound is heard the ventricular systole has ceased; the aorta and pulmonary artery have reacted upon their contents; the cavities of the ventricles are separated from the systemic and pulmonary systems by the closure of the semilunar valves, the *tension* of which produces this sound. This is the true mechanism of the valves, and a sufficiently physical explanation of the origin of both sounds, none other, in my opinion, being reconcilable with the teachings of Natural Science.

NOTES.

(1). This systolic impulse has been aptly likened by Billing to the sensation felt when applying the hand to the (masseteric portion of the) cheek whilst the jaws are firmly closed, and the muscles alternately relaxed and put in action, as in mastication.

(2). When the auricular attachment of the ventricle descends, moves forward, and approaches the apex, the left auricle steps forward and downward into the place just occupied by the ventricular base.—Sincox, *loc. cit.*

(3). Speaking of the movements of the heart in the ass, Sibson says that during the ventricular systole the aorta and pulmonary artery "are drawn down about a quarter of an inch by the neighbouring descending fibres."

(4). This experiment was several times tried by Mr. Waters of Liverpool and myself, in order to determine, if possible, whether there was, as stated by Sibson, any "zone of comparative rest" in the ventricular walls; but we could find none. I have since frequently repeated and modified the same experiment, and always with the result stated in the text.

(5). Valentin's Physiology, translated by Dr. Brinton.

(6). Monthly Retrospect of Medical Science (Jan. 1849).

(7). Hunter's quick mind anticipated the remarks in the text, for, speaking of the auriculo-ventricular valves, he says, "They are more complex, requiring an additional apparatus to make them answer the intended purpose; for it is necessary that their loose floating edges should be restrained from inverting themselves into the auricle upon the contraction of the ventricles. This is done by tendons, which are fixed at one extremity along the edge of the valves, and at the other to some part upon the inside of the ventricle. The tendons which are longest are inserted into columns of muscle, the intention of which is very

evident: for, if they had gone the whole length in form of a tendon, they would have been too long when the heart contracted, and the valves in such a case would have allowed of being pushed into the auricles, so far as to admit of the blood escaping back into the cavity; but the carnesæ columnæ keep the valves within the ventricle, in the contracted state of the ventricle."

(8). This we have seen is scarcely correct; the valves are closed before the ventricular systole, which, however, as Billing says, causes them to become tense and produce the first sound. With this slight exception all he says on the subject is perfectly true.

(9). My best thanks are due to Mr. Bartlett of the Zoological Gardens for assistance in listening to the hearts of large reptiles, birds, &c. Mr. Bartlett will corroborate me as to the character of the sounds of the eagle's heart.

It must not be supposed that I allude to the sounds of the heart in a frightened or excited bird; no such thing: Mr. Bartlett selected a powerful Australian eagle (*aquila audax*) that had become perfectly tame and friendly with those to whom he was accustomed, his keeper holding him in his arms, and permitting me to auscultate without fear. Mr. Bartlett explained to me that the bird was naturally too bold to be frightened, and too confident in his keeper to be excited; consequently the heart was acting normally. I listened also at the Gardens for the sounds of the heart in the Python Sabæ and Guinea Rock-snake, as well as to the Alligator (*alligator lucius*) and the Snapping and Edible Turtle, but in no instance could I clearly detect any sound. I shall, however, return to the inquiry.

I may mention, that, before I had listened to the heart of a bird, I predicted to Mr. Bartlett and to a friend what would be the character of its sounds, solely from an examination of its anatomical structure, and the result verified the accuracy of my opinion.

(10). Harvey considered that the passage of the blood produced sound at the time of the ventricular systole; he says, "Ita dum istis cordis motibus fit portio sanguinis et venis in arterias traductio, pulsus feri et exaudiri in pectore contingit."—*De*

Motu Cordis, cap. quintum. He nowhere else alludes to sound, and does not appear to have recognised the second sound.

(11). Harvey, in describing the heart's action, gives the auricle its proper share of the phenomena: he says, "Primum sese contrahit auricula, et in illa contractione, sanguinem contentum in ventriculo cordis conjicit."

Dr. H. M. Macpherson
with the author's permission

ON THE

MORTALITY OF CALCUTTA

DURING THE

TWENTY YEARS ENDING 1860.

BY HUGH M. MACPHERSON,
SURGEON BENGAL ARMY.

ON THE MORTALITY OF CALCUTTA DURING
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31st DECEMBER, 1860.

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No one has yet ascertained with an approach to accuracy what the real population of Calcutta is, or the annual mortality among the varied races of which it is composed. It may indeed be asserted with truth, that there is no city of equal size in any part of the world, the vital statistics of which are so little known. We are aware of the rapid growth of its trade and commerce in recent years; but as respects the corresponding increase which must have taken place in the numbers of the different classes of inhabitants, we are wholly left to conjecture.

So long ago as 1836 Captain H. B. Henderson published, in the 20th Vol. of the Asiatic Society's Journal, a very interesting paper entitled "the results of an inquiry respecting the law of mortality in British India;" and a year or two later Mr. James Prinsep and the committee upon municipal improvements placed on record some valuable tables, quoted in Sir Ranald Martin's well known work on tropical climates, which show in an approximate form the proportions contributed to the total population and total mortality by Europeans, Eurasians, Armenians, Hindoos, Mahomedans, &c., respectively. But the data on which their returns were

founded were very imperfect, and although a careful endeavour was made in 1851, by the then Chief Magistrate, Mr. Elliot, to take an accurate census of Calcutta,—the Hindoo population of which he eventually estimated at 274,506 and the Mahomedan at 111,170,—no further systematic attempt appears to have been made to ascertain the amount and causes of mortality from 1838* until 1857. In the latter year the Municipal Commissioners directed their serious attention to the subject, and commenced giving to the public the valuable though necessarily scanty summaries, which have since appeared in their successive Annual General Reports.

In their endeavours to obtain accurate information, even they have met with many difficulties, owing to ignorance and prejudice on the part of some, and to signal apathy on that of others who should have been better able to appreciate the importance of the inquiry. In their Report for 1860, they write "we are afraid, that our best efforts to submit a complete report upon the mortality of the town have proved unsuccessful," and "with exception of the number of deaths which we consider tolerably correct, the most interesting details connected therewith, are, as far as natives are concerned, entirely wanting, whilst with regard to Christians, they are susceptible of considerable doubt." It is, therefore, little matter for wonder that private individuals, however zealous, have failed to do more than glean from the records of the different hospitals a few details regarding some one or more diseases, the statistics of which they may have been desirous to illustrate.

It is with the view of exposing the discreditable state of ignorance we have hitherto tolerated, on a matter respecting which there exists in most other societies the keenest desire to obtain reliable information, that I have sought to gather

* The India Journal of Medical and Physical Science for September 1851 contains the query:—"Is the climate of India congenial or not to the children of Europeans?" which it answers in a sense very favorable to India.—In 1844 too Lt. Col. Sykes read before the Statist. Section of the British Association a paper on the *Population and Mortality of Calcutta*, based chiefly on a report made by Dr. Strong to the Government of Bengal:—and in 1848 Dr. Cuthbert Finch communicated to the same Society some important remarks, on the *Fatal Statistics of Calcutta* which we shall hereafter allude to. Except in one instance however, neither Dr. Finch's tables nor those of Col. Sykes refer to any season later than 1837-38.

from the registers of mortality of certain classes among us, a few facts likely to interest every member of the community, and the European portion of it in particular. Without seeking for the present to draw positive conclusions,—a very vain attempt with no population returns of any sort to guide us,—the tables here presented seem to admit of deductions being drawn, sufficient to form a basis for comparison with the experiences of past and future seasons, and for testing the efficacy of the measures now in progress or those that may hereafter be devised, to improve the sanitary condition of the town.

It cannot be questioned that there has been a marked improvement in the health of resident Europeans of late years, as contrasted with their condition thirty years ago:—a result attributable, in a measure at least, to the healthier moral tone that has prevailed. But when we come to the non-resident population, we find that a very large mortality is recorded in its ranks from year to year, and that of the deaths among European protestants registered in this city during the last decade, nearly one half occurred among persons who had no fixed occupation or house in Calcutta. The vast majority of the victims were sailors visiting the port; while 76 in each 100 persons who died of cholera belonged to the same class.

Again the blessings of vaccination, in spite of many obstacles, have year by year been slowly but surely diffusing themselves among the native population in the neighbourhood, and the visitations of small pox have been both less frequent and less fatal. Yet, as we shall presently see, nearly 16,000 deaths of Hindoos and Mahomedans from *all causes* are annually reported, and there is too much reason to believe that 17,000 would be a more correct estimate of the number which actually occur. It is high time therefore that more should be done than has yet been attempted, to remove the many preventable causes of disease which are known to exist. But it is only by continuous and careful observation as to the comparative unhealthiness of different localities in the town and harbour, that the real causes of disease, whether among Europeans or natives, can be rightly ascertained or effectually remedied.

As to the data on which the accompanying tables are based, it is right to mention, that those referring to Hindoos and Mahomedans, have been most kindly supplied in detail for each

month of the last twenty years by Mr. Wauchope, Chief Commissioner of Police, to whom I am indebted for much other useful information. Those affecting Europeans have been prepared by myself from the register of deaths of members of the Church of England, from the Scotch Church register, and from that of the Protestant Dissenters, to the custodians of all of which I am much indebted for the ready access afforded to their records. These registers include the deaths of much the largest proportion of the really European population of Calcutta, and are on that account very valuable; the more so that they have apparently been kept with great care.

The numbers given for both natives and Europeans will be found to differ materially from those contained in the annual reports of the Municipal Commissioners.* As regards the natives there is reason to believe, after careful inquiry, that the numbers in the annual reports are not perfectly correct, and that mine are the more accurate of the two. As respects Europeans, the present object is to deal with the mortality of the *civil* population exclusively, whereas other published returns embrace deaths among the soldiers in garrison and the recruits and invalids of the season as well. The Municipal Commissioners' statements have moreover hitherto included still-born children, and all native Christians buried according to the rites of any protestant sect. These I have omitted in the following tables, which are on the whole believed to represent the causes and number of deaths among persons of purely European birth and parentage, more nearly than any non-military ones which have yet been made public.

In addition to this, however, an attempt has been made to separate deaths among residents from those which have occurred in the floating population. The latter term comprises those who had no residence in Calcutta, and were there only temporarily, as sailors, indigo planters, and government servants of the civil or military services on temporary leave of absence from their stations. The residents include all persons whose calling or profession, and abode, were in Calcutta.

The task of making this division will at first sight be supposed to have been an extremely difficult one, but owing to the

* I have especially to acknowledge the kind assistance afforded in many ways by Mr. Turnbull of the M. C. office.

careful entry of the occupations of the deceased generally made in the church registers, it has proved less arduous than was to be expected; and the results obtained have amply compensated me for the time bestowed upon it.

The reason of confining attention as regards Europeans to the Protestant returns for the present, is simply this, that notwithstanding courteous offers of assistance from those who had other records in their charge, it was found that without devoting to the latter a far larger portion of time than it was in my power to afford, it would be impossible to submit them to the same careful analysis which the former have undergone. And considering the object in view, it seemed better to do a portion of work accurately, than to attempt to tabulate a larger number of returns, the accuracy of which could not be vouched for.

It only seems necessary further to premise, that all the native tables refer to Calcutta within the Mahratta ditch,—while the European ones include also deaths in the suburbs of Ballygunge, Alipore, Garden Reach, and Howrah. As to the former, nothing more than an approximation to the truth is presumed to have been reached, yet such an approximation as admits of just comparisons being drawn between one year and another, and one month and another; and the tables seem to give us a tolerably clear conception of the general average proportion of mortality contributed by epidemics, as well as of the relative number of deaths occasioned at different seasons of the year, by specific causes and by all causes combined.

It is to be regretted that it is impossible for the present to contrast, except in the most cursory way, the state of the public health of this city with the results which have been ascertained in regard to the corresponding classes in Bombay and Madras. But those presidencies must be reckoned to be singularly fortunate in being able to command the minute information they have already published respecting their native population. I fear it will take years ere we possess equally detailed statistics in Calcutta; where I shall rejoice, when, without attempting any thing more precise, we are able to boast of a really trustworthy record of the actual number who die in each year, and of their sex, trade, and place of residence, as well as cause of death.

Table A.
Showing the Mortality among Hindoos and Mahomedans from Cholera, Small Pox, and All Other Diseases,
in each of the 10 years from 1841 to 1850 inclusive.

Year.	HINDOOS.					MAHOMEDANS.					TOTAL.				
	Cholera.	Small Pox.	All Other Diseases.	Total.	By Cholera per 1000 of Total Deaths.	Cholera.	Small Pox.	All Other Diseases.	Total.	By Cholera per 1000 of Total Deaths.	Cholera.	Small Pox.	All Other Diseases.	Total.	By Cholera per 1000 of Total Deaths.
1841	3417	24	4896	8297	4135	29	1700	32	1033	3725	4724	85	5177	56	6759
1842	3195	18	4742	8055	4218	17	1350	14	2009	4083	3322	34	6545	32	7441
1843	3571	247	4563	7381	3483	374	1168	88	2109	3395	3471	267	3729	235	6072
1844	4061	1705	4744	10508	3824	1670	1137	2138	5025	3483	2302	4811	2840	6882	15333
1845	4335	28	4672	9038	4796	370	1305	39	2304	4148	4692	94	6240	67	6879
1846	4301	66	4713	9180	4726	61	2126	22	2455	4583	4638	48	6427	78	7178
1847	3971	28	5231	7020	2717	370	870	5	2767	3732	2229	13	3041	33	3278
1848	1459	85	6468	7003	2075	121	1032	22	2305	3409	3032	63	2502	107	2863
1849	2062	1239	6096	9397	2717	1294	1205	485	3133	3883	3257	1349	3867	1734	7239
1850	2324	3270	5940	11534	2535	1024	1100	2249	4443	2374	2010	3348	4420	8109	15977
Total	32327	6098	50918	89943	3593	744	14370	3004	23002	40136	3523	791	46097	9702	73890

Table B.
Showing the Mortality among Hindoos and Mahomedans from Cholera, Small Pox, and All Other Diseases,
in each of the 10 years from 1851 to 1860 inclusive.

Year.	HINDOOS.					MAHOMEDANS.					TOTAL.				
	Cholera.	Small Pox.	All Other Diseases.	Total.	By Cholera per 1000 of Total Deaths.	Cholera.	Small Pox.	All Other Diseases.	Total.	By Cholera per 1000 of Total Deaths.	Cholera.	Small Pox.	All Other Diseases.	Total.	By Cholera per 1000 of Total Deaths.
1851	3889	20	5779	9688	3459	57	1285	12	2072	3369	3814	35	4374	32	7953
1852	3078	46	5841	8965	3373	51	1111	13	1708	2827	3529	45	4189	59	7244
1853	4516	17	5667	10200	4443	136	1086	9	1549	3537	4118	67	5632	19	7215
1854	2169	96	5749	8014	2706	119	913	17	1089	2529	3476	64	3832	113	7448
1855	2704	59	5724	8508	3249	58	950	11	1891	2802	3350	38	3744	61	7045
1856	3158	146	6226	10130	3413	144	1082	32	1818	2932	3690	109	4540	178	8344
1857	2584	2355	6328	11470	2257	2055	1254	819	2392	4455	2855	1843	3838	3177	8890
1858	3653	72	5717	9442	3868	70	1542	61	2558	3651	3992	129	3105	123	8975
1859	3126	44	6026	9796	3191	44	1550	10	2501	4131	3721	54	4976	64	5134
1860	4255	39	6705	10999	3868	35	2298	25	2449	4772	4813	52	6553	64	6144
Total	32732	2888	60092	96002	3389	2078	13671	102	20462	34525	3785	287	45823	2880	51454

Looking at these returns for 20 years, we find that a larger mortality from *all causes* was recorded in 1850 than in any other twelve months: it amounted to 15,977. The least was in 1848, when the number of deaths reached no higher than 10,472,—and the annual average was 13,076.8.

The other seasons most unfavorable to life were, 1857,*
1860,
1844,
And those least unfavorable,..... 1854,
1843,
1847,

such differences, as we shall have occasion to notice hereafter, being due in no slight measure to the prevalence of epidemics.

For the latter 10 years the average number of deaths was 13,115.7, and the difference between the first and second decade would have been still greater, had those who died in hospital in each year, been added. They were probably not fewer than 1200, but their number not being accurately known has not been included in the table.

The mortality from cholera alone amounted in 1860 to 6553, whereas in 1848 it was but 2502, or little more than $\frac{1}{2}$. The years 1842, 1846, and 1845 were, like 1860, very fatal, and 1847, 1854, and 1843 proved in comparison, particularly healthy.

As to the relative degrees in which Hindoos and Mahomedans are liable to the epidemic we have no means of drawing just conclusions, though the conjecture we have hazarded further on is unfavorable to the latter both as regards this and other classes of disease. But of the total number of deaths from *all causes*, the proportion from cholera was nearly equal in the two races during the first period, and slightly higher (40 per 1000) in Mahomedans during the last. It is also worthy of note that during the 10 years ending 1860, the proportion of cholera deaths to total deaths was *less* in Hindoos by 21 per 1000, but *greater* in Mahomedans by 23 per 1000. The ratio of mortality was moreover higher in the latter class than in the former in each year of the decade, excepting 1853.

From small pox the deaths were far more numerous in

* The order in which these as well as all the other years and months specified in this paper are placed, indicates the comparative degree of mortality or otherwise shown in them.

1850 than in any other of the seasons under review. They amounted to 4430, exhibiting a marked contrast to the 17 registered for 1853.

Other years in which the disease proved very destructive were 1857, 1844, and 1849. For 1842, 1851, and 1841 on the other hand the highest number of deaths recorded in any one season is 24, showing how much less common and less virulent the complaint was in those years; and a curious fact, which had been remarked by the Superintendent General of Vaccination in his personal experience, is proved by the returns in my possession, viz: that in every season when the mortality from small pox has been excessive, the disease did not, as at other times, show itself first in January, and after prevailing for the 2 or 3 following months, gradually disappear as the rainy season drew near, but gave unmistakable signs of its approach in the months of November and December preceding, and continued its ravages long after the rains had fairly set in.

The proportion of deaths by small pox to the total mortality, seems to have been nearly the same in Hindoos as in Mahomedans in both decennial periods; but it is satisfactory to observe how much less fatal the disease proved in the second 10 years than in the first—the ratio having been as 3880 to 9702; moreover during that second decade only one very serious epidemic occurred. These facts seem to afford conclusive proof of the real spread of vaccination—thanks in part to the decline of prejudice, but more to the strict personal supervision now exercised over the vaccinators, their proceedings and returns. Something too is perhaps due to the partial introduction among the people of English ideas as to medical treatment, in supersession of the do-nothing system, or worse, of the native Hukeem and Byd.

Casting the eye back on the years in which the mortality was greatest from *all causes*, and comparing them with those in which it was largest from cholera, and from small pox separately—it will be observed that 1860, which was one of the black years in the first list, was marked likewise by the highest mortality from cholera; and that 1850, 1857 and 1844, which show a like painful pre-eminence, were the 3 years out of the whole 20 wherein the largest number of persons fell victims to small pox. The better to demonstrate how much

the total mortality was influenced by epidemics, let us exclude the latter, and we shall then find that 1859, 1860, 1857 and 1856 were the years in which most natives died,—especially Hindoos,—and the years 1843, 1841, 1845, and 1844 were of all the 20, those least unfavorable to life.

We must not overlook the increase, during the last decade, of deaths from non-epidemic causes; an augmentation influenced by the known increase in the amount of population. At the same time, though the number of people who earn their bread in Calcutta is certainly very much larger than it used to be, the proportion of persons who sleep within the Ditch, has not increased in at all the same ratio; and if we could feel sure that Mr. Elliot's estimate in 1851 fairly represented the population as it then stood, we could arrive at a tolerably accurate approximation as to what it is now.

Could we with any certainty assume 280,000 to represent the Hindoo population, and 113,000 to be the actual number of Mahomedans* we should arrive at the following conclusions as to the ratios of deaths to living persons for the year 1860.

1st.—That the deaths from *all causes* amounted in Hindoos to 3.9 per cent or not quite 1 in 25 of the living population; in Mahomedans to 4.22 per cent or little more than 1 in 23½; and in the 2 taken together to 4 per cent or almost 1 in 25:

2nd.—That as regards cholera alone, the mortality was in Hindoos 1.51 per cent or 1 in 66 of the population; in Mahomedans 2.03 per cent or 1 in 49; and in the 2 taken together 1.66 per cent or 1 in 60:

3rd.—That from small pox alone, the deaths in Hindoos were 0.01 per cent of the living, or 1 in 7179½; in Mahomedans 0.02 per cent or 1 in 4520; and in both together 0.01 per cent or 1 in 6140.

4th.—Even calculating the deaths at 17,000, the total rate of mortality would be hardly 4½ per cent or 1 in 23, whereas Dr.

* These numbers do not include the very large portion of the natives who, although they earn their daily bread in Calcutta, do not live within the Mahratta Ditch, but in the immediate suburbs, or in the numerous villages which skirt the other side of the river. They probably amount in the aggregate to considerably upwards of 200,000 persons.

Duncan Stewart reckoned it at 6 per cent or 1 in 15½, for the 4 years ending 1835—the native residents in town being in 1832, according to his returns, 187,081. Dr. Finch states the average annual mortality among the Hindoos during the 12 years to have been 5.6 per cent, and that of the Mahomedans 3.7 per cent; but I cannot help thinking that, as regards the former, even this rate is excessive. It seems to me very difficult to account for so great a difference between the two races. In Calcutta the Hindoos and Mahomedans are, in essentials, one people. Their houses and clothing are very nearly the same; and if the diet of Mussulmans is more varied and nourishing than that of the majority of Hindoos, this advantage on the part of the former is more than counterbalanced by their less cleanly and more dissipated habits.* In the absence of a census, and of an accurate register of deaths, we are necessarily left entirely to conjecture, but I think that my conclusions drawn from the most recent returns, are likely to be nearer the truth than the observations made at earlier dates by Martin, Stewart & Finch. Comparatively favorable as is the ratio which I have deduced, it still represents a mortality twice as great as that of all England,—its large cities included, with their teeming populations of poor and often starving people. It is moreover nearly three times as high as that of sepoy mortality in the native army of Bengal, which on an average of 26 years was 1.39 per cent of strength. Sepoys no doubt are picked men, who are well fed and clothed, and cannot be strictly compared with any civil population composed of old men, women, and children, as well as male adults: still the facts ascertained regarding the deaths among them, make us hesitate to adopt a view of the unhealthiness of Calcutta so very unfavorable as that which has heretofore been entertained.

But whatever may be the proportion of deaths to the living inhabitants, we observe from these tables that there was from year to year the same marked variableness in the number of casualties, which characterises sickness and mortality in tropical countries generally; the deaths from *all causes* having ranged from 10,472 to 15,977; those from cholera from 2592 to 6553; and those from small pox from 17 to 4430.

* The Bombay mortality returns show that in respect to cholera at least the flesh-eating portion of the native community suffer more severely than those who do not eat meat.

Table C.
Showing the Comparative Mortality in different months of the year among Hindus and Mahomedans, from Cholera, Small Pox, and All Other Diseases, in the 10* years ending 31st December 1850.

Months.	HINDOOS.				MAHOMEDANS.				TOTAL.			
	Cholera.	Small Pox.	All Other Diseases.	Total.	Cholera.	Small Pox.	All Other Diseases.	Total.	Cholera.	Small Pox.	All Other Diseases.	Total.
January	2021	576	4414	7011	1102	291	1817	3210	3723	867	6231	10821
February	3613	1125	3040	7817	1301	697	1824	3722	4044	1722	4873	11539
March	2911	1855	2950	8000	1810	865	1556	4231	5721	2090	4508	12917
April	6035	1383	3229	10647	2467	615	1400	4301	8403	1398	4133	14739
May	3216	796	2745	6757	1547	253	1387	3217	4853	989	4132	6884
June	1189	341	2440	3970	721	133	1367	2231	1210	404	3827	6201
July	639	223	3427	4279	298	91	1618	1907	888	204	5675	6207
August	718	45	3993	4756	312	16	1755	2083	90	6097	7231	1443
September	1599	42	4594	6235	522	94	1797	2413	60	6261	8463	9127
October	2071	64	5463	7598	1002	20	2112	3137	3973	74	7578	10738
November	1732	277	6090	7999	1068	133	2573	3769	2790	419	8317	11547
December												

* The returns for 1845 are not included, the detailed record for that year being missing. The Total of the 12 months however is given as above, having been found entered in the book of the succeeding year.

Table D.
Showing the Comparative Mortality in different months of the year among Hindus and Mahomedans, from Cholera, Small Pox, and All Other Diseases, in the 10 years ending 31st December 1860.

Months.	HINDOOS.				MAHOMEDANS.				TOTAL.			
	Cholera.	Small Pox.	All Other Diseases.	Total.	Cholera.	Small Pox.	All Other Diseases.	Total.	Cholera.	Small Pox.	All Other Diseases.	Total.
January	1906	111	5795	7872	765	94	1865	2664	2731	135	7660	10526
February	2002	403	4111	7176	994	112	1650	2756	3568	615	6011	9719
March	4941	827	4230	10078	1796	303	1438	3537	6977	1228	5677	13582
April	6727	755	3777	11259	2983	392	1392	4767	7790	1047	5169	13966
May	4179	371	3724	8274	1659	137	1427	2923	5588	428	5151	11268
June	2497	156	3428	6081	1146	58	1394	2598	3853	214	4622	8618
July	1297	71	4372	5740	817	13	1481	2311	2044	84	5853	7881
August	1049	21	5445	6515	694	12	1712	2418	1544	33	7187	8734
September	1425	11	5317	6753	674	0	1925	2600	11	7775	9312	2146
October	221	7	5011	5239	694	7	1828	2529	2915	14	7739	10668
November	2502	10	7162	9674	1031	2	2168	3191	3333	12	9350	12695
December	2728	45	7051	10424	1058	14	2342	3414	3783	69	9993	13855

By Cholera, per 1000 of Total Deaths. By Small Pox, per 1000 of Total Deaths. By All Other Diseases, per 1000 of Total Deaths.

The next point of interest seems to be to ascertain what proportion of the mortality which we have been considering, occurred in each month of the year, and consequently what months are on an average of years least unfavorable, and what months most unfavorable to life among natives. That subject tables C and D are intended to illustrate. And from them we find that, taking the deaths from *all causes*, the *worst* months were April, March, December, and November, while the *healthiest* were July and June. The difference between the extremes of mortality was very great, in fact about 50 per cent—as is exemplified in the aggregate of deaths in 20 Aprils and 20 Julys, the former months showing 28,735, while the latter show only 14,248.

In respect of cholera the worst months were April, March, May, and February, and the healthiest, August, July, September and October. The difference between the highest and lowest ratios of deaths was still more striking than in the previous case: viz. in 20 Aprils, 16,193, and in 20 Augusts only 2586. It has been supposed by some* that the celebration of the religious ceremonies of the Hindoos and Mahomedans, as at the churruck poojah and mohurru, tends largely to increase the aggregate mortality from cholera. Now extreme dissipation or exhaustion of frame unquestionably predisposes to cholera, or to any other epidemic disease, under all circumstances. But the festivals mentioned happen to occur at the season when cholera is *always* most rife at any rate *among Europeans as well as natives*; the great proportion of deaths observable seems therefore to be coincident with, rather than in any tangible degree dependent upon the religious ceremonies.

It is possible that the new rice which is brought into the market in the months of January and February, tends, in cholera seasons, to encourage a tendency to that disease. It is notorious that all natives consider it unwholesome, and never use it if rice which has been longer in store is any how procurable. The very poor however frequently have no choice but to live upon the inferior sort, and the price even of it is more than double what it was 20 years ago. It is true that wages have risen greatly since the mutinies, but the increase has hardly been in proportion to the rise in the price of food of every kind. The very poor of all classes consequently suffer severely

* Vide Appendix to Maxwell's Practical Treatise on Cholera, 1838.

in times of scarcity, owing to the inferior quality of provisions: the more so that in dear seasons the condiments taken with their food are even more than usually adulterated.

With regard to small pox, the deadliest months were March, April, February, and May, and the healthiest, September, October, November, and August. The variation from the highest to the lowest was very marked, the deaths which in 20 Marches numbered 3918, amounting in 20 Septembers to only 72. That is, the mortality in the latter was not one-fiftieth part as great as in the former.

Lastly if we strike out epidemics altogether, the result shewn is, that in each decade the greatest mortality occurred in December, November, October, and January, and the least in June, May, April and March.

We have seen that April is the month of all the 12 in which the most deaths occur, both from *all causes* and from cholera, and the most fatal April of the 20, was that of 1844, when the deaths from *all causes* amounted to 3148, and from cholera alone, to 1931.

The smallest number from *all causes* in any one month occurred in June 1842, and was 445;

The smallest from cholera alone happened in July 1843, and was 46;

The lowest number from *all causes* was therefore only a little more than $\frac{1}{4}$ of the highest recorded, and from cholera the lowest mortality was only $\frac{1}{4}$ part of the highest from that disease.

One more point must be referred to, with respect to cholera and to small pox. From an analysis of the returns in my possession it appears, that *there was no month in the 20 years in which no deaths from Cholera were registered*. But in the case of small pox, although there was no March, April, or May in the 20 years, and only 1 June, and 1 July, in which no death from the disease occurred; yet in

9 Novembers,
8 Decembers,
7 Januarys,
7 Septembers,
7 Octobers,
5 Augusts,
and 3 Februarys,

of the same period, there was no casualty from small pox.

Table E.

Showing the Total Number of Out-door patients treated in each month of the year, at the Calcutta Native Hospital, and its Branch Dispensaries, from 1851 to 1860, both years inclusive.

	Native Hospital.	Park Street Dispensary.	Gurubhutta Dispensary.	Chitpore Dispensary.	Garden Reach Dispensary.	Total.
January	150839	53204	54026	48591	3555	310215
February	145040	53726	53665	47924	3443	303903
March	163703	62248	64664	54668	4235	349508
April	153090	61555	61300	53150	3955	333050
May	158713	63243	63471	57253	4227	346907
June	158173	61982	60876	57907	4249	349187
July	182380	69967	71907	64219	4602	393075
August	185654	73553	68426	66518	4757	398908
September	171809	67780	61320	60073	4585	365567
October	174461	68141	57578	59162	4685	364027
November	167181	64628	56786	52871	4396	345862
December	151532	54543	53707	47844	3912	311538

A subject for enquiry of hardly less consequence than the rate of mortality, is the amount of sickness prevailing in different months. Having noted those most destructive to life, let us next observe in which of them sickness was generally most rife. Table E. shows the number of persons who sought out-door relief at the Native Hospital and its 4 Branch Dispensaries, during the last 10 years, and is a fair criterion of the comparative degrees of sickness of the various months among the poor of all races and creeds; for the patients attending those institutions comprise a large portion of Europeans and East Indians as well as of natives. We find that the largest number of applications for relief were made in July, August, and September, and in the month of August most of all:—December, January, and February showing the fewest calls for medical aid. The proportion of persons who presented themselves varied considerably in the twelve months, though not perhaps as much as might have been expected. The total in 10 Februarys was 303,903, and in 10 Augusts 398,908, which gives an average range between the highest and lowest number, of 9500 per annum.

From the Meteorological Tables* it will be perceived, that this most sickly month of August is that in which the fall of rain is greatest, and the range of thermometer least; while the mean temperature is between 83 and 84 F. The thermometer therefore stands higher in April, May and June, and its range is then twice as great as in July, August, and September; yet, leaving epidemics out of account, the 3 former months are much less unhealthy than the latter.

Hence the plain inference seems to be that, as others have before remarked, it is the damp and heat combined which prove so prejudicial to health, and to them we owe the malarious emanations so productive of the obstinate Remittent and Intermittent Fevers which undermine the constitution, and frequently terminate in serious hepatic derangements and bowel complaints. In these months more especially are developed congestions of the liver and spleen, which, if not early checked, prove rapidly fatal; or, if less acute, yet surely tend to lower the vital powers of those suffering from them, and, later

* For the information contained in Tables F and H, I am indebted to Baboo Radhanath Suddhar's very useful "Introduction to Indian Meteorology."

Table F.
Register of Rain fall in Calcutta from 1841 to 1860.

Year.	Amount of Rain.	Year.	Amount of Rain.
	Inches.		Inches.
1841	60-25	1851	64-16
1842	76-11	1852	81-41
1843	64-34	1853	52-08
1844	73-86	1854	66-47
1845	62-08	1855	71-46
1846	76-44	1856	64-23
1847	72-36	1857	68-97
1848	58-69	1858	59-76
1849	70-51	1859	68-66
1850	76-27	1860	52-61
Average of 20 Years 66.98 Inches.			

Table G.
Showing the average Temperature and the Fall of Rain in Calcutta, in each month of the 8 years ending
31st December 1860.

Months.	1853.		1854.		1855.		1856.		1857.		1858.		1859.		1860.		Average.		
	Therm.	Rain.	Therm.	Rain.	Therm.	Rain.	Therm.	Rain.	Therm.	Rain.	Therm.	Rain.	Therm.	Rain.	Therm.	Rain.	Therm.	Rain.	
January	62.4	0.10	67.2	Nil	66.4	0.46	68.6	1.06	68.0	Nil	68.1	0.07	69.1	Nil	66.3	Nil	67.1	0.21	
February	74.2	Nil	70.6	1.01	72.5	0.11	72.7	Nil	75.6	Nil	71.6	0.54	74.3	0.23	73.6	0.69	73.8	0.15	
March	82.9	Nil	80.1	1.08	79.2	0.14	80.2	2.23	80.5	0.96	81.1	0.25	78.7	0.28	82.6	Nil	80.6	1.15	
April	86.6	1.00	83.5	7.25	85.3	7.25	85.8	5.07	85.6	0.92	82.3	1.80	82.3	0.97	84.3	1.29	84.9	2.47	
May	80.6	2.4	86.2	3.75	82.8	3.53	82.7	8.56	87.0	0.33	86.0	0.29	86.1	1.18	88.7	2.21	86.0	1.79	
June	89.2	8.48	84.1	16.82	85.6	6.4	83.3	12.57	85.5	10.30	85.2	8.41	83.8	8.57	84.1	6.46	86.0	10.1	
July	84.1	12.78	83.3	10.00	82.3	19.18	82.3	10.94	82.2	12.48	83.8	17.96	85.4	10.69	83.9	17.62	83.5	13.9	
August	82.8	12.44	83.2	11.29	83.7	11.29	82.4	10.30	82.0	18.70	83.6	16.63	82.9	12.21	83.6	14.65	82.9	14.4	
September	83.5	9.15	82.6	9.82	82.7	11.09	83.4	9.02	82.5	13.30	83.9	14.72	82.9	11.55	83.4	11.68	81.1	10.2	
October	81.1	4.94	80.6	4.01	81.2	2.58	81.0	9.21	81.6	1.60	81.3	8.03	81.5	1.86	84.9	1.68	81.9	4.72	
November	73.4	Nil	74.2	0.90	74.3	Nil	75.2	Nil	73.0	Nil	73.8	Nil	74.3	Nil	75.5	Nil	74.5	0.19	
December	69.9	Nil	74.8	Nil	71.1	0.85	Nil	68.5	Nil	77.4	Nil	66.7	1.68	68.7	Nil	2	Nil	68.5	0.99

Table H.

Range of Thermometer in the different months of the year from 1854 to 1859.

Months.	Thermometer.	Months.	Thermometer.
January,	17.9	July,	6.4
February,	17.3	August,	5.2
March,	16.3	September,	6.6
April,	14.7	October,	8.8
May,	13.3	November,	14.2
June,	9.0	December,	16.4

in the year, when the temperature is low at night though the sun is still powerful by day, cause them to fall ready victims to dysentery and to diarrhoea.

Let us next give our consideration to the mortality which has been recorded among Europeans during the past 20 years.

From Returns K and L it appears that the number of deaths in the decades ending 1850 and 1860 respectively, were 3828 and 4893. There was therefore an increase of 1065 in the number of casualties registered during the last ten years, the greater part of which increase in fact occurred during the last five. And if we further contrast the numbers in the 2 periods, we perceive that the whole of that increase had reference to the floating population, among whom the deaths were more than doubled, having risen from 980 to 2195. How this comes to be so, is very intelligible, when we recollect

the augmentation there has been in the quantity of shipping, and consequently in the number of sailors visiting the port,—as well as in the number of other Europeans whom the growing commerce of the place, with the vast railway and other works in progress, has attracted to India.

The tonnage of shipping in 1842-43 was only 266,125, and in 1859-60 was 602,738. We can therefore readily see why the number of deaths has likewise become so much larger, knowing as we do the high proportion that is always contributed to the total mortality by the single class of sailors.

Not only was there no increase in the deaths registered among the resident population; they were actually fewer in the second decade, and amounted only to 2698, whereas they were 2848 in the 10 years ending 1850. There was also a decrease of 13 in the female deaths in the same period. And these results, of which it may at first sight appear difficult to offer a satisfactory explanation, may be accounted for in the following manner.

1st. People live a quieter and more domestic life than formerly, are less given to the excesses of the table, and are otherwise less defiant of the climate. They possess too, better means of preserving health than could be commanded in former years, by any but the wealthiest class.

2nd. The enlarged and still growing facilities for travelling, enable invalids to seek change of climate far more easily, and at an earlier stage of illness, than was formerly possible.

3rd. Children, who here as elsewhere contribute very largely to the general mortality, are not kept in this country to so advanced an age as they used to be. The proportion of Eurasians has probably rather diminished of late years, as the more purely European community has increased. And Europeans in general visit England, taking their children with them, at shorter intervals than persons of the same class were able to do a few years back. So that although there are probably many more very young children in Calcutta than there used to be, there are absolutely fewer who have passed the age of 3 or 4 years. This view of the case seems further to be confirmed by Table V, from which it appears that, though the number of deaths of children under 1 year in the second decade, considerably exceeded those in the first, the reverse was the case among children of all the succeeding ages up to 9.

Table K.

Showing the Mortality among Europeans (Protestants) in each of the 10 years from 1841 to 1850 inclusive.

Years.	Total Deaths.	Males.	Females.	Of the Resident Population.	Of the Floating Population.
1841	356	222	134	285	71
1842	401	265	136	291	110
1843	327	205	122	261	66
1844	416	271	145	337	79
1845	453	318	135	341	112
1846	440	318	122	293	147
1847	342	223	125	266	82
1848	389	222	107	247	82
1849	382	266	116	235	147
1850	376	248	128	292	84
Total.	3828	2558	1270	2848	980

Table L.

Showing the Mortality among Europeans (Protestants) in each of the 10 years from 1851 to 1860 inclusive.

Years.	Total Deaths.	Males.	Females.	Of the Resident Population.	Of the Floating Population.
1851	375	246	129	244	131
1852	376	281	95	208	168
1853	451	342	109	241	210
1854	392	294	98	210	182
1855	524	381	143	277	247
1856	548	436	112	246	302
1857	590	433	157	339	251
1858	638	473	165	350	281
1859	487	365	122	274	213
1860	512	385	127	309	203
Total.	4893	3636	1257	2698	2195

Comparing the 20 years one with another, we notice that 1858 was the worst of the entire series. Of the first 10 years 1845 was the most unfavourable to life. In 1858 the mortality reached as high as 638, the lowest number of deaths in any one year of the 20 being 327, viz. in 1843. And of the last 10 seasons 1851 was the least fatal, the deaths registered being 375. 1857 was, like 1858, a bad year, the deaths rising to 590. And next in order came 1856, with 548 casualties, of which no less than 302, or more than one half, occurred in persons belonging to the floating population. It should be remarked that the tonnage of shipping was greater that season than in 1857, or than in any other year except 1858. But this is not sufficient to account entirely for the unusually high rate of mortality recorded against it. Had the excess of deaths occurred in 1857 or 1858, it would have been less remarkable, because in these years the number of Europeans of all classes visiting Calcutta was, owing to the mutinies, extraordinarily great.

A reference to the Meteorological Tables does not help us out of the difficulty as regards the particular season in question. But we find that, as a general rule, those years which are particularly fatal to natives are also more than usually unfavourable to life in Europeans: the close relation between the two being most noticeable, if we take into account the resident population only. Thus 1858, which produced the highest mortality from *all causes* in Europeans, exhibits the highest mortality from cholera in natives; and 1857 was a very fatal year both among natives and Europeans. In fact, the more closely the subject is examined, the more forcibly it appears, that a high rate of mortality in one class is certain to be accompanied by great mortality in the other,—though the degree may possibly be somewhat less marked, and though the particular form in which the epidemic poison proves fatal to its victims may vary according to the intensity with which it attacks them, their susceptibility to its influence, or the race to which they happen to belong. The data would likewise certainly seem to warrant our believing, that the chances of a season proving unhealthy are greater, when the temperature is higher than usual,—when the rains are late in setting in,—when the fall of rain exceeds or comes short of the average quantity by more than a few inches,—or when several months pass without any rain at all, as not unfrequently happens in the months of November, December, and January.

If we look to the residents only, whose numbers are necessarily less fluctuating than those of the entire population, the year 1858 is still found to be the most unfavorable to life, 1857 coming next to it; and we further notice that the range of mortality in the last 10 years was quite as wide in Europeans as among natives. Taking the season of 1852, when the deaths were only 208, and fewer than in any subsequent year, we find the difference between it and 1858, to have been 142 or 40½ per cent, inasmuch as the deaths in the latter were 350.

Table M shows that May, April, and March are the months of the year in which the largest proportion of deaths occur, May standing foremost if we include the floating population, but April being the most destructive to life if we take residents only into account. We noticed in tables C. and D. that April was the month most unfavourable to life in natives. The fact just stated, therefore, only tends to confirm the truth of the observation made above, as to the intimate relation between the laws of mortality as they affect natives, and as they affect Europeans.

From return N. it will be noticed that the amount of shipping in harbour is usually somewhat greater in May than in April. This however is not sufficient to account for the large number of deaths registered in the former among the floating population. The real explanation is to be found in the height reached by the temperature in May, which is the hottest month in the year, and that in which the direct rays of the sun are most powerful.

The exposure which even in the comparatively cool months produces a large proportion of deaths both on board ship and on shore, proves more and more fatal as the season advances, by causing coup de soleil and apoplexy, as well as cholera. The casualties, which in 10 Januarys were 167, in 10 Mays were 430, *although the tonnage of shipping in harbour was greatly less in the latter than in the former*. Besides the exposure to the sun however, and the excesses committed on shore, both of which prove so dangerous to seamen, the river is at its lowest in May, and the unclouded sun acting on the filth left strewn on the muddy banks at ebb tide,—at those points especially where the great sewers of the town empty their contents into the Hooghly,—generates a malarious poison far more rapidly and in a much more concentrated form

Table M.

Showing the Comparative Mortality in the different months of the year among Europeans (Protestants) during the 20 years ending 31st December, 1860.

Months.	Total Deaths.	Males.	Females.	Of the Resident Population.	Of the Floating Population.
January	569	360	209	402	167
February	598	400	198	412	186
March	796	563	233	506	290
April	897	652	245	576	321
May	970	736	234	540	430
June	752	544	208	451	301
July	728	518	210	468	260
August	747	535	212	486	261
September	689	499	190	432	257
October	665	489	176	407	258
November	655	460	195	429	226
December	655	438	217	437	218

Table N.
Showing the Tonnage of Shipping in the Port of Calcutta on the last day of each month, during the 6 years ending 31st December, 1860.

Months.	1855 Tonnage.	1856 Tonnage.	1857 Tonnage.	1858 Tonnage.	1859 Tonnage.	1860 Tonnage.	Total in 6 years.
January	65,993	118,945	90,349	112,599	157,483	119,968	665,337
February	74,081	102,478	85,093	89,569	132,908	83,807	567,987
March	62,336	100,130	71,084	77,473	106,328	59,263	476,614
April	62,382	97,897	69,063	105,696	104,272	59,788	499,038
May	98,237	94,569	69,327	133,898	97,238	66,669	549,038
June	96,022	103,264	86,407	133,789	91,585	68,006	579,664
July	75,017	115,016	91,667	135,896	92,037	77,618	587,551
August	104,554	123,861	88,330	146,768	116,148	85,804	665,465
September	120,644	124,947	95,464	138,020	115,865	90,946	685,886
October	109,140	126,300	98,330	150,753	188,258	82,943	700,724
November	129,420	139,562	156,920	186,776	125,040	105,379	843,097
December	134,361	128,189	133,935	179,951	116,298	104,396	796,230
Grand total,	1,128,787	1,375,098	1,130,969	1,581,170	1,393,520	1,004,587	

than at other seasons of the year.* The risk from exposure is not of course wholly confined to sailors. But they have to encounter a double danger—their exposure on shore predisposing them to suffer on their return to their ships from the malarious effluvia constantly present around them.

The water† they drink too at that season in such large quantities, being taken directly from the Hooghly, though there is better to be had within reach, is more impure and more deleterious to health owing to the low state of the river before noticed. And still more injurious are the poisonous mixtures always for sale in the bazar, which they swallow with avidity, under the names of beer, ginger-beer and lemonade.

Lastly there is reason to believe that many of the sailors remaining in Calcutta after the 1st May, are men of more than ordinarily idle and dissipated habits. They consist in great part of those who having deserted the ships in which they came out to this country, have lived on in the low haunts open to such persons, until their money and their credit are alike expended. Starving and desperate at last, and with constitutions permanently damaged, they are glad to go on board ship on any terms, and succumb in large numbers before getting out to sea,—the first attack of serious illness, of whatever description, rapidly proving fatal.

As regards the comparative mortality of males and females, if we take the proportion of deaths in the 2 sexes for the whole 20 years in the resident population, we find that nearly 45 per cent (44.82) were females,—43.9 per cent in the 10 years ending 1850, and 45½ per cent in the 10 years ending 1860. Now as European males probably outnumber European females in Calcutta by one-third, at the lowest computation, these figures seem to point to a considerably higher rate of mortality in the latter sex than in the former. And

* My belief in the correctness of these views is strongly confirmed by the following passage taken from a lecture of Dr. Watson, one of the ablest physicians of the day. "For producing malaria it appears to be requisite that there should be a surface capable of absorbing moisture, and that this surface should be flooded and soaked with water and then dried, and the higher the temperature, and the quicker the drying process, the more plentiful, the more virulent (more virulent probably because more plentiful) is the poison that is evolved."

† We here speak of the water as it is drawn up the ship's side in a bucket for immediate use, not as it is after being filtered and purified, or even after the solid matter which it contains has been allowed to subside.

as regards months, the proportion of deaths in females was highest in January, February, and March; and lowest, and nearly equally low, in April, May, and August; the range extending from 51½ per cent in January to 42½ per cent in April.

Thus far we have been able to deal with the *number* only of the deaths in Europeans, and the general inferences to be drawn therefrom. Let us now proceed to consider what the *causes* of death were, and what other information of interest may be gleaned from the more detailed entries introduced in the registers from the 1st April 1856.

We commence with Table O, which shows the causes of death in Europeans (protestants) in each of the 5 years ending 31st December, 1860. The total mortality in that period amounted to 2652 individuals, of whom were
males 1992; females, 660;
residents, 1462; non-residents, 1190; while of the residents there were males, 822; females, 640; and of non-residents males, 1170; females, 20. The deaths among non-resident as compared with resident females were therefore as 1 to 32 only, but the deaths in non-resident males exceeded those in resident males in the proportion of 11 to 1, the former being in excess of the latter in 4 of the columns given in this table.

A glance shows *cholera* to have been more destructive to life, than any other single disease, or class of diseases. It produced 716 casualties, or 1-fourth of all the deaths recorded; and of those 716 fatal cases, 544, or just 76 per cent, occurred among the floating population, viz. 75 per cent in males, and 0.9 per cent in females, while there were only 113 deaths, or 15½ per cent, in male residents of Calcutta, and 59 or 8¼ per cent in female residents.

Dysentery and diarrhoea, both of which are included in the heading *bowel complaint*, come next to cholera, the number of deaths from this cause having been 485, of whom were
males 334; and females 151;
residents 292; and non-residents 193. Of the residents there were males 143; females 149; the number of the latter was therefore positively larger, and, having respect to the number of living women, very much greater than the

Table O.
Showing the Causes of Death in Europeans (Protestants) in each of the 5 years ending 31st December 1860.

Years.	FEBRUARY.		MARCH.		APRIL.		MAY.		JUNE.		JULY.		AUGUST.		SEPTEMBER.		OCTOBER.		NOVEMBER.		DECEMBER.		TOTAL.	
	Resident.	Non-Resident.	Resident.	Non-Resident.	Resident.	Non-Resident.	Resident.	Non-Resident.	Resident.	Non-Resident.	Resident.	Non-Resident.	Resident.	Non-Resident.	Resident.	Non-Resident.	Resident.	Non-Resident.	Resident.	Non-Resident.	Resident.	Non-Resident.	Resident.	Non-Resident.
1856	15	12	20	13	47	13	11	94	35	200	36	12	13	11	13	11	13	11	13	11	13	11	13	11
1857	17	14	24	17	38	22	7	116	23	121	11	10	10	10	10	10	10	10	10	10	10	10	10	10
1858	22	17	30	20	40	25	18	145	28	155	15	12	12	12	12	12	12	12	12	12	12	12	12	12
1859	21	15	25	18	35	20	10	105	25	130	15	10	10	10	10	10	10	10	10	10	10	10	10	10
1860	21	15	25	18	35	20	10	105	25	130	15	10	10	10	10	10	10	10	10	10	10	10	10	10
Total	101	80	134	100	195	121	50	505	127	636	105	62	62	62	62	62	62	62	62	62	62	62	62	62
Total of Class	101	80	134	100	195	121	50	505	127	636	105	62	62	62	62	62	62	62	62	62	62	62	62	62

* This heading includes Delhi, Tumkur, — Camp & Suddi — Apoplexy — Convulsions &c.

† Including 20 Deaths in Childhood.

former. Of the total male deaths too 191, or nearly 57½ per cent, took place in the floating population.

Of 251 deaths from *fever*, 169 were in males, and 82 or nearly ¼ in females;— 181 in residents, and 70 or 27½ per cent only in non-residents; and of residents 101 were males, and 80 or 44 per cent females; a large proportion considering the relative living numbers in the 2 sexes.

With regard to the heading *brain diseases*, (which include, it must be observed, the convulsions of children and fits of all sorts, as well as brain fever of drunkards, sunstroke, and apoplexy) we remark that the total deaths amounted to 290:

viz. males, 213; females, 77.
residents, 194; non-residents, 96, or 33 per cent, and of resident males, 120; resident females, 74, or 38 per cent. Knowing how many sailors die of sunstroke, apoplexy and nervous disorders, from exposure in the sun and drinking, the smallness of the proportion of deaths in non-residents strikes us at first sight as surprising. But our wonder subsides when we recollect that the non-resident deaths do not include those of children, the mortality from convulsions among whom tends so largely to swell the list of casualties from diseases of the nervous system in the resident population.

From *liver disease* the mortality was 101;
viz. males, 79; females, 22;
residents, 69; non-residents, 32 or 31½ per cent; and of resident males, 48; females, 21, or 30½ per cent.—The inference therefore seems to be that liver disease is much less common among sailors &c. than among residents, and, as might be expected, much less so likewise in the female than in the male sex.

The majority of deaths from liver disease in soldiers were shown in No. 1X of this Journal to have occurred in men who had been from 7 to 14 years in India. Except in a few acute cases, the deaths from it in Calcutta have generally happened in persons who had been some years resident, and who were perfectly temperate in their habits; the disease in such cases usually making its approaches in the most insidious form.

From *diseases of the lungs and air passages* the mortality was 197, viz. males, 133, females, 64: residents, 139, non-residents, 58, or 29½ percent; and of resident males, 76, females, 63, or 45½ percent. Though the proportion of deaths under this heading is very small compared with the total from *all causes*, it is perhaps somewhat larger than might have been expected. The numbers have unquestionably been swelled by the deaths of some who came out to India in the vain hope that the warm climate might stay the progress of chest complaints, the seeds of which were already too surely sown in Europe. Some such cases I have myself been able to trace in the registers from which these tables are taken; and the deaths in non-residents have probably been augmented in this way quite as largely, in proportion, as those of the fixed population.

I was anxious to bring prominently into view the degree of mortality occasioned by diseases which are distinctive of a tropical climate, and which are generally believed to be those most fatal to Europeans in Calcutta. But in doing so I regret having left so many other forms of sickness grouped under vague term of *all other diseases*.

The total deaths under this head were 476:

viz. males, 287, females, 189;
residents, 38, non-residents 95;
and of resident males, 196, and resident females, 185.

The numbers among residents are great as compared with non-residents, owing partly to the circumstance that many deaths of children are included in the former, as well as of people of advanced age who suffered from no particular disease,—while none of either of these classes are comprised in the non-resident deaths.

That 57 out of 72 *accidental deaths* or 79 per cent, and 45 out of 64 deaths from *unknown causes* or upwards of 70 per cent, should have taken place in non-residents, is not surprising. And the above analysis of the number under the different headings sufficiently proves what was stated above as to the large contribution made by non-residents, especially of late years, to the aggregate mortality of Calcutta.

But we cannot quit this return without specially observing,
1st How large a proportion of all the deaths in residents during the 5 seasons, occurred in females:
viz. 43½ percent or 1 in every 2¼.

2nd. That in the case of *bowel complaint* the number of female deaths actually exceeded that of males, and, relatively to the presumed proportion of living persons of the 2 sexes, was very much greater.*

3rd That in regard to *fevers, lung diseases, all other diseases, and brain diseases*, the female deaths were to the male deaths respectively as 1 to 2¼, 1 to 2½, 1 to 2, and 1 to 2½.

4th That as concerns *cholera* on the other hand, the deaths in females were much fewer than in males, being only about 1 in 3, from *liver disease* 1 in 3¼, and from *accidental causes*, only 1 in 7½.

These results are confirmed by the percentages given in Table P, which shows concisely, in another form, the classes of disease most fatal severally, to resident males, resident females, and males of the floating population.

We see from it that *lung diseases*, and *fevers* are slightly, the complaints included in *all other diseases* considerably, and *bowel complaint* greatly, more destructive to life in females.* Again *liver diseases*,† *brain diseases*, and *cholera* are all very decidedly more fatal in males; *accidental deaths*, and *deaths from unknown causes* being also more numerous in the latter than in the former.

I have stated in a note that the deaths in *child-bed* were 20 in number, or 3.12 per cent of all the female deaths.

The ratio for all England and Wales as taken from the Registrar General's returns is, on an average of years, about 1½ per cent of the total female deaths. The mortality caused by child-birth, directly or indirectly, seems therefore at first sight twice as high in Calcutta as at home.—Lind, long ago remarked that child-bearing was perilous to European women in Calcutta, and Clark in 1770 mentions that putrid puerperal fever (now happily all but extinct) was extremely common in Bengal.‡

* The ratio of deaths from *bowel complaint* to total deaths in Soldiers' wives is as high as 28½ per cent, in the four years ending 1853-54, but in soldiers themselves it was still higher, viz. 33½ per cent.

† During the period referred to in the above note, nearly 6½ per cent of all the deaths in soldiers were caused by *liver disease*, but under 1 per cent of all the deaths in soldiers' wives. Indian Annals No. VIII.

‡ Quoted by Dr. Chevers in his very valuable treatise "On the Means of Preserving the Health of European Soldiers in India." To the same friend my thanks are due for many useful hints and references connected with this paper.

Table P.

Showing of the Total Deaths from all causes among Europeans (Protestants) noted in Table P., what number per cent were caused by each Class of disease in men and women of the Resident population, and in men of the Floating population respectively.

Classes of Disease.	RESIDENT POPULATION.		FLOATING POPULATION.
	Males.	Females.	Males.
	Total Deaths 822	Total Deaths 640	Total Deaths 1170
	Deaths by each class of disease, per 100 of Total Deaths.	Deaths by each class of disease, per 100 of Total Deaths.	Deaths by each class of disease, per 100 of Total Deaths.
Liver Disease	5.83	3.28	2.64
Lung, &c. Diseases	9.24	9.84	4.86
Fevers	12.28	12.50	5.81
Cholera	13.74	9.21	45.89
Brain Diseases	14.59	11.56	7.94
Bowel Complaint	17.39	23.28	16.32
All other Diseases	23.84	28.90*	7.77
Accidents	1.58	0.31	4.86
Cause Unknown	1.45	1.09	3.84
	100	100	100

* Or, excluding deaths in Child-bed, 25.78.

Many of the causes however which then operated so unfavorably have been removed, and the mortality has unquestionably diminished of late years. But women of all classes have still to struggle against the debilitating effects of this relaxing climate, and it can hardly be wondered at if it is found, that the casualties in childbed are somewhat more numerous than in England. That the difference is not in reality so much in favor of England, will readily be perceived if we reflect how few unmarried and old European women there are in Calcutta, and therefore how much larger a proportion of the total female population child-bearing women constitute, here than at home.

From the Bombay mortuary returns for the five years ending 1860, I find, that the proportion of deaths in childbed to the total female deaths was as follows in different classes of the population of that town.

In Europeans... 5 per 100	Native Christians 1.92 per 100
Indo-Europeans 5.21 " "	Parsees 1.87 " "
Jews 2.4 " "	Mussulmans 1.84 " "
	Brahmins 4.29 " "

If therefore the ratio is high in Calcutta among Europeans and Indo-Europeans, it is still higher in Bombay; and it appears that Jewesses, no doubt as being themselves orientals, suffer much less than Europeans. The wives of native Christians, Parsees and Mussulmans show a mortality only a little higher than is experienced in England; and it is reasonable to suppose that could they command equally skilled attendance, the casualties would be actually fewer than in Europe. Why the mortality should be so much higher in the families of Brahmins than in other classes of natives, I am unable to guess, unless it is attributable to their greater unwillingness to resort to hospitals, or to European medical aid in any shape.

On analyzing the 3rd column, devoted to males of the floating population, we find that of the whole 1170 deaths recorded, as many as 46 per cent, or almost a half were from cholera, while only 13½ per cent of the deaths in resident males, and only 2½ of the resident females were owing to that cause. Such a fact should at once arrest attention, and induce us to institute a careful inquiry as to the cause of this sad state of things, and the best method of remedying it. For it is surely but natural to ask why there should be so fearful a difference between residents and non-residents.

We have already touched upon the causes of disease generally in the floating population; and if we look carefully into the matter we shall find, that admitting the existence of the noxious influences above referred to, the forms in which disease might *a priori* be expected to develop itself with greatest virulence, are precisely those from which the Return tells us that the greatest mortality was produced, viz. *cholera* and *bowel complaint*.

When we think of the life too often led in Calcutta by sailors, and the unfavorable circumstances generally under which they are placed in this port, we see that the digestive and nervous systems must necessarily be sapped, and cease to wonder that year by year so large a proportion of them lay their bones in India.

There can be no reasonable doubt that the excessive mortality among seamen in Calcutta is due in very great measure to their own imprudence, and to their want of knowledge how to take care of themselves. But there is also a very general, and I believe, a well founded impression, that though cases of cholera will occur at times in all parts of the harbour, in spite of every precaution on the part of captains of ships, and of steadiness and sobriety on the part of the crews, certain moorings are more obnoxious to the disease than others. The localities indicated are, Colvin's Ghaut, which is close to the mouth of the great sewer,—Thompson's Ghaut,—Tolly's Nuliah,—the Cooly Bazar,—Fort Point,—and Armenian Ghaut. Having taken some trouble to collect evidence on this head from the Marine authorities, as well as from Dr. Woodford, Police Surgeon, and other medical men who have much to do with the shipping, I have arrived at the conclusion, that no one of the moorings indicated is always infested with cholera; nevertheless, when the complaint is prevalent, it is sure to show itself with special virulence at one or more of these places. They are, in short, the favorite haunts of this most unwelcome visitor. Any of the moorings may remain free from cholera for a year or two, but it is sure to return; and the collective testimony of those who were good enough to favour me with their opinions on the subject, tends to show, that the moorings just below Colvin's Ghaut are, as regards cholera, the worst of any; the ship lying nearest the bank being liable, here as elsewhere, to suffer most severely, as a general rule,

though it occasionally happens that the nearest ship escapes, while the one lying 2nd or 3rd off is violently attacked. At the same time it should be stated, that the disease is often seen in a severe form in other spots than those just named—at the Esplanade moorings, for example, which are usually healthy.

Even ships lying out in the stream are by no means exempt from its ravages. In examining the registers for 1858, I was struck with the unusual number of deaths that occurred in the Sardinian Frigate "*Amazona*," which visited Calcutta that season, and in which alone as many as 22 Protestant seamen were carried off by cholera! On enquiry I find that at first she lay in the stream; but, in consequence of the malignant cholera on board, was brought to the Esplanade moorings, where the mortality still continued so great, that the men had to be taken on shore altogether for a time.

It is not my present object to write directly on sanitary matters. But there are possible remedies for the evils just noticed which it may not be out of place to allude to.

1st. The erection of a good quay all along the river bank would greatly diminish the breadth of surface of dirty mud exposed at low tide, and thus remove a pregnant source of malaria. The many advantages likely to be derived in other ways from such a quay have been demonstrated repeatedly, but its importance in a *sanitary view* appears never to have been sufficiently dwelt upon, though there can be little difference of opinion as to its great value in this latter respect.

2nd. An abundant supply of good wholesome water should be made conveniently and generally available for ships in harbour. Then a question suggests itself as to how this pure water is to be obtained, and is one which may itself give rise to considerable discussion. An improvement on the existing state of things would be, to construct a fine large tank near the river bank, as has been suggested before now. It would however probably be better still to bring the water of the river Damoodah to Calcutta, from the point—only 21½ miles distant—where it approaches nearest to this city. We feel certain that were good and pure water always at command at numerous places along the quay, Hooghly water would soon cease to be used for cooking and drinking purposes. And if to this were added more stringent regulations

than are now enforced in the bazars,* especially with regard to the sale of adulterated articles of food and drink, we should in the course of three or four years have far fewer deaths among the floating European population to record and to deplore. For besides a diminished rate of mortality from disease, as a result of the measures suggested, we should be able to congratulate ourselves on a less formidable number than each year's register at present discloses, of bodies "found floating" in the river.

We have pointed out the individual months in which most deaths from *all causes* are usually found recorded. Table Q is intended to illustrate this point still further, by showing what proportion of deaths during the last 5 years occurred in each month of the twelve, from *fevers*, *bowel complaint*, and *cholera* respectively. And, to begin with the last named, we find from this return, that in ~~four~~ out of the five years the largest number of deaths took place in May, and that in those 5 Mays they amounted to 127, or 39.4 per cent of the deaths from *all causes*; while in 5 Septembers they were only 27 (or little more than $\frac{1}{3}$ of the former), and amounted only to 13.7 per cent of the aggregate deaths in the same month. April was nearly as bad a month as May, the cholera deaths in 5 Aprils being 111; and next in order came the months of March, showing 83, and June, 76 casualties, from the same disease.†

As regards *bowel complaints* again—August was the worst month, contributing 68 deaths; and July was nearly as bad, giving 62; followed by September and October, the former showing 50 deaths, and the latter 48. On the other hand January, February, and March were the months when there were fewest deaths from *diarrhoea* and *dysentery*.

* There is perhaps nothing in all India which reflects more discredit on the British Government than the state of the Loll Bazar. As regards the mere repression of brawling, the police have effected a material improvement: but the open drunkenness in the streets is still scandalous, and the unfortunate sailor nearly as liable as ever to be decoyed into haunts of vice, where he is first plied with drugged liquor, and then fleeced of his property.

† As regards the months most unfavorable to life, the tables quoted by Dr. Finch tend fully to confirm the conclusions which I have drawn from more recent data, both as to Europeans and natives. We observed before that cholera is *always* present among the *Native* population of Calcutta, and the returns in my possession show that in like manner *no month of the years under review was without a death from cholera* in the *European* *Protestant* population. Only one case however is registered in each of the months of August 1851, and October 1857.

Table Q.
Showing the Comparative Mortality from Fevers, Bowel Complaint, and Cholera, among Europeans (Protestants) in the different months of the year, from 1st April 1856 to 31st December 1860.

Months.	Fevers.					Bowel Complaint.					Cholera.					Total Deaths from all Causes.			
	1856	1857	1858	1859	1860	1856	1857	1858	1859	1860	1856	1857	1858	1859	1860		Total.		
January	...	2	3	3	2	10	...	7	2	6	6	21	...	11	5	8	2	26	138
February	...	0	6	2	3	14	...	7	6	3	5	21	...	19	5	10	9	43	157
March	...	2	3	2	2	9	...	12	4	6	9	30	...	20	10	6	36	83	217
April	...	0	6	4	4	25	7	6	10	6	10	38	24	21	18	6	42	111	275
May	...	2	5	6	6	27	12	6	5	14	7	44	31	20	20	23	94	322	792
June	...	0	6	11	6	34	6	6	6	13	1	32	7	16	30	8	55	176	268
July	...	4	4	11	6	29	17	9	12	17	7	62	10	13	27	2	55	208	558
August	...	7	5	9	3	1	25	9	20	19	4	52	10	1	3	22	7	35	221
September	...	7	7	6	3	3	20	5	9	15	5	48	11	5	3	4	27	106	268
October	...	4	2	5	3	1	15	11	5	14	6	42	48	11	1	16	7	35	208
November	...	5	3	8	4	3	22	4	15	5	7	31	33	4	7	10	9	43	500
December	...	2	3	2	5	15	9	8	8	9	3	27	47	13	12	6	4	39	192
Total	...	46	50	52	41	251	106	102	87	87	485	121	145	108	97	105	710	2032	4903

Thanks to the greater abundance and freer use of Quinine,* *fevers* contribute now a days far less to the general mortality than they used to do; and there are, we believe, medical men in Calcutta, in whose practice scarcely a case of fever in an European adult has terminated fatally for many years past.

In the Table before us the largest number of deaths are shewn to have occurred in June, when they were 34, and next in July, and May, when they were 29 and 27 respectively. April, August, and September exhibited nearly similar proportions; and March was the most healthy month of the twelve in this respect, giving a total mortality from *fevers* of only 9 in 4 years.

This comparison of months leads to a similar comparison of *seasons*, and Table R embodies and contrasts the results arrived at on this very interesting and important subject,—both as regards Europeans and natives,—first *including* epidemics, and secondly *excluding* them from the reckoning.

a.—From a reference to the Return it appears that in *Natives*, the healthiest four-monthly period *under all circumstances* was that which embraced July on the one hand, and October on the other, and which I have included in the *rainy* season. The deaths in it were nearly 26 per cent of the total annual mortality.

b.—*Including Epidemics* the mortality during the 20 years may be said to have been precisely the same in the 4 *cold* months extending from November to February, as in the 4 *hot* months from March to June: viz 37 per cent.

c.—But *Excluding Epidemics*, the *cold* months produced a much larger number of deaths than either the *hot* or *rainy* seasons, the first amounting to fully 40 per cent of the whole, while in the *hot* season they were 34½ per cent, and in the *rainy* season only 25½.

Taking the European Protestant population on the other hand, we find that the deaths from *all causes* in the twenty years ending 1860 were thus distributed over the 3 seasons of the year:

* Quinine was a greatly more expensive and less common drug 36 years ago than it is now. My friend Dr. Mactier informs me that the records of the Government Dispensary show it to have cost 4 *annas* per grain in the early part of 1826. The quantity expended in 1825-26 was 8 lbs. 11 oz.;—in 1826-27, 61½ lbs.;—in 1832-33, 97 lbs.;—in 1840-41, 207 lbs.;—in 1850-51, 513½ lbs.; in 1860-61, 1113 lbs., the cost price having fallen at the same time from 90 Rs. to 3 Rs., per ounce.

Table R.
Showing the Comparative Mortality in the Cold, Hot, and Rainy Seasons, among Hindoos and Mahomedans, Europeans (Protestants), and the Garrison of Fort William, inclusive and exclusive of Epidemics.

Months by Seasons.	HINDOOS AND MAHOMEDANS.		EUROPEANS.		Garrison of Fort William.	
	20 years ending 1860.		Protestant Population (Civil).		5 years ending 1860.	
	Including Epidemics.	Excluding Epidemics.	All Causes.	Including Epidemics.	Including Epidemics.	Excluding Epidemics.
Cold (November December January February)	36.80 per ct.	40.14 per ct.	28.40 per ct.	35.90 per ct.	13.14 per ct.	15.60 per ct.
Hot (March April May June)	36.86 per ct.	34.44 per ct.	37.14 per ct.	40.79 per ct.	44.29 per ct.	37.58 per ct.
Rainy (July August September October)	36.33 per ct.	25.20 per ct.	32.43 per ct.	33.29 per ct.	42.56 per ct.	46.80 per ct.

Of each 100 persons who died,
 28 died in the Cold season
 39 " " " Hot "
 33 " " " Rainy "

in other words, the *cold* season was the least unfavorable to life, and then the *rainy* season, the *hot* months producing the largest amount of mortality.

For the last 5 years, however, we are able to separate from the total deaths the numerous casualties due to cholera alone, and if we exclude these last, it appears that in each 100 of the aggregate annual deaths,

24 occurred in the Cold season
 37 " " Hot "
 39 " " Rainy "

This is a somewhat different result from that given by Sir Ranald Martin and others: but there appears to me to be no doubt whatever that *considerably fewer* deaths in Europeans occur in the *cold* weather than at other seasons.

And that this is the case seems clearly demonstrated by the 3rd division of the Table, which relates to the Garrison of Fort William, and which, whether we include cholera or exclude it, shows the deaths in soldiers to have been *greatly fewer* in the *cold* season than during the other periods.

Including Cholera, out of each 100 deaths in soldiers in Fort William, during the five years referred to,

13 occurred in the Cold weather
 44 " " " Hot "
 43 " " " Rainy "

and Excluding Cholera

nearly 16 in the Cold season
 38 " " Hot "
 47 " " Rainy "

Although therefore, it will be observed, as regards the mere proportions, there is some difference between the military and civil returns, they coincide in establishing the main facts, that the *cold* season is in *both* classes the least unfavourable to European life; the *hot* season being the most unfavourable; if we include cholera, but the *rainy* season the most destructive, if cholera be left out of account.

Such being the case, it is sufficiently obvious how vitally important it is to arrange that European troops coming to India, should land in Calcutta in the cold season. And it is no less clearly incumbent upon all on whom the despatch of

troops from India depends, to make such arrangements as shall *under all circumstances* ensure the sailing of transports not later than the 1st of February. The principle has been acknowledged in a general way of late years, but has not been fully carried out in many instances. It has however rarely been neglected without valuable lives being sacrificed in consequence, and ample reason given to regret that any delay, however unavoidable, should have occurred.

As regards natives, in like manner, it has been found, that men embarking in the months of February, March, and April, are much more liable to suffer severely from epidemics on board ship than those leaving Calcutta at other seasons. Various Cooly Emigration Agents have consequently resolved on despatching none after the first of February, and have, we believe, applied to Government to prevent any being sent to sea from Calcutta after that date.

The next four tables show, under different forms, the proportion of deaths at different ages among European Protestants in Calcutta. Had we any knowledge of the numbers living at the corresponding periods, many conclusions of the utmost importance might be deduced from the figures given. Even without a census, the want of which cannot be sufficiently deplored, many interesting points may be eliminated.

From Tables U and V we find that in the last twenty years, out of an aggregate of 8721 deaths, 819, or nearly 1 in each 10½ occurred during the first twelve months, and 1202 or nearly 1 in 7 during the first 2 years of existence, that being the period in which there are most living children in Calcutta, and in which the greatest mortality is observed here as in all other climates and circumstances. In the succeeding periods, from 2 to 5 years, 5 to 10, and 10 to 15 respectively,—the casualties were only a little more than 4, 3, and 1½ per cent of the total deaths. After the age of 15 the ratio quickly rises again, and between that and 20 is 5½; between 20 and 30 it is 22½; and the deaths reach the highest point between 30 and 40—being in that period 24½ per cent.

In the later terms the ratios diminish. There are fewer living Europeans in Calcutta, between the ages of 40 and 50, and the deaths are less numerous in that period. The percentage accordingly descends to 12,—then in the succeeding 10 years to 5½,—in the decade from 60 to 70 to 3½,—and in that from 70 to 80 to 1½. There were as many as 57 deaths in persons of between 80 and 90 years of age; 19 in persons between 90 and 100; and 9 in persons said to be upwards of

Table S.

Showing the number of Deaths at different ages among Europeans (Protestants) during the 10 years ending 31st December 1850.

Deaths at ages.	Of the Resident Population.	Of the Floating Population.	Males.	Females.	Total at each Age.
Under 2 Years,	579	2	323	258	581
2 and under 5 ...	196	2	109	89	198
5 " 10 ...	148	2	70	80	150
10 " 20 ...	214	82	194	102	296
20 " 30 ...	510	318	578	250	828
30 " 40 ...	469	312	598	183	781
40 " 50 ...	338	162	393	107	500
50 " 60 ...	163	33	136	60	196
60 " 70 ...	136	14	74	76	150
70 " 80 ...	46	1	24	23	47
80 " 90 ...	29	1	13	17	30
90 " 100 ...	5	...	3	2	5
Above 100 ...	7	...	...	7	7
Unknown	8	51	54	5	59
Total.	2848	980*	2569	1259	3828

* Of these 17 only were Females.

Table T.

Showing the number of Deaths at different ages among Europeans (Protestants) during the 10 years ending 31st December 1860.

Deaths at ages.	Of the Resident Population.	Of the Floating Population.	Males.	Females.	Total at each Age.
Under 2 years.	609	12	331	290	621
2 and under 5 ...	166	2	86	82	168
5 " 10 ...	125	...	62	63	125
10 " 20 ...	157	160	223	94	317
20 " 30 ...	414	722	913	223	1136
30 " 40 ...	472	873	1159	186	1345
40 " 50 ...	297	249	438	108	546
50 " 60 ...	184	96	210	70	280
60 " 70 ...	142	22	94	70	164
70 " 80 ...	71	1	32	40	72
80 " 90 ...	27	...	10	17	27
90 " 100 ...	14	...	8	6	14
Above 100 ...	2	...	1	1	2
Unknown	18	58	69	7	76
Total.	2698	2195*	3636	1257	4893

* Of these 24 only were Females.

Table U.

Showing the ratio per cent of deaths at different ages to the aggregate deaths, in each class of the European (Protestant) Population, during the 20 years ending 31st December, 1860.

Ages.	PER CENTAGE OF DEATHS AT EACH AGE.				
	Resident Population.	Floating Population.	Males.	Females.	Total.
Under 2 years	21.42	.44	10.53	21.78	13.78
2 and under 5 years	6.52	.12	3.14	6.79	4.19
5 " 10 "	4.92	.06	2.12	5.68	3.15
10 " 20 "	6.68	7.62	6.72	7.79	7.02
20 " 30 "	16.66	32.75	24.02	18.79	22.52
30 " 40 "	16.96	37.32	28.31	14.66	24.37
40 " 50 "	11.44	12.94	13.39	8.54	11.99
50 " 60 "	6.25	4.06	5.57	5.16	5.45
60 " 70 "	5.01	1.13	2.70	5.80	3.60
70 " 80 "	2.10	.06	.90	2.50	1.36
80 " 90 "	1.00	.03	.37	1.35	0.65
90 " 100 "	.34	...	.17	.31	0.21
Above 100 "	.16	...	.01	.31	0.10
Unknown.	.46	3.43	1.98	.47	1.54
Total.	100.	100.	100.	100.	100.

100.* It may be noticed that the number of deaths in people who have passed the age of fourscore, was the same in the decade ending 1850 as in that ending 1860: that of the 9 persons who had passed their hundredth year, 8 were women: and that of the 9 deaths, 7 were recorded in the earlier of the 10-yearly periods under review. The question suggests itself whether,—as the general health has certainly improved of late years,—it may not be that the ages of the 7 old ladies were somewhat overstated; especially as in more than one instance they are mentioned as having been 'about' the time of life given in the register.†

If we view the deaths among the resident population, and floating population, independently of each other, and pursue a like course in regard to males and females, the results are shown to differ in a marked degree from those just stated.

1st, looking at the resident population, the following facts present themselves:—out of 5546 deaths, as many as 1188=21½ per cent, or 1 in 4½, occurred in children under 2 years of age; in the next period 6½ per cent; and between 5 and 10 years of age almost 5 per cent. Between 10 and 20 again the deaths were

6½ per cent,

" 20 & 30 16½ "

" 30 & 40 17 "

but they descend between 40 & 50 to nearly 11½ "

and between 50 & 60 6½ "

2nd. Among the floating population there were but 20 deaths in as many years, in children under 10. But between that age and 20 we find 242 casualties or 7½ per cent,

between 20 & 30 as many as 1040 or 32½ per cent,

" 30 & 40 1185 or 37½ "

" 40 & 50 411 or 13 "

and between 50 & 60 129 or 4. "

As might be expected, only 3 deaths are recorded among the floating population, in persons of upwards of 70 years of age. Of these 2 were between 70 and 80, and 1 between 80 and 90.

It appears from the above, that while in the resident population not quite 34 per cent of all the deaths occurred in

* The number of deaths in persons over 60 was thus not quite 6 per cent of the total:—whereas in London in the last quarter of 1861 it amounted to 20 per cent.

† Among those who had passed their hundredth year, I find the names of Grant, Clark, Little, Patch, Rowe, Thomas, Jones, and DeRozario; of these all but two are stated to have been upwards of 110, and Mrs. Thomas was 125.

persons between the ages of 20 and 40,—in the floating population as many as 2225, or *nearly* of 70 per cent, took place in persons of the same period of life.* This is of course owing to the circumstance that the vast majority of sailors and other casual visitors to Calcutta are men in the prime of life.

3rd. As regards the sex of those who thus succumbed to the climate—6205 were males and 2516 females :—all of the latter, except 41, being residents. As was to be expected from the facts observed in all other countries, we find that there were more deaths in children of both sexes in the first 2 years of life, than in any later period up to 20; the mortality being, as usual, somewhat greater in males up to 5 years old, and rather greater in females from 5 to 10.

a. Of the aggregate deaths in resident males, 10½ per cent were in children under 2; fully 3 per cent in those between 2 and 5; and 2 per cent in children between 5 and 10. From 10 to 20 the mortality amounted to 6½ per cent, " 20 " 30 " 2½ " " 30 " 40 " 2½ " " 40 " 50 " 1½ "

b. As to resident females we remark, that the whole number of deaths was actually (though perhaps not relatively) smaller than in males, and farther that in the former the mortality was more equally distributed over the various periods of life than in the latter.

Though the female mortality in the early years of life, indeed up to 20, was on the whole higher, relatively to the total female deaths, than that of males,

yet between 20 and 30 it was only 1½ per cent, " 30 and 40 " 1½ per cent, and 40 and 50 " 8½ per cent.

Between 50 and 60 the mortality was still somewhat larger in men—but at each period after that age, the great majority of the deaths recorded took place in females. It is quite natural that in both sexes the largest mortality should be noticed between the ages of 20 and 40, the great bulk of the living population, especially of the floating population, belonging to that period of life.

If we contrast the decades to which Tables S and T have

* In London during the last quarter of 1861, 13 per cent of all the deaths were amongst adults of from 20 to 40 years of age, and 19 per cent in those of from 40 to 60.—*Lancet* of January 25th 1862. This forms a remarkable contrast to the figures given in the text.

Table V.

Showing the number of Deaths of European Boys and Girls (Protestants) at different ages up to 15, in the 20 years ending 31st December, 1860.

Ages.	10 Years ending 1850.			10 Years ending, 1860.			Grand Total.
	Boys.	Girls.	Total at each age.	Boys.	Girls.	Total at each age.	
Under 6 months.	125	83	208	156	116	272	480
6 Months and under 1 year.	91	88	179	81	79	160	339
1 Year and Under 2 "	107	87	194	95	94	189	383
2 " 3 "	47	39	86	46	38	84	170
3 " 4 "	37	27	64	23	20	43	107
4 " 5 "	25	23	48	18	23	41	89
5 " 6 "	16	19	35	17	15	32	67
6 " 7 "	20	14	34	15	14	29	63
7 " 8 "	15	14	29	8	11	19	48
8 " 9 "	13	14	27	8	8	16	43
9 " 10 "	6	19	25	14	15	29	54
10 " 11 "	15	10	25	2	7	9	34
11 " 12 "	4	8	12	4	10	14	28
12 " 13 "	17	6	23	5	6	11	34
13 " 14 "	4	7	11	8	5	13	24
14 " 15 "	9	7	16	9	5	14	30
Total.	551	465	1016	509	408	917	1991

reference, we notice that the deaths under 2 years old in the 10 years ending 1860, exceeded by 40 those of the preceding period. They were in both instances more numerous in males than in females, though the difference was less, especially in the last 10 years, than is common in Europe. In children over two years of age, and up to 10, there was a decrease in the number of deaths during the second decade; but between 10 and 20, and 20 and 50, there was a large increase. That increase however was entirely confined to the floating population, for the deaths were actually fewer in the resident population at each period, except between 30-40, when they were exactly equal. Among the floating population the deaths in the period between 10 and 40, or, more properly speaking, between 15 and 40, rose from 82, 318, 312 and 162, to 160, 722, 873, and 249 respectively. That is from 10-40 they were actually twice as numerous, from 40 to 50 once and half as much so, and from 50 to 60 thrice as much so, in the second decade as in the first. The resident population column shews a slight increase in the deaths during the same decade among persons between the ages of 50 and 80; but on the whole there was, as before noticed, an actual decrease in the total number of deaths from 2848 to 2698.

Table V shows the deaths in children of all ages up to 15, and they may all be reckoned among the resident population, except about 30. We find that out of an aggregate of 1991 deaths, $480=24.10$ per cent, or one in every 4, occurred under 6 months; and $339=17.03$ or nearly one in 6, between 6 months and 1 year; so that $819=41.13$ per cent, or one in about every 2½, occurred in the twelve months after birth. We further notice that $383=19.23$ or 1 in every 5½, took place in the 2nd year; 170 between the 2nd and 3rd, and 107 between the 3rd and 4th,—the number continuing to decrease up to the age of 12.

As respects the two decennial periods, there was an increase of 64 in the deaths under 6 months, during the ten years ending 1860, but a decrease of 19 between 6 months and 1 year, and of 5 in the year 1 to 2.—There was also a decrease in all the succeeding periods up to 9. With reference to the sexes, we find 281 deaths of boys, and 199 of girls, under 6 months. The excess of male deaths was thus upwards of 30 per cent of the whole. But in the next 6 months the difference was very much less, being only 172 boys to 167 girls, and, again, between the 1st and 2nd year, 202 to 181.

A problem of the deepest interest to European parents of all classes in this land of exile is the following:—*Are the chances of life for European children better or worse in this country than in Europe, up to the age of 5 years?* People have of late been disposed to think that children for the most part thrive quite as well, if not better, in India, and there is reason to believe that this opinion is correct. But it must be stated that we do not as yet possess data sufficient to settle the question definitively. The Editor of the Calcutta Medical and Physical Journal, in his number of 1st September 1837, before alluded to, does not hesitate to say that the climate of Calcutta is twice as much congenial to children as that of London. Dr. Cornish likewise, in his valuable paper on the causes of death in Madras during 1859,* considers that from Mr. Neison's reports we are justified in concluding that all European children born in India have a greater advantage in the struggle for existence up to the age of 5 years, than they would have in England and Wales. The data however on which the comparisons between Calcutta and London are founded, do not seem to be sufficiently extensive or sufficiently apposite to support the very broad conclusion arrived at, and Colonel Hannyngront† has pointed out, that as regards the Madras Medical Fund Calculations "the sufficiency of the data is questionable. He further finds it highly probable that the returns from which the table was deduced, do not include all the members of each family; in proof of which it is only necessary to state that the births range between the years 1806 and 1838, but the deaths are between 1824 and 1838. Hence it easily appears that in the Madras Table, the mortality of infants is much under-rated." "Compared with other authentic tables this one shows, that while infant life is perhaps more safe in India than in England, the difference is not extreme. Thus, $10,000-7,500=2,500$ is the proportion that die under 5 years of age. This is not far from what would be shewn by the Carlisle table, if the deaths by small-pox were excluded, that formidable disease being now almost extinguished." Even this qualified opinion as to the superior advantages of the climate of India for very young children is reassuring, and with it we may very well rest satisfied until we can obtain

* Madras Medical Journal, July 1861.

† In his letter of 1851, with Appendix, on the state of the Bengal Military Orphan Fund.

more certain information. That can be, only when it shall please the Government to introduce a regular system of Registry, a step which need not cost much money—or, after it has once been set on foot, any great amount of trouble. There are no political considerations to interfere with taking a census, or enforcing a registry of births, marriages, and deaths, as regards the Christian population, and the importance and advantages attaching to such measures are so obvious, that they can hardly be much longer deferred.

Table W.

Showing the percentage of Deaths under 5 years, at each annual period in Calcutta, as compared with children in Europe.*

AGES.	CALCUTTA 1568 DEATHS.			EUROPE.		
	Boys.	Girls.	Total.	Boys.	Girls.	Total.
Under 1 year	30.04	24.71	54.75	31.00	24.76	55.76
1 year & under 2 "	12.04	11.91	23.95	10.37	9.91	20.28
2 " " 3 "	5.83	4.81	10.64	5.63	5.59	11.22
3 " " 4 "	2.91	2.53	5.44	3.72	3.73	7.45
4 " " 5 "	2.28	2.91	5.19	2.66	2.63	5.29
Total,.....	53.10	46.87	100	53.38	46.62	100

In the meanwhile we may compare the percentage of deaths under 5 years at each annual period in Calcutta, with the deaths in England at the corresponding periods, and Table W clearly proves this much at least, that of all who die under 5 years, the casualties at the earliest period are quite as

few here as in Europe. As regards the first year of life the ratios seem slightly in favor of Calcutta, in the second rather in favor of Europe, and in the third and fourth years again in favor of Calcutta. The differences however are trifling, and the results shewn, if corroborated by further observations, would indicate that the season of teething is slightly more trying to children in India, but before and after that period they thrive at least as well in India as in Europe, up to the age of 5.

Although no positive conclusions can be derived from the comparison, we may further note, that in England during the 10 years ending 1854, the proportion of deaths under 1 and from 1 to 5, to total deaths, were 22.8 and 16.5 per cent.*

In France, (in 1853) ... 17.7 and 11.3* "

And in Calcutta, { (on an average } 14.6 and 13.34 "

Again, we find from the *Lancet* of January 25th 1862, that in the last quarter of the year 1861 in London, 38 per cent of all the deaths were in children under 5 years of age, whereas in Calcutta it was but 27.94 of the resident Europeans, in the 20 years under review.

As regards sexes, there were in England in 1849-50, deaths in boys under 1 year, ... 17.1, ... girls 16.4; boys over 1 and under 5, ... 21.0, ... girls 21.5. In Calcutta, boys under 1, 8.07, ... girls 6.5; " " 1 and under 5, 7.08, ... girls 6.25.

All these figures settle nothing, but seem, so far as they go, to be in favor of Calcutta; and more positive testimony in its favor is borne by the low rate of mortality which has been observed in the educational establishments of the place.

It is to be regretted that we have not accurate returns from each of them. Meanwhile the Return of deaths among the children of the European Orphan Asylum, for which I am indebted to the kindness of Dr. Fayer, shews a rate of mortality so low, as could, we believe, with difficulty be paralleled in any similar institution in Europe. Even taking into account how much is due in that excellent Institution to the careful management of the ladies who season after season

* These numbers are taken from the *American Journal of Medical Science*, for April 1859.

Table X.

Showing the Strength and the number of admissions, removals, and deaths, of the children in the European Female Orphan Asylum Circular Road, Calcutta; for 21 years ending 31st Dec. 1861.

Year.	Total Strength.	Admitted.	Removed.	Died.	REMARKS.
1841	52	2	11	...	
1842	43	16	5	2	Ulcerated sore throat.
1843	52	12	6	2	1, Spleen; 1, Dysentery.
1844	56	9	1	1	Tubercular Disease and Hydrocephalus.
1845	62	9	2	1	Pertussis.
1846	63	19	5	2	1, Phthisis; 1, Dysentery.
1847	80	12	11	...	
1848	81	11	8	...	
1849	84	11	6	1	Pertussis.
1850	88	13	10	2	Sisters 1, Phthisis; 1, Typhus Fever, (when out on holiday).
1851	86	9	9	1	Inflammation.
1852	85	7	13	...	
1853	79	4	10	...	
1854	73	10	12	...	
1855	71	9	9	...	
1856	71	4	8	2	1, Jaundice; 1, Convulsions.
1857	65	10	5	...	
1858	70	13	11	2	1, Pertussis Sequela; 1, Cholera, (out of School).
1859	70	4	11	...	
1860	63	22	8	1	Mesenteric Disease and Stomatitis.
1861	74	4	12	...	
Total.	1468	210	173	17	
	200				
	1668	400			
		200			
		17			
		1668			Deaths—=1.019 per cent.

have so generously devoted their time and energies to its supervision,—when we find, in a long series of years, a mortality of little more than 1 per cent among girls of all ages from 2½ to 17, we are forced to conclude, that the Calcutta climate cannot be so hostile to European life as has been supposed. It seems rather to show what Calcutta might be, could we ensure for all classes the abundance of air and unadulterated and suitable food, which the orphan girls enjoy in the asylum afforded them in Circular Road. That they are really strong and healthy children their looks sufficiently testify; and a glance at the column of Remarks in table X shows us, that out of the total 17 deaths in 21 years, 3 were caused by whooping cough, 1 by ulcerated sore throat, and 2 by consumption,—diseases which are certainly not peculiar to India, or ascribable to the unfavorable climate of Calcutta. It appears from a similar table published in 1837, that the mortality in the asylum up to that period had been 1.6 per cent; so that small as it was then, it has diminished instead of increasing, of late years.

Nor is a low rate of mortality confined to the institution named. The medical officers who have charge of the Upper and Lower Military Orphan schools, and of the various other schools in Calcutta, assure me that deaths are of extremely rare occurrence in any of them. But the Return given is especially valuable, because it is believed to be entirely accurate, and it shows, what no other Calcutta return could demonstrate, the rate of mortality among children of purely European parentage. The children in the E.F.O. Asylum have, no doubt, before their admission, passed through the stage of teething, which, as I have already indicated, appears to be the period of infant life to which this climate is least favorable; but on the other hand the return embraces other critical periods of life, so that the results presented are highly satisfactory. I am not however prepared to say that, as a rule, children brought up in this country possess the same energy, either mental or physical, that they would have had, if brought up under equally favourable circumstances in Europe.

These conclusions as to the healthiness of children in Calcutta throw some doubt on the absolute necessity that was supposed to exist of transferring the Lower Orphan school (military) to the Hills; but though the number of deaths can hardly be expected to be smaller, the children will acquire in that cooler climate, as they grow up, greater vigor of constitution than they would have done in the plains.

It would have been most interesting to compare the experiences of the Lawrence Asylums at Sanawur and Aboe with those of Calcutta, but the annual reports, though speaking most highly of the health of the children generally, are not specific enough to admit of positive conclusions being drawn.

A return of suicides in Bombay given in the Mortuary report of that town for 1857, led to my drawing up Table Y, from information most obligingly furnished me by Mr. J. Blessington Roberts, Coroner of Calcutta.

Table Y

Showing Suicides among Natives of Calcutta during the Ten years ending 31st December, 1861.

	MEANS USED.					
	Submersion.	Fire Arms.	Cutting Instruments.	Hanging.	Poisoning.	Not Mentioned.
Natives, Calcutta	0	0	51	102	61	61
All classes, Bombay	110	13	15	14	46	*1
						199

* This man precipitated himself into a quarry.

It will be seen that the means of destruction used in Calcutta are very different from those employed in Bombay. In Calcutta the manner of death was unfortunately not specified at the time, in as many as 61 cases out of 275. There is however no reason to suppose that they would have affected the general ratios indicated in the other columns. We find that in this City, *hanging* is by far the most common mode of committing suicide, *poisoning* coming next, and lastly *throat-cutting*: whereas in Bombay *submersion* is most usual, and next to it *poisoning*.

We believe that in Bengal generally, as in Bombay, a

large proportion of the total suicides occur by submersion; and I am unable to account for the different fashion which prevails in town—and we know that fashion exercises a most powerful influence on suicides in all countries—except on the ground that there are fewer tanks or pools of water in Calcutta than in the Mofussil, where privacy could be secured for the perpetration of the act.

It will not fail to be observed, that in no case were fire-arms used by Bengalees for their self-destruction. This is exactly what was to be expected.

In M. Quetelet's very curious and interesting Treatise on Man it is stated, that in France, in the Department of the Seine, during the 9 years ending 1825, submersion was by much the favorite mode of suicide, and next to it fire-arms which were largely employed, and after them asphyxia—a means of suicide unknown in India. The least popular means of all was poisoning.

It is not improbable that other methods are now the fashion in Paris.

With regard to the suicides in Calcutta—the largest number occurred in the months of January, May, July, and October, during the ten years under review; and, as regards season, the *Rainy* weather showed slightly the largest proportion—as was also the case in Bombay.

Before concluding these remarks, I would only further allude to the rates of mortality from all causes among natives in Madras and Bombay, as compared with Calcutta. Table Z shows that they varied much, in the three Presidency towns, and that both in Madras and Bombay there were fewer deaths from cholera, but more casualties from small pox, than in Calcutta.

Taking Hindoos and Mahomedans together, the proportion of deaths from cholera was nearly equal in the two former cities, and in neither was it one-third as high as in Calcutta. From small pox however the mortality was 6½ per 1000 greater in Bombay, and 17½ per 1000 greater in Madras, than in this town.

If we deal with Hindoos alone, the deaths from cholera in Calcutta were four times as numerous as in Madras, and nearly three times as numerous as in Bombay: while the mortality from small pox, which amounted here to 29½ per 1000 of the total deaths, was 33½ per 1000 in Bombay, and nearly 52 per 1000 in Madras.

Table Z.
Showing the proportion of deaths borne by Cholera and Small Pox to the Total Deaths among
Hindoo and Mahomedans in Madras in the year 1859, as compared with an average
of 5 years in Bombay, and of 10 years in Calcutta.

	CALCUTTA AVERAGE OF 10 YEARS.			MADRAS FOR THE YEAR 1859.			BOMBAY AVERAGE OF 5 YEARS.		
	Total Deaths.	Proportion by Cholera.	Proportion by Small Pox.	Total Deaths.	Proportion by Cholera.	Proportion by Small Pox.	Total Deaths.	Proportion by Cholera.	Proportion by Small Pox.
Hindoo.	96032	338.9 per 1000	29.8 per 1000	8120	83.2 per 1000	31.9 per 1000	97884	118.7 per 1000	33.3 per 1000
Mahomedans.	34225	378.3 per 1000	28.7 per 1000	1828	20.7 per 1000	23.5 per 1000	54456	74.8 per 1000	42.7 per 1000
Hindoo and Mahomedans Combined.	131157	349.3 per 1000	29.5 per 1000	9948	103.9 per 1000	40.7 per 1000	152340	107.3 per 1000	38.8 per 1000

At Madras cholera was $6\frac{1}{2}$ times more fatal in Mahomedans than in Hindoos, but in Bombay only 1 Mahomedan died of the disease to $1\frac{1}{2}$ among the Hindoos: and the Madras deaths from this complaint, which were 207 per 1000 of the aggregate mortality, were 378 $\frac{1}{2}$ in Calcutta, and only 42 $\frac{1}{2}$ per 1000 in Bombay.

On the whole, therefore, whereas in Calcutta, Hindoos suffered considerably less from cholera than Mahomedans, but rather more from small pox, in Madras they suffered greatly less from the first cause, and greatly more from the second. In Bombay on the other hand Hindoos suffered most from cholera, and least from small pox. We further notice, that while in Calcutta cholera is *always* epidemic in March, April and May,* it is not so in Bombay. There, Dr. Leith tells us,† "*there are no regular annual times of maxima and minima*, and in looking at the returns of the successive years, the outbreaks or exasperations of the disease, as indicated by the deaths, are seen to have taken place at apparently irregular periods."

Small pox, however, in Bombay as in Calcutta, produces the highest rate of mortality in the months of March and April.

The form of the Bombay Mortuary Returns, minute and valuable as they are, unfortunately prevents us from contrasting the deaths among Europeans in that City with those which occur here. They moreover include in one list, soldiers, sailors &c. as well as more permanent residents; therefore no comparison of much value can be instituted between them and the Tables which I have submitted in my present paper.

* Although here cholera is always epidemic in these months, such is not the case in the Bengal Presidency generally,—nor even at places in the immediate neighbourhood of Calcutta. At Dum Dum for instance—formerly the Head Quarters of the Bengal Artillery, and only 7 miles distant from Government House—in most years only a few sporadic cases occur throughout the twelve months. But at times the disease breaks out with uncommon virulence, wholly irrespective of the season of the year, and when it is by no means prevalent in Calcutta. By such an epidemic in August 1859, one-sixth of a whole detachment of recruits with their families, were carried off in 10 days, the pestilence confining itself almost entirely to their barracks, and disappearing as suddenly as it had come upon us.

† Mortuary Report of Bombay, for 1857.

Conclusion.

The general results of this examination of the registers of mortality lead us to believe, that Calcutta is less unhealthy for European residents than has generally been supposed. But though the conclusions suggested coincide with the personal experience and observation of some of the oldest inhabitants, our present means of judging are so imperfect, that we cannot venture to state anything with certainty. I only hope that the various points touched upon, whether as regards Europeans or Natives, will provoke discussion, and lead to further enquiry; and that the effect will be our learning with accuracy the number of living persons as well as of deaths,—in short the relative value of life—among individuals of different castes and races in Calcutta.

Much may be hoped from the accession to the Board of Municipal Commissioners of a medical man of the known energy of the officer recently appointed, and still more may be looked for from the new Municipal Act which is now before the Legislative Council of Bengal. It will, we trust, confer upon the Commissioners full powers to enforce by penalties many regulations which the public looked to them to introduce, but which they have heretofore possessed no real authority to carry into execution.

CLINICAL NOTES.

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FIVE YEARS' HOSPITAL EXPERIENCE OF PNEUMONIA.

IN two clinical lectures, published in the *Edinburgh Medical Journal* for May 1856, and devoted to a retrospect of cases treated during the preceding winter session, I advanced certain opinions in relation to pneumonia, and the acute diseases of the chest generally, which further experience now enables me to confirm. These opinions, if correct, are very important in their bearing on practice; and I shall, therefore, accompany a short statement of the facts which I have now to adduce by such an amount of commentary as may enable their bearings to be clearly understood.

While acting as pathologist to the Royal Infirmary from 1848 to 1853, I had become convinced of what I had, indeed, suspected long before—that the tendency to death of many inflammatory diseases, and, in particular, of inflammation of the lungs and pleura, when uncomplicated, was very much overrated by those who had formed their ideas upon the received doctrines of the schools. This I inferred from the rare occurrence of deaths due to pneumonia and pleurisy (and, I may add, to pericarditis, peritonitis, and acute meningitis), apart from those organic diseases or surgical accidents, which might be said naturally to terminate in one or other of these acute affections. I also inferred, from my experience as pathologist, that this tendency of inflammations to a favourable result was on the increase; or rather (to put the precise character of my convictions in a clearer light), that, in proportion as we were getting rid of the severe forms of epidemic disease (fever, dysentery, scurvy, influenza), which had deteriorated the health of the population previously to 1848, we were also getting rid of the more severe and unmanageable types of acute inflammation; especially the inflammations of the serous membranes of the chest and abdomen, which I had seen in fearful activity during a few months of the year 1847, and pneumonia, which, within my

own experience as a student, had been a much more fatal disease in hospital practice than it ever became after 1848.

Although it was reasonable to ascribe part of the diminished mortality of inflammations under treatment to improvements in treatment, it did not appear to me possible that the whole, or even the greater part, of the change could have been thus brought about,—*1stly*, Because there were various principles of treatment in operation within the Infirmary itself, whereas the change was not in any one set of wards, but over the whole institution. *2dly*, Because many of the cases admitted to the Infirmary were only admitted at very late stages of their disease, when treatment, however good, could be of little avail, and when active treatment was out of the question. *3dly*, Because I had myself seen, in connection with the epidemics of 1846–48 (during which time I had acted as resident physician), a large number of cases of inflammatory disease, which I was convinced would have yielded to no treatment, and which, in fact, proved fatal with very little opportunity for treatment beyond stimulation. These cases had made a deep impression on my mind, and had convinced me that inflammatory diseases, like fevers, were to a great extent subject to unknown epidemic causes of increase and diminution, both as regards frequency and severity. This doctrine I see daily more and more reason to believe well-founded. I have, in fact, no doubt whatever, that, during the last twelve or thirteen years, a very great diminution has taken place in the intensity, as well as the frequency of inflammations generally,—a change corresponding in all respects with that which I have elsewhere noticed (*Edin. Med. Journal*, July 1859, p. 47, *et seq.*) as having occurred in regard to typhus fever.

Into the causes of this favourable change I do not propose at present minutely to inquire, although the inquiry is one of great importance. I cannot, however, refrain from stating my belief (even if it should appear at present unwarranted by precise facts), that the acute inflammations are quite as much, or very nearly as much, within the domain of the sanitary reformer as the more obviously epidemic fevers; and further, that some even of the chronic organic diseases have already yielded, and may be expected still further to yield, to the improved habits, the better clothing, the greater abundance of food, and the diminished destitution of the population generally. Looking back over thirteen years of almost continuous hospital experience, I feel assured that chronic affections of the kidney and liver, and perhaps also of the heart, have diminished in frequency, as causes of death, to a most material extent. Let us hope that these changes in the right direction will become even more apparent than now, and that they will be rendered permanent as regards future generations.

In the clinical lectures alluded to at the beginning of this article, founded upon an analysis of the cases under treatment for four months of the winter session 1855–6, I remarked “that, out of the

whole deaths, twenty-two in number, there have been *four* only due to what can be fairly called acute disease; or, excluding two deaths from fever, *there have been not more than two deaths out of twenty referrible to acute inflammations.*” I further showed that, with the exception of these two (both well marked cases of pyæmia, one from erysipelas with gangrene, the other from a fish-bone lodged behind the œsophagus), “inflammations of the lungs and pleura have not been, directly or indirectly, fatal in our wards, in a single instance, during the present session.”

I propose, in this paper, to illustrate still further the actual state of the case as regards pneumonia, or rather pleuro-pneumonia, by submitting a brief report of the mortality in my wards, in so far as it bears on this disease, during five complete years ending in December 1859. There is only one way of doing this so as not to mislead; viz., to give an account of all the deaths, whether directly from pneumonia or not, in which decided pneumonia formed part of the disease existing at the time of death. All these cases, so far as I can discover them, will be here found. I have to add two remarks in the way of caution. To use these data as statistics of death and cure, to be set against other statistics of death and cure, possibly founded on a different estimate of pneumonia, will be to make an unguarded and, I think, an unscientific use of them. Further, to argue from these data as confirmatory of any special principle of treatment in pneumonia, will be wrong; inasmuch as my treatment, though I may venture to hope it has not been a bad treatment in fact, has not been at any time founded on special or peculiar principles; but simply on the watchful adaptation of means to ends in the individual case. Indeed, in our hospital, freely admitting, as we do, cases in all stages of disease, often utterly neglected before admission, and, when not neglected, treated in every conceivable manner, it is evident that we must have the most ample opportunities of seeing what nature does and can do, either unassisted by art, or, in some instances, worse than unassisted. It should be understood, too, as respects the ordinary physicians of the hospital, that though they have a limited power of selection (which is always exercised in favour of urgent cases by preference), they have practically none of that power of rejection of cases supposed to be unsuitable, which is, by custom, allowed to a large extent in certain other wards. It may fairly be assumed, therefore, that, if the Edinburgh Infirmary has admitted many of the most urgent cases of pneumonia from among the poorer classes of the community, not the least desperate, nor the least neglected, of these cases have passed through my hands as an ordinary physician.

For several years before 1855, while I was occupied as assistant-physician and junior acting physician to the Infirmary, the cases under my personal care were, to a disproportionate extent, instances of chronic disease, and were limited to the male sex. Since that period, owing to arrangements among the ordinary physicians with

regard to clinical teaching, into which it is unnecessary to enter here at large, I have never had under my care fewer than fifty beds fairly divided between male and female patients; all the male, and by much the greater proportion of female, beds having been devoted to ordinary miscellaneous cases, most of them severe and urgent; and the succession of these having been, as a rule, fully more rapid and varied than in the wards of the other ordinary physicians, especially during the winter months. I shall, therefore, record my experience only from the year 1855, taking in so much only of the period preceding the changes referred to as is necessary to complete that year, and make a five years' survey from 1855 to the end of 1859. I believe, however, that my experience before 1855 was exactly in accordance with that presently to be related, in respect to the point now under discussion.

Here, then, is my entire personal experience of fatal acute inflammation of the lungs, whether simple or complicated, during these five years.¹

In 1855, the only case of this kind that occurred to me was one of double pleurisy, with pyæmic abscesses in the lung, from a fish-bone imbedded in the cellular tissue behind the œsophagus. This case is one of great interest, and is recorded with some detail in the clinical lecture above referred to. It was originally admitted to the female clinical ward, and was afterwards sent up to my ward by the resident physician, under the impression that it was a case of fever, the patient being at the time in a state of great exhaustion. The girl, a respectable domestic servant, had been under treatment outside the hospital, and the fish-bone was supposed by herself, and by the surgeon who attended her, to have been removed.

In 1856, I find three fatal cases of pulmonary inflammation; all, however, evidently secondary to grave disorders, sufficient of themselves to compromise life, and of older standing than the pneumonic affection. One of these patients, John R., æt. 40, was said to have been an epileptic; he had fallen (it was said) during a fit, and had injured his head; he lived for eight days with all the signs of compression of the brain, and was afterwards found (as was suspected before death) to have fractured the skull in the lateral regions and at the base. The pneumonia in this case presented hardly any symptoms. In another case, Robert S., æt. 43 (like the preceding, admitted to the ward for noisy patients, of which I had then the charge), there was severe maniacal delirium tremens, which was afterwards discovered to be complicated with fracture of the first rib, diffused abscess around the seat of the fracture, suppuration

¹ To ensure accuracy and completeness, I have not only searched carefully through the ward-books, but also through Dr. Haldane's registers of the Pathological Department; and further, I have had a complete list made out by Mr. Welsh (to whom I would express my obligations), of all the fatal cases of pneumonia, pleuro-pneumonia, or broncho-pneumonia, simple and complicated (in all, 40 cases), returned in the statistical register of the entire hospital from 1855 to the end of 1859.

of the shoulder-joint, and pyæmia. This case is noticed in the *Edinburgh Medical Journal* for August 1856, p. 129. The third case, Laurence C., though nominally under my care, was treated during my absence from town, in Ward 6. It was one of broncho-pneumonia supervening on Bright's disease of the kidney.

In 1857 there were two fatal cases of pneumonia, both secondary to chronic organic disease. The first was only two days in hospital. He was an intemperate man, John S., the subject of Bright's disease, and, as was afterwards found, of fatty liver in a high degree. He was admitted to Ward 10, on the 1st of January, far gone in double pneumonia, and with intense albuminuria and renal desquamation. He died on the 3d. The other case was that of Patrick F., æt. 39, subject to aggravated chronic bronchitis and emphysema for nearly a year before his death, which occurred from the complication of these disorders with a rather chronic condensation of the lower lobe of the left lung. He died on 8th April (Ward 4).

In 1858, there were two cases of pulmonary inflammation under treatment, and ending in death, very much resembling those recorded above. One, Anne S., æt. 39 (died 16th October, Ward 16), was complicated with Bright's disease of the kidney, in an advanced stage. The other, James B., æt. 30 (died 5th November, Ward 4), was a case of emphysema and bronchitis of old standing, on which pneumonia had supervened about a week before admission. He was five days under treatment. I must also, for the sake of completeness, allude to the case of a child of 15 months, apparently of feeble organization from birth, which was brought to me evidently moribund on the 8th of July. As the mother was greatly distressed, and wished something to be done, I sent her up stairs to a ward to warm the child, and to administer a little wine. It died, however, two hours after admission, and considerable inflammation of the lungs was found by Dr. Haldane on a post-mortem examination.

In 1859, I witnessed, within the space of little more than a week, two more fatal cases of pneumonia, one of which only was complicated. Neither of these cases was under treatment more than forty-eight hours. The complicated case was one of severe and long-standing emphysema and bronchitis, with cardiac dilatation to a moderate degree, admitted in *extremis* on the 4th June, and dying on the 5th June (Ward 15, Bridget D.). The other case was that of a man of excessively intemperate habits (Thomas M.C., æt. 55), admitted into Ward 4 on the 27th May, with absolutely complete hepatization of the upper lobe of the right lung, which was so much enlarged by inflammatory effusion as to cross the middle line of the sternum, and was already becoming disintegrated by suppuration, with the characteristic prune-juice expectoration in very large quantity. The patient had been ill only about a week, but was exceedingly exhausted on admission. The treatment was almost exclusively by stimulants; but he died on the second day after entering the hospital, and the ninth of the disease. There was no organic complication.

The preceding details show, that out of ten or eleven cases of inflammatory affections of the lungs, being absolutely the whole number that can be discovered after the most diligent search, in which acute pneumonia, or anything like acute pneumonia, occurred among all the cases terminating in death under my care from 1855 to 1859, only one can fairly be said to be a death from idiopathic or uncomplicated pneumonia. Further, that in this one case the disease occurred in a man of excessively intemperate habits, and had gained ground so far before admission that the whole upper lobe of the right lung might be said to be in a state of destructive suppuration. I believe I may say further with truth, though from the old date of some of the cases I am not quite sure of their whole history in this respect, that in no one of all these cases had anything approaching a rigidly antiphlogistic treatment (as it is called) been pursued, either before or after admission. Many of them, indeed, had been entirely neglected; but in such as had been seen by medical men before admission, it had apparently never occurred to the medical man to use such a treatment as I have indicated; and, in particular, not one of all these fatal cases was bled. The treatment in the hospital, so far as treatment was possible, consisted of diffusible stimulants and cough mixtures, with such food as could be taken, mild diuretics, and, in one or two, the extremely cautious use of antimony. I think it therefore perfectly clear that there is no room for the supposition of any death having been brought about by too great activity in the use of heroic remedies in any of the cases here recorded.

During the same period (1855-1859), I find from the hospital books that I have treated, on an average, from 12 to 20 cases of inflammatory disease of the lungs annually; or, in the aggregate, from 60 to 100 cases, including under the larger number a rather vague estimate of all kinds and varieties of disease in which acute or serious symptoms went along with the evidences of pulmonary condensation; and under the smaller, a rather restricted calculation of those cases which I regarded at the time as being genuine pneumonia. Some readers may possibly be surprised at the latitude here allowed to my statistics; but I have long been of opinion that not all cases attended with pulmonary condensation, and marked by acute symptoms, are pneumonia in the proper sense of the term, and I therefore prefer to leave it to the reader to take either the larger or the smaller number, according to his own idea of what should be called pneumonia. I have formerly made some remarks on this subject, which will indicate my own views,¹ and will show on what principles I have proceeded in the naming of my cases. I have already

¹ *Edinburgh Medical Journal*, May 1856, p. 981. Compare also *Erit. and For. Med. Chir. Review*, January 1854; in a paper entitled "Modern Practice in Diseases of the Chest," in which I have shown the results of erroneous diagnosis in causing the application of the treatment of pneumonia to cases of collapse of the lung.

expressed my conviction, that severe cases (judging by symptoms) were the exception; mild cases the rule. Still there were not wanting numerous cases having all the more characteristic symptoms and signs of pneumonia, as described by Grisolle, and other well-known authorities; and though very serious dyspnoea and fever were exceptional, I have rarely failed to find the characteristic expectoration, which I have always estimated at a far higher value in diagnosis than any of what are commonly called the physical signs taken singly, or even, in some cases, than several of them taken together.

I have said that it is not the object of this paper to vindicate a system of treatment in pneumonia; for the simple truth is, that I have no system to vindicate. Some years ago (in 1851, I think), when I had only lately been appointed to the charge of a single hospital ward as assistant-physician, I was asked by an Italian physician then in London, of high reputation, and fully versed in continental opinions, what was the system pursued in Edinburgh, and what I followed myself—meaning, he said, "Are you antiphlogistic, or contro-stimulant, or stimulant, or expectant, in your practice?" I replied, "So far as I can observe, we have no system in the matter; for myself, within the last three months, I have treated different cases by all these methods, believing that what is to be treated is not so much *the pneumonia*, as the *individual patient*." I mention this, to show the state of mind in which I began hospital practice. Nevertheless, it may be the means of doing good, and at all events of affording a point of comparison or contrast with the observations of others, if I state as shortly as possible the rules by which I have been guided in the administration of remedies. Among single drugs, antimony has been first on my list: the greater number of the cases (not excluding some of those which appeared most debilitated), which were attended by marked fever and oppression, have had it in one shape or another; commonly in the ordinary form of tartar emetic, in doses varying from $\frac{1}{10}$ of a grain to 1 grain every hour or two. I have differed from some of my friends, and I believe from some of my colleagues, in giving the antimony *always alone*, i.e., with nothing to mask its physiological effects; and in always simply withdrawing it, or diminishing the dose, so soon as any form of unfavourable effect, such as vomiting, purging, or depression of the system, was continuously manifested. This I believe to be better practice, on the whole, than the current method of giving the antimony with opium. Further, I have always withdrawn the antimony the instant the fever appeared to be decidedly checked, and the patient in the way of convalescence; having rarely found any relapse to follow from this practice, which has the great advantage of allowing the diet of the patient to be carefully adjusted to his capabilities of digestion in convalescence, without the chance of disturbance by a superfluous medicine. Very many mild cases, and some severe cases coming in late in the disease, have been

treated by little more than common cough mixtures. Opium has sometimes been given, but chiefly as a palliative; calomel with opium (indeed mercury in any form) has been very little employed; having been given only in obstinately continuing condensations, and then only as an experiment, with great caution, and with, as I think, little positive result. In one case, indeed, the resident physician had prescribed calomel and opium in the acute stage, and I continued it, experimentally; and I am bound to state that the patient made a good recovery; but his gums were not touched. Blistering has been reserved, for the most part, for severe cases and obstinate condensations threatening to become chronic; poultices, warm fomentations, turpentine, and, more rarely, leeches, have been used as local applications in the acute stage of pain; chloroform, stimulating liniments, blisters, and iodine, at later stages, when pain was not removed. Blood-letting has been used in two cases by me, and in two or three more before the patients' admission. All that were bled did well, but I seldom see cases early enough, and acute enough, and in sufficiently robust individuals, to justify the use of this remedy; in which, nevertheless, I have by no means lost faith, believing it to be both very useful in fit cases, and very apt to be made a bad use of in incautious hands. Stimulants, and especially ethereal stimulants, have been freely used in cases in which the vital powers seemed in danger of failing; and that, whatever the treatment in other respects may have been. But my practice has differed entirely, if I rightly apprehend the matter, from that of the late Dr Todd of London, in respect that I have never given stimulants very largely, or as a matter of routine, or of aliment; almost never in slight cases, or in the early stages of the disease; and very rarely indeed to young persons, or to those not habituated to the use of alcoholic drinks. Food has been given simply according to the patient's powers of digestion; neither withheld, nor pressed: in the febrile period, bread and milk, or beef-tea with arrowroot, or both; during convalescence, a diet more generous and varied, but not overstimulating. The *diète absolue*, if I may judge from what I was taught and have seen myself, is in no favour in Edinburgh, either in pneumonia or in other febrile diseases. And to conclude, in all cases of doubt and difficulty, I have uniformly adopted the principle, that nature is to be trusted to a great extent; believing that patients will recover much better and sooner under no treatment at all, than under a routine treatment blindly enforced; or, in other words, that nature is a better manager than a bungling physician, who has always an inexorable system in hand to control her operations.

ON INDIGO IN THE URINE.

By W. GILCHRIST, M.D.,

TORQUAY.

Reprinted from the Edinburgh Medical Journal, December 1861.

THERE are several cases on record in which a blue deposit has been observed in the urine, but these are so few in number that I believe the following notes of such a case may not be uninteresting. The patient is a poor woman residing at Christow, a small village in Devonshire. For some years she has been under the care of my friend Dr Lillies of Chudleigh, through whose kindness I have been enabled to see the patient, to obtain specimens of the urine for examination, and to give the following notes of her case:—

I shall first describe the urine.

The urine is of a dirty greenish yellow colour, strongly ammoniacal; Sp. grav. 1010. On standing some time it deposits a dirty greenish blue sediment, which to the naked eye seems to be composed—1st, Of a heavy white crystalline deposit; 2d, Of whitish flakes of organic matter; and 3d, Adhering to or mixed with the latter, particles or patches of a deep blue colour. Microscopic inspection proves the white deposit to consist of the triple phosphates; the flakes of organic matter to consist of tessellated epithelial cells, which are swollen, granular and opaque, so that the nucleus is invisible. These cells are in the state which Virchow terms "cloudy swelling." The blue matter under the microscope appears amorphous, and presents two distinct shades—a light and a dark blue—both in brightness of tint resembling rather the shades of Prussian blue than those of indigo. Besides the blue matter there are little masses of a yellowish brown colour, which are finely molecular in structure. Neither the blue nor the brown matter is altered by water, acetic acid, ammonia, or cold alcohol. In warm alcohol both are soluble. Strong nitric acid dissolves both, destroying the colour.

On adding sulphuric acid to a small portion of the clear urine (after Carter's method), a faint pink or copper coloured tint is produced, thus proving that only a small amount of uroxyanthin is present. Heat or nitric acid produces no coagulum.

The following notes of the case are given me by Dr Lillies of Chudleigh:—

"Mary Clampit, ætat 58, a labourer's wife. Has been the subject of rheumatism since the age of 19. About twenty-four years

ago, by a fall from a horse, she was pitched on her back, and has never been quite well since. Two years after this accident she suffered from what appears to have been a severe attack of hæmaturia, which continued more or less for two years and a half.

"Fourteen years ago she was seized with acute pain over the region of the heart, when she consulted a physician, who, on examining her chest, pronounced that organ to be diseased. At or about this time she first remarked a blue deposit in her water, which became at the same time very offensive; and this condition has continued more or less ever since. For the past four years her sight has been impaired from incipient amaurosis.

"At present she suffers at times at night from slight dyspnoea and palpitation. She sleeps very badly from the frequent desire she has to micturate, and the pain about the neck of the bladder, which she experiences just before and during the act. At times she is obliged to get up every five or ten minutes, sometimes passing two tea-spoonfuls, sometimes two table-spoonfuls, but seldom more. The total amount of urine passed in twenty-four hours is about three pints. She is very weak, and has an anæmic appearance. Tongue clean; appetite capricious. She fancies that after taking salt fish or salt bacon there is always more 'blue stuff' in the water, and more pain in passing it. When the water is 'very blue,' she feels as if gravel or something rough was passing. Bowels are regular. There is considerable oedema of the lower extremities, and some puffiness under the eyelids. A bellows murmur accompanies the first sound of the heart. It is loudest at the base, and is heard along the course of the larger vessels. Pulse 84, rather jerking. The veins are everywhere full, in the neck particularly so; but there is no venous pulse. The treatment which has been most beneficial has consisted of the administration of opium and the mineral acids."

Remarks.—Although, on account of its small quantity, I have not been able to obtain a positive chemical identification of this substance with indigo, I have little doubt that it is so. By physiological and chemical writers, uroglaucon is classed with urohaematin as one of the colouring matters of the urine.¹ But it is not proved that either of these substances exists as such in the healthy urine. Both are products of decomposition, and it has not been satisfactorily shown that they have any share whatever in producing the orange tint of healthy urine. Very little is known about the true colouring matters of urine; and the inquiry is found to be a very difficult one, both on account of their ready decomposition, and also of their intimate connexion with other constituents of the secretion. Regarding indigo or uroglaucon, the following may be remarked. From the researches of Heller, Kletzensky, and Schunck, it appears

¹ See *Lehrbuch der Physiologie*, by O. Funke, 1860, vol. i., p. 500; *Anleitung zur pathologisch-chemischen Analyse*, von Dr Hoppe, 1858, p. 120.

that a neutral substance, capable of being resolved by treatment with acids into red and blue indigo and sugar, exists in the natural urine. This substance was first described by Heller under the name of uroxanthin, and it is now known as indican (Schunck); it is identical with the indigo-yielding substance of the isatis tinctoria. In an interesting paper by my friend Dr Carter, it is shown that this indican may be regarded as a constant and normal element of the urine, and that it also exists in the blood. Dr Hoppe of Berlin has looked for indican (using Schunck's method) in the cerebro-spinal fluid, and in other transudations, but has met with negative results.² Carter, however, once detected it in an acute pleuritic effusion. I am inclined to agree with the latter in regarding indican as an entirely excrementitious product. But the question occurs, What is it which, in certain cases of disease, determines the resolution of this substance into indigo? and where does the change occur? I can find only other two cases of indigo (or of a blue deposit) in the urine. One of these, mentioned by Dr Prout in his work on "Stomach and Urinary Diseases" (3d edit., p. 96), is that of a middle-aged man of a nervous temperament, in whose urine indigo appeared after taking a seidlitz powder. No details of the case are given. The other case occurred in the practice of Dr Eade of Norwich, and is related by Dr Beale in his "Archives" (No. IV., p. 311). This patient was an elderly man, and the most prominent symptoms were great frequency of micturition, and pain before and during the act. The urine was of sp. grav. 1015; contained pus, phosphates, blue particles, which, like those of the above case, were of two shades—a light and a dark blue. The latter had the form of crystalline spicules. The urine was also highly ammoniacal. No cause could be assigned for the ailment.

What occurs in these cases is probably this. Indican is a very unstable compound—a low temperature being sufficient to decompose it into indigo (red and blue) and sugar. A ferment or a mineral acid produces the same effect. One or other of these causes effects the transmutation, probably when the urine is in the bladder, and a sediment of indigo, with but a small amount of indican, is the result. This I believe to be the pathology of such cases. It has been observed that a greenish or blue colouring matter is formed on the surface of pus and other albuminous substances when they have been allowed to stand for some time exposed to the air.³ Probably this is also a similar transmutation of indican, by the action of a ferment, into uroglaucon. But a few exact chemical observations are required fully to elucidate the subject.

¹ Edinburgh Medical Journal, August 1859.

² Virchow's Archiv, bd. xvi., p. 393.

³ See Hoppe, *loc. cit.*, p. 121.

From the author

XIII. — *On the Estimation of Sugar in Diabetic Urine by the loss of Density after Fermentation.*

By WILLIAM ROBERTS, M.D. Lond., Physician to the Manchester Royal Infirmary.

Read October 16th, 1860.

WHEN saccharine urine is fermented with yeast its specific gravity, previously ranging from 1030 to 1050, falls to 1009 or 1002, or even below 1000. This result is chiefly due to the destruction of the sugar it contained, but partly also to the generation of alcohol, and its presence in the fermented product.

As the diminution of density must be proportional to the quantity of sugar broken up by the ferment, the amount of this diminution evidently supplies a means of calculating how much sugar any urine contains, always provided that the remaining ingredients of the urine continue unchanged, or become changed in some uniform ratio.

In order to ascertain the exact relation subsisting between the density lost on fermentation and the sugar destroyed, experiments were made on the fresh urine of several diabetic patients in the Royal Infirmary.

The following procedure was adopted :

1. The amount of sugar per 100 parts was first accurately determined by the volumetrical method, with Fehling's test solution.
2. Next the density was taken by the specific gravity bottle.

3. About 4 oz. of the urine were then placed in a 12 oz. bottle, with a drachm or two of German yeast, and set aside in a warm place to ferment, taking care to cover the mouth of the bottle with a slip of glass or a loose cork.

4. In from twelve to eighteen hours fermentation was usually over, and at the end of twenty-four hours the froth and scum had subsided or been dissipated sufficiently to permit the density to be again taken.

Operating in this way on a urine passed on the 21st of April, I obtained the following results:

Sugar per 100 parts by the volumetrical method	7.69
Density before fermentation at 60° or D	1038.60
Density after fermentation at 60° or D'	1005.92
Density lost or D-D'	32.68

The relation, therefore, between the density lost and the per centage of sugar in this instance was as 32.68 to 7.69, or as 1 to 0.235; so that by multiplying the density lost into the co-efficient 0.235 we have for product the amount of sugar per 100 parts which this urine contained. That is, sugar per 100 parts or $S = (D - D') \times 0.235$.

On repeating the experiment a great number of times with different specimens of urine and different specimens of yeast, the number 0.230 was found to be the more exact co-efficient.

The degree of exactitude with which the quantity of sugar may be determined by this method is very great; indeed with the precautions to be mentioned presently it seems susceptible of nearly as much accuracy as the volumetrical method.

The following table places in comparison twenty observations made by the two methods on various diabetic urines, with densities ranging from 1031.52 to 1053.48:

TABLE I.

No.	Sugar per 100 parts by the formula $S = (D - D') \times 0.23$	Sugar per 100 parts by direct volumetrical analysis.	Difference.
1	7.51	7.69	0.18
*2	7.47	7.69	0.22
3	6.68	6.66	0.02
*4	6.72	6.66	0.06
5	5.16	5.18	0.02
*6	5.19	5.18	0.01
7	5.65	5.77	0.12
*8	5.65	5.77	0.12
9	4.47	4.35	0.12
*10	4.49	4.35	0.14
11	7.85	8.06	0.21
12	5.91	6.10	0.19
13	11.27	11.36	0.09
*14	11.21	11.36	0.15
15	5.69	5.68	0.01
16	8.11	8.06	0.05
*17	8.09	8.06	0.03
*18	8.00	8.06	0.06
19	8.20	8.20	0.00
20	7.61	7.74	0.13

These results are so close that the discrepancies may be regarded as due to errors of manipulation rather than to any fault in the method.

In pursuing the inquiry further it was found that the volumetrical analysis, in spite of every care in its performance, did not possess the delicacy and certainty required in a standard when minute differences were concerned; inasmuch that, when a discrepancy appeared between the indications of the volumetrical and the fermentation methods, it was found impossible to decide in which proceeding the error lay.

In order therefore still further to test the constancy of the results, artificial diabetic urines were prepared by diluting a natural diabetic urine with known volumes of water, or of a healthy non-saccharine urine. Assuming the estimate of the sugar in the original urine to be correct, the quantity of sugar in the dilutions could be ascertained

* Those marked with an asterisk are duplicate experiments.

with almost absolute accuracy by a simple calculation. In the following table the results obtained by fermenting these dilutions are placed side by side with the calculated quantity of sugar.

TABLE II.

No.		Sugar per 100 parts according to the formula $8 - (12 \cdot 5) \cdot 8 \times 23$.	Sugar per 100 parts by calculation from the first estimate.	Difference.
1	A natural diabetic urine	5'91	—	—
2	The same mixed with $\frac{1}{10}$ of its bulk of water	5'31	5'32	0'01
3	The same mixed with $\frac{1}{20}$ of its bulk of water	4'71	4'73	0'02
4	The same mixed with $\frac{1}{30}$ of its bulk of water	4'16	4'14	0'02
5	The same mixed with $\frac{1}{40}$ of its bulk of healthy urine	5'34	5'32	0'02
6	The same mixed with $\frac{1}{50}$ of its bulk of healthy urine	4'77	4'73	0'04
7	The same mixed with $\frac{1}{60}$ of its bulk of healthy urine	4'15	4'14	0'01
8	The same mixed with $\frac{1}{70}$ of its bulk of healthy urine	0'70	0'59	0'11

Numbers so nearly alike as those in these two columns may be considered as practically identical. In the last experiment only, where the quantity of sugar was under one per cent., was there a sensible discrepancy.

Satisfied now with the accuracy of the fermentation method, I was desirous of determining with more certainty and exactitude the required co-efficient; which, from the preceding experiments, using the volumetrical analysis as a standard, was fixed at 0'23.

I was unable to obtain grape sugar in sufficient purity to make solutions of known strength, and had recourse to cane sugar; using for the purpose the best loaf sugar of the shops.

Solutions were made, varying in strength from 2 to 10 per cent. both with distilled water and healthy urine. But as cane sugar becomes converted into grape sugar under

the influence of yeast before fermentation begins, and the density of the solution is thereby materially increased, corrections had to be made on both these accounts before the experiments could be fairly compared with those on diabetic urine. In making the first correction cane sugar was taken as $C_{12}H_{22}O_{11}$ and grape sugar as $C_6H_{12}O_6$. In making the second correction allowance was made for the increase of density in accordance with the tables published by the authors of the report on "Original Gravities."*

Six solutions were made of cane sugar in water; two containing 10 per cent. and the remaining four containing respectively 8, 6, 4 and 2 per cent. of sugar.

The mean co-efficient obtained from these six experiments was 0'234.

Twelve solutions were similarly made in a healthy non-saccharine urine; two containing 10 per cent., three 8 per cent., and two 2 per cent. The remainder contained respectively 6, 4, 1'4, 1 and 0'6 per cent. These yielded a mean co-efficient of 0'228; and the general mean for the eighteen trials was a fraction under 0'23. These experiments therefore confirmed the results previously obtained with the volumetrical analysis as a standard.

Equally correct results were obtained in operating on weak solutions, containing only 1 or 0'6 per cent. of sugar, where the density lost was not more than 3 or 4 degrees, as with solutions containing 10 per cent. of sugar, in which the loss of density exceeded 43 degrees.

Having examined the question experimentally, and fixed the co-efficient from multiplied trials under varied conditions, it was not without interest to examine a little more closely the several items which go to constitute the "density lost" in fermentation, and to endeavour to arrive

* Report on Original Gravities, by Professors Graham, Hofmann and Redwood, in the *Quarterly Journal of the Chemical Society*, 1853.

synthetically at similar results to those obtained by the more empirical method of direct experiment.

If we take a solution of cane sugar in water, made so that 100 grains are contained in each 1000 grain-measures, its density at 60° will be 1038.64.

Such a solution consists, by weight of 100 grains of sugar, and 938.64 grains of water.

After fermentation there remains 53.74 grains of alcohol, and 987.18 grains of water.

Such a mixture of alcohol and water has, according to Gilpin's tables,* a density of 993.

This figure is very near that obtained in actual fermentation. The density of the fermented product is, however, a little higher, about 1½ or 2 degrees. This slight excess is to be attributed to the escape of a portion of alcohol with the carbonic acid during fermentation, and to a little soluble matter taken up from the yeast.

Some effect probably, though I know not of what kind, is likewise produced by the retention in the liquor of more than its own bulk of gaseous carbonic acid in a state of solution.

It was uniformly found, with solutions of equal strength, that those made with urine showed rather more "density lost" than those made with water. This is an indication either that changes take place in some of the non-saccharine constituents of the urine during fermentation—but changes so slight and so constant in their nature that they do not interfere materially with the accuracy of the mode here proposed for estimating sugar—or else that urines usually reputed non-saccharine do in reality contain a small quantity of sugar.

An excessive quantity of yeast was used in these experiments in order to hasten the process of fermentation. The quantity of soluble matter taken up from the yeast was

* See Henry's *Chemistry*, tenth edition, vol. ii. p. 343.

very small. Two drachms shaken up with 4 oz. of water, and left in a warm place for thirty-six hours, only increased the density of the water by 0.3°. In the above experiments a piece of yeast about the size of a large filbert or small walnut was employed to ferment four ounces of urine, a little more or a little less making scarcely a sensible difference in the results.

The fermented urine continues somewhat turbid for a day or two after fermentation is completed, and the degree of this turbidity has an appreciable though slight effect on the density. In the experiments detailed in this paper the second density was taken about twenty-four hours after the addition of yeast, and before the urine had completely cleared. It was found in four trials that if the urine was allowed to rest twelve hours longer the density fell by 0.2° or 0.3°. It is desirable, therefore, where a series of experiments is made, and scrupulous accuracy and uniformity are required, always to take the second density at about the same period after fermentation has ceased.

The method here proposed for determining sugar is not put forward in rivalry with the accurate and elegant volumetrical process now usually employed by chemists; but it is believed that it will be of great service to the medical practitioner, who is unaccustomed to delicate chemical manipulations. Any one who has had much practice in the volumetrical method knows that great nicety and considerable experience are requisite to insure trustworthy results. Fehling's test solution also is liable to speedy deterioration unless hermetically sealed from the air. This arises from the conversion of a portion of the tartaric acid of the test into racemic acid, which, equally with sugar, has a reducing power on the oxide of copper, and when present of course vitiates the results. For these reasons it may be surely predicted that the volumetrical method can never come into general use at the bed-side.

The fermentation method, on the other hand, is of exceedingly easy performance, and the taking of densities is an operation to which the medical practitioner is daily accustomed. I shall have an opportunity through another channel to bring this method under the notice of my medical brethren, and to enter more fully on the particulars which concern its clinical application. I content myself here with a short sketch of the process, as it may be conveniently carried out in private or hospital practice.

1. The specific gravity of the urine is taken at the ordinary temperature of the ward or bed-room.
2. Three or four ounces of the urine are poured into a 12 oz. phial, together with a lump of German yeast of the size of a large filbert.
3. The bottle is lightly corked, or covered with a slip of glass, and set aside in a warm place to ferment.
4. In about eighteen hours, when the fermentation has entirely ceased, the bottle is tightly corked and removed to the ward or bed-room so that it may cool to the temperature at which the specific gravity was taken the day before.
5. The urine in the meantime clears, and in five or six hours it may be decanted into an appropriate vessel and the specific gravity taken again.
6. The amount of "density lost" is thus ascertained, and the following simple and most convenient rule expresses the result of the analysis. *Each degree of "density lost" indicates one grain of sugar per fluid ounce of urine.* So that in the example already quoted in the earlier part of this paper, where the urine lost 32.68° of density after fermentation, the quantity of sugar indicated was 32.68 grains per ounce, or 653.6 grains per imperial pint.

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URÆMIC INTOXICATION.

BY

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URÆMIC INTOXICATION.

WHEN we consider how important a part the kidneys fulfil in depurating the blood, we can readily believe that any serious interruption to the due performance of their function must be attended by great disturbance in the healthy action of the other organs of the economy. We find that such is actually the case. Physiological experiments, together with many well-established cases of disease, have taught us that suppression of the urinary excretion is one of the most dangerous events that can happen in the whole range of pathological occurrences.

In the present memoir, I propose to consider the subject of uræmia, or that condition of system due to the accumulation in the blood of matters which in health are removed by the kidneys, basing what I have to say mainly upon my own investigations.

When the renal arteries of an animal—as, for instance, a dog—are ligated, or the kidneys removed, death ensues in from two to four days generally, though occasionally life is retained for a longer period. In one of my own experiments, the animal, a small dog, lived for twelve days; and Marchand¹ mentions a case in which a sheep lived for nearly a fortnight after removal of the organs in question.

At first, the animal upon which this operation has been performed does not appear to be seriously inconvenienced thereby. It eats, sleeps, and follows its other instincts with but little irregularity. After a variable period, noticeable symptoms begin to manifest themselves. These are loss of appetite, nausea, vomiting, and indisposition to exertion of any kind. Occasionally there is purging. Finally, there is either coma or convulsions, or both, and the animal dies in a state of stupor, or in epileptiform convulsions. If, previous to the death of the animal, the blood be examined, it is found loaded with urea, and this substance can generally be detected in the matters vomited and discharged *per anum*. *Post-mortem*

¹ De l'existence de l'urée dans les parties de l'organisme animal autres que l'urine. *L'Expérience* 1839, t. II. p. 43.

examination reveals the presence of urea in the contents of the stomach, in the blood, the lungs, liver, and other parts of the body.

In Bright's disease, as it affects the human subject, we find that, owing to structural changes in the kidneys, the urine is imperfectly eliminated. An element, albumen, is separated from the blood, which does not normally exist in the urine. The excretion, upon analysis, is found to be deficient in urea, and may even, as I have myself found, be almost entirely free from it. Coma and convulsions at length appear, and death soon follows. If, previously to the above, the blood be analyzed, urea is discovered in large quantity, and a *post-mortem* examination reveals the presence of this substance in nearly every portion of the system. Such is the natural termination of Bright's disease. Sometimes, however, the retained elements of the urine, by their action on the brain and nervous system, produce inflammations of important structures, and death ensues before the stage of uræmic poisoning is reached.

In puerperal eclampsia, in the latter stages of scarlet fever, in yellow fever, and in cholera, symptoms and appearances similar to those above mentioned are frequently present, and probably depend upon a like immediate cause.

To these symptoms, taken collectively, the terms uræmia, uræmic intoxication, and others of like import have been applied.

Various theories have been propounded, relative to the immediate cause of uræmia. Thus, Osborne¹ considered it due to arachnitis; Prevost and Dumas² to effusion into the ventricles of the brain; Rees³ thinks it may be caused by a watery state of the blood; and Bence Jones⁴ supposes it to be induced by an accumulation of oxalic acid in this fluid. There is no evidence to support any of these hypotheses; in fact, each has been positively disproved.

The view most generally held, has been that which ascribes uræmia to the direct action of the urea retained in the blood. Later investigations have called the correctness of this hypothesis in question, and much cogent evidence has been adduced against it. Like most other physiological questions, the present is not to be solved but by the patient and thorough research of numerous investigators. I can only hope that the observations and experiments which follow may prove to be contributions in the right direction.

The fact that urea is formed in the blood may be regarded as sufficiently determined. It is not present in the muscles, for analysis fails to detect it; but other substances, the immediate products of their destructive metamor-

¹ On the Nature and Treatment of Dropsical Diseases. London, 1837, p. 36.

² Annales de Chimie et de Physique, t. xxiii. p. 90 et seq.

³ On the Nature and Treatment of Diseases of the Kidneys, &c. London, 1850, p. 67.

⁴ Lectures on Animal Chemistry. Medical Times, January 34, 1852.

phosis, and which admit of still further degradation, are found; and the experimental evidence, which establishes the fact that increased muscular exertion leads to increased elimination of urea, indicates these organs as one, at least, of its sources of origin. In addition, we have the beautiful researches of Bechamp,¹ which are conclusive as to its production from proteinaceous substances.

Prevost and Dumas,² Marchand,³ and numerous other observers, have shown that, after extirpation of the kidneys, urea accumulates in the blood, and consequently these organs cannot be regarded as forming this substance. They, in fact, only separate it, as they do the other constituents of the urine, from the blood brought to them by the renal arteries. As the healthy kidneys are constantly in action, urea never normally exists in the blood in any very considerable amount. It is only when, through disease or other cause, the kidneys are prevented performing their function of elimination, or when large quantities are submitted to examination, that urea is to be detected in the blood in any but very small proportion.

The immediate cause of the production of the group of symptoms known as uræmia is at present, as has been already stated, a point upon which there exists some diversity of opinion, though numerous researches have been made with the view of elucidating the subject. Urea has been injected into the blood of animals, and as death did not often follow, this substance has been regarded by some as non-poisonous. Even urine, filtered so as to free it from mucus and epithelium, has, under like circumstances, proved harmless; but when injected unfiltered, death invariably ensued. The inference therefore from these last cited experiments is, that the urine, if not eliminated, is not, as such, capable of causing death, for the mucus and epithelium are not separated from the blood by the kidneys, and consequently are not, properly speaking, constituents of the urine. As, however, the injection of unfiltered urine has always resulted in death, the further deductions must be drawn that epithelium, or mucus, or both, are of themselves poisonous, or that one or both are capable of so acting upon the retained elements of the urine, as to cause the formation of some substance possessed of toxic properties. These conclusions are, however, of course only legitimate, provided that the experiments referred to have been conducted with the care and accuracy so absolutely essential in all physiological investigations. It will be seen hereafter that it is more than probable the experiments cited were not altogether free from error.

In a philosophical treatise on the subject of Bright's disease, Frerichs⁴ advances the opinion that uræmia is not due directly to the presence of

¹ Annales de Chim. et de Phys. Novembre, 1856.

² Annales de Chim. et de Phys., 1823, t. xxiii. p. 90.

³ L'Experience, 1839, t. ii. p. 43.

⁴ Die Brightsche Nierenkrankheit und deren Behandlung. Braunschweig, 1851, p. 111.

urea in the blood, but to the conversion of this substance into carbonate of ammonia, through the agency of a ferment supposed to be present in the circulating fluid. This view has been accepted by Braun,¹ but has been controverted by Schottin,² Zimmerman,³ Gallois,⁴ myself,⁵ Bernard,⁶ and others.

The arguments relied upon by Frerichs to support his theory are based, mainly, upon experiments on the lower animals—dogs—but partly upon observation of cases of Bright's disease which came under his notice. As this hypothesis, from the source whence it comes, from its apparent simplicity, and from certain facts which seem to give it support, has attracted to a considerable extent the attention of pathologists, I deem it important, before proceeding further, to enter somewhat at length into its consideration.

The fact that urea may be injected into the blood without uræmia following, and that even filtered urine may be thus introduced with impunity, are circumstances considered by Frerichs as incompatible with the hypothesis which ascribes this condition to the retention of the urea *per se*. That in animals, the kidneys of which have been removed, there was vomiting and purging of ammoniacal matters; that ammonia was exhaled from the lungs; and that after death, as well in them as in persons dying of Bright's disease, who during life had manifested symptoms of uræmia, ammonia was discovered in the blood and in the contents of the stomach and intestines; that urea has been frequently found in the blood of persons suffering from Bright's disease, in whom no uræmia was present, and *vice versa*—constitute, in his opinion, a mass of evidence strongly in favor of the theory he has advanced. Add to all this the results of experiments performed by him with direct reference to the point in question, and we have the main points before us upon which he has constructed the ingenious and beautiful hypothesis, that uræmic intoxication is directly due to the presence of carbonate of ammonia in the blood, which substance has been formed from the retained urea through the action of a ferment. Of the nature of this ferment he does not attempt to furnish an explanation.

Two series of experiments were performed by Frerichs. In the first, a

¹ The Uræmic Convulsions of Pregnancy, Parturition, and Childbed. Edinburgh edition, 1857. Translated by Dr. J. Mathew Duncan, p. 16. I regret that I have not the original work (*Lehrbuch der Geburtshilfe u. s. w. Wien, 1857*) to refer to. Dr. Duncan's translation, however, appears to be very correctly rendered.

² Vierordt's Archiv., 1853, Heft 1, p. 170.

³ British and Foreign Medico-Chirurgical Review. Am. ed., January, 1853, p. 223. From Deutsche Klinik, No. 37.

⁴ Comptes Rendus, No. 14, Avril, 1857.

⁵ On the Injection of Urea and other substances into the Blood. North American Medico-Chirurgical Review, April, 1858.

⁶ Leçons sur les Propriétés Physiologiques et les Altérations Pathologiques des Liquides de l'Organisme, t. II. Paris, 1859, p. 36.

solution containing from two to three grammes of urea (thirty-one to forty-six grains) was injected into the veins of dogs, the kidneys of which had been previously removed. The animals remained for some hours to all appearance unaffected; a circumstance which Frerichs regards as indicating that the urea, as such, exerted no detrimental influence upon the nervous system. After a longer or shorter period (from one and a quarter to eight hours), the animals became restless, and vomited acid chyme, or a mucous matter of a yellow colour, according as the stomach was full or empty, at the commencement of the experiments. Under the latter condition, the vomited matter was of decided alkaline reaction. Convulsions occurred, and, at the same time, ammonia was expelled with the expired air. Finally, the animals fell into a state of coma, the respiration became stertorous, and death soon followed. Occasionally the convulsions were absent, coma being the first symptom of cerebral disturbance. After death, which generally occurred in from two and a half to ten hours after the injection of the urine, ammonia was always found in the blood. The contents of the stomach, in most instances, gave off a strong ammoniacal odor, and contained carbonate of ammonia in large quantity. In one case they were feebly acid, and even then ammonia was present. This substance was also found in the bile and other secretions.

In the second series of experiments, a solution of carbonate of ammonia was injected into the circulation, the kidneys of the animals remaining intact. Immediately afterwards convulsions ensued, which, in some cases, were very violent, but were soon succeeded by a comatose condition. The respiration was laborious, and the expired air was loaded with ammonia. There was also vomiting of biliary matters. The stupor lasted for several hours; so long as it was present, ammonia continued to be exhaled from the lungs. Gradually it ceased to be expired, and the animals slowly recovered their senses. If, during the coma, an additional amount of carbonate of ammonia was injected, convulsions again occurred, the vomiting was renewed, and the urine and feces were involuntarily evacuated. In from five to six hours, the ammonia again disappeared from the blood, and the animals regained their ordinary liveliness.¹

There are several objections to be urged against these experiments of Frerichs, and the inferences drawn from them by him and those who coincide with him in his uræmic theory. In order to present these more connectedly and with greater clearness, I quote Frerichs' own details of the first experiment of each series, which may be taken as the type of the others of its class. The first series consisted of six experiments.

¹ No. 1.—Both kidneys were extirpated from a young full-grown dog, at 3 o'clock P. M., by opening the abdominal cavity. On the following morning, the animal appeared to be perfectly well; it ate, and wagged its tail when spoken

¹ Op. cit., p. 111 et seq.

to. The posterior extremities, however, appeared to be paralyzed. At 3 P.M. a solution of two grammes of urea was injected into the left jugular vein. The animal remained in the same state as before, its condition not being altered in the least. At 4 P.M. it became restless, seemed to be choking, and vomited several times. The matter thus ejected consisted of a slimy yellow fluid, with a strong alkaline reaction. Soon afterwards convulsions supervened, the animal rolled from one side to the other. Opisthotonos ensued in the posterior, alternating with violent contractions of the other muscles of the body. From time to time the animal was quiet, after which the convulsions returned with increased violence. In the meanwhile, the vomiting continued, and a glass rod, moistened with chlorhydric acid, and held to the nose, caused the formation of thick white fumes, showing the presence of ammonia in the expired air. By degrees, the convulsions abated in violence, and at length entirely ceased. The animal now lay in a state of sopor, the respiration being quick and difficult. Finally, the respiratory motions became weaker, and at 5½ P.M. death ensued.

"The cavities of the body were immediately opened, and the blood contained in the heart and large vessels collected. It was of a dark violet colour. The colour did not become markedly clear by heating, but by the following morning it had assumed a bright scarlet hue. After four minutes, the fibrin was strongly coagulated. The blood-corpuscles were not changed from the normal form. Ammonia in considerable quantity was contained in the blood—a glass rod previously moistened with chlorhydric acid giving rise to the white fumes indicative of its presence.

"A part of the defibrinated blood was mixed with water, and distilled in the water-bath. A fluid with an alkaline reaction came over, which, being neutralized with chlorhydric acid and evaporated, deposited crystals of chloride of ammonium. To another part of the blood caustic potash was added, and an ammoniacal odour was given off.

"The stomach was strongly contracted, and contained still a few corroded pieces of bone, and a small quantity of a yellow, tenacious, ropy fluid. The stomachal mucous membrane was of a livid red colour, partly through vascular injection and partly through imbibition. The fluid contained in the viscus exhaled a sharp odour of ammonia, reacted strongly alkaline, and formed thick fumes with chlorhydric acid. In an alcoholic extract of the same, no traces of unconverted urea were discovered. In the bile, also, urea was sought for, but only compounds of ammonia were detected. The substance of the brain, and the membranes covering it, contained the normal quantity of blood, and the fluid in the ventricles was not increased in amount. The lungs were healthy, except that posteriorly there was some congestion. The mucous membrane of the trachea and bronchia was slightly reddened. The liver and spleen were, to all appearance, healthy. In the abdominal cavity, a small quantity of bloody fluid was contained. No evidences of intense peritonitis were visible."

The other experiments of this series do not differ in any material respect from the one quoted, except that in the third no ammonia was detected in the expired air. The details of the fifth and sixth are not given.

Now, in these experiments there is not the least evidence that carbonate of ammonia was the cause of death, or even that this substance was present in the blood in any abnormal amount.

In the first place, it is asserted that ammonia was present in the pulmonary exhalation. This statement is doubtless correct, but the inference which Frerichs draws from it is, I think, unwarranted. Ammonia can generally be detected in the products of respiration in healthy dogs, into the veins of which no urea has been introduced, and in which the kidneys

¹ *Op. cit.*, p. 278 *et seq.*

are in healthy functional activity, by employing the test made use of by Frerichs. It is, however, occasionally absent, and in several cases which will hereafter be more specifically referred to, in which urea had been introduced directly into the blood, ammonia was not to be found in the pulmonary exhalation, though recourse was had not only to Frerichs' process, but to others of far greater delicacy.

In the second place, the means employed by Frerichs to establish the existence of ammonia in the blood were faulty in the extreme, and such as will always effect this object if urea be present, even though ammonia, in the first instance, be altogether absent. The defibrinated blood was distilled in a water-bath (at what temperature we are not informed), and an ammoniacal fluid collected. Now, no matter how carefully this operation was conducted, if any urea was present, a portion of it would have undergone decomposition, and ammonia would have appeared in the distillate.

Thus, I took about thirty cubic centimetres of freshly-drawn dog's blood, defibrinated it by shaking it in a bottle with small strips of lead, and then added to it five grammes of urea. It was then placed in a small retort, and carefully distilled in the water-bath at a temperature of 85° Cent. (185° F.), till a sufficient quantity had passed over. A few drops of the distillate were then placed on a strip of glass, and held for a few moments over a vessel containing chlorhydric acid. Upon evaporation, and subsequent microscopical examination, crystals of chloride of ammonium were found in large quantity. A similar strip of glass was then exposed to the vapor of chlorhydric acid, and held for a short time to the ball of the retort whilst the distillation was progressing, in such a manner that the vapour was received upon it. Thick white fumes of chloride of ammonium were produced, and crystals of the same substance were formed upon the glass.

In order further to establish the presence of ammonia, Frerichs added caustic potash to another portion of the blood. This proceeding was equally objectionable as the other, for this substance decomposes urea into carbonate of ammonia, and, of course, therefore it was not surprising that an ammoniacal odour should have been evolved.

Now, I have no intention of denying that ammonia existed in the blood of the dogs submitted by Frerichs to experiment. On the contrary, I believe it to be generally present in the blood of most animals. The fact that Frerichs detected it by holding over the blood a glass rod moistened with chlorhydric acid, is perhaps sufficient to establish the point. But that this circumstance is at all confirmatory of his theory is far from being the case. Richardson¹ has shown, by numerous experiments, that ammonia is a constant constituent of the blood of dogs and many other animals, and by repeated observations I have satisfied myself of the correctness of this

¹ *The Cause of the Coagulation of the Blood. Astley Cooper Prize Essay for 1856. London, 1858.*

conclusion. I have frequently failed when using Frerichs' test with the glass rod moistened with chlorhydric acid, which, besides its want of extreme delicacy, is liable, from several causes, to yield erroneous results. These, however, with care, may generally be avoided. I have, therefore, preferred either Reuling's process with logwood, or Richardson's, which consists in moistening a slip of glass with chlorhydric acid, and exposing it to the vapour arising from the blood, when, if ammonia be present, minute crystals of chloride of ammonium will be formed, and may be readily perceived under the microscope, with an object glass of moderate power. This process has already been referred to as the means employed during the present researches in examining for ammonia.

I am aware that Davy¹ has not been able to detect ammonia in the blood of the common fowl. Not having access at present to the original paper, I know not what process he employed. With Richardson's method, I have never failed to find it in the blood of this animal in the course of fourteen experiments with reference to this point.

It will be seen, therefore, that ammonia is not an abnormal constituent of the blood, as Frerichs evidently supposes. There is no proof, either, that any of the retained urea was converted whilst in the system into carbonate of ammonia.

The fact that ammonia was found in the contents of the stomach, which consisted of fragments of undigested bone and of mucus, should not have the least weight in the matter, as it is well known that this latter substance causes the decomposition of urea, and the consequent formation of ammonia. No urea was detected in the alcoholic extract of these contents, and it is therefore probable that it was, as Frerichs supposes, entirely decomposed. There is no evidence, however, to lead us to infer that this change was effected in the blood, but, on the contrary, much to warrant us in believing that it took place in the stomach, through the action of the mucus present in this viscous.

In support of this view, I adduce the following experiments:—

Expt. To a full grown dog, fasting, two grammes of urea were administered dissolved in water. Fifteen minutes afterwards, the animal was killed by injecting a solution of woorara under the skin. The stomach was opened, and found to contain nothing but a quantity of thick, tenacious mucus, which exhaled a strong ammoniacal odour. On bringing a glass rod moistened with chlorhydric acid near it, the dense white fumes of chloride of ammonium were formed, and crystals of this substance appeared on a slip of glass used in the manner before specified.

Expt. To another dog, somewhat smaller than the other, five grammes of urea were given in the same manner as before, and at the end of ten

¹ Archiv des Vereins für gemeinschaftliche Arbeiten, Zweiter B. 1856, S. 120.
² North American Medico-Chirurgical Review, vol. iv. No. 1, 1860, p. 149.
 (From Dublin Quarterly Journal of Med. Science, Nov. 1859, p. 425.)

minutes the animal was killed by dividing the medulla oblongata. The stomach was immediately opened. It contained a few fragments of bone, some pieces of undigested meat, and a quantity of thick mucus. The mass was of feeble alkaline reaction, and evolved a barely perceptible ammoniacal odour. The presence of ammonia was further established, as in the first instance.

The contents were then examined for urea by the process hereafter detailed, and this substance was shown to be present in considerable amount. Owing to an accident, it was not weighed.

If, however, the animal has eaten largely a short time previous to the injection of the urea, the change into carbonate of ammonia does not occur, and if the animal be killed soon after the administration of the urea, this substance is found intact in the stomach. If, however, sufficient time has elapsed, it is absorbed into the circulation and excreted by the kidneys.

The following experiments are adduced, as tending to establish these propositions:—

Expt. A full grown dog was fed largely on animal food, and thirty minutes afterwards two grammes of urea were administered to it. Fifteen minutes after taking the urea it was killed by section of the medulla oblongata. The contents of the stomach were of acid reaction. On testing for ammonia by Richardson's method, a negative result was obtained. Neither were fumes of chloride of ammonium formed by the proximity of chlorhydric acid.

Expt. A full grown dog was confined and fed during three days on raw beef, two and a half pounds being given to him in the twenty-four hours. A uniform quantity of water was allowed. The urine of each period of twenty-four hours was collected, and tested for urea by Liebig's process with the nitrate of mercury. On the fourth day, the animal was fed as before, but with each meal two grammes of urea were administered. Six grammes were administered in all. The immediate effect was to increase to a considerable extent the amount of urine eliminated, which contained an augmented quantity of urea. The following table exhibits the results:—

	1st day.	2d day.	3d day.	4th day.
Quantity of urine .	815 c. cm.	795 c. cm.	891 c. cm.	1073 c. cm.
Urea	15.25 gram.	14.75 gram.	15.10 gram.	20.22 gram.

Gallois,¹ from experiments on rabbits, arrived at a similar conclusion, and has also shown that urea acts as a violent poison on these animals when injected into the stomach in sufficiently large amount. A train of symptoms was induced similar to those of uremia, of which the animals died. During the progress of these symptoms, there was no ammonia in the breath.

I cannot therefore perceive, from a consideration of Frerichs' experiments, and from those performed by myself, as well as from the other evidence adduced, that there are any facts to warrant us in concluding that urea is transformed whilst in the blood into carbonate of ammonia.

¹ Op. cit.

As has been already stated, Frerichs performed another series of experiments, in which the carbonate of ammonia was directly introduced into the blood. In this series, a filtered solution containing from one to two grammes of the carbonate was employed. The first experiment, from which the others do not materially differ, is detailed by Frerichs as follows:—

"No. 1.—Into the jugular vein of a strong, full-grown dog, the solution was very slowly injected. The dog moaned and fell into deep stupor, broken occasionally by convulsions. The respiration was quickened, and the expired air was loaded with ammonia. The coma lasted for three hours, after which the animal regained its ordinary liveliness. During the coma, ineffectual efforts to vomit twice occurred."

The experiments of this series amounted to six in number. None of the animals subjected to them died—a fact which, to say the least, is not one calculated to support Frerichs' hypothesis.

That carbonate of ammonia, when introduced directly into the circulation, produces considerable disturbance in the phenomena of life is doubtless correct. It is, however, excreted so rapidly by the lungs, that the most enormous quantity may be injected without death ensuing. Thus Dr. Steiner,¹ late of the United States Army, performed some years since a series of experiments, some of which show this very strikingly. In one instance, two ounces of strong aqua ammoniæ, diluted with an equal amount of water, were introduced into the circulation of a horse, and although the animal suffered severely for a few minutes, it soon rose from its feet, and trotted about as though nothing had happened.

I have myself² also experimented with reference to this point, and have injected as much as sixty grains of carbonate of ammonia into the jugular vein of a dog in normal condition. Convulsions ensued, and ammonia was exhaled from the lungs. The animal, however, recovered perfectly in a short time. Urea was also injected with a like ultimate result. The symptoms observed were, however, by no means identical in both cases.

In a second series of experiments performed, and detailed in the same memoir, the kidneys were removed, and in these animals death followed in a few hours after the injection of ammonia and other substances. From these experiments it is apparent that carbonate of ammonia possesses no pre-eminence as a toxic agent over substances not regarded as poisonous. The investigations, however, are not sufficiently extensive to warrant the formation of decided conclusions from them. It is perceived, however, that when urea was injected it was not decomposed into carbonate of ammonia either in unmailed dogs or in those deprived of their kidneys.

In Bright's disease, there is a condition of system present very similar to that existing in animals the kidneys of which have been removed. In

¹ Medical Examiner, vol. v. No. 11, N. S., 1849, p. 644.

² North American Medico-Chirurgical Review, vol. ii. p. 291.

persons, therefore, suffering from this affection, carbonate of ammonia, if injected in large quantity into the blood, might cause death, or, even if retained in this fluid to any very great extent, the same result might follow. As, however, we have no proof that there is any accumulation of this substance in the blood during the progress of the disease in question, we cannot ascribe uræmic intoxication to its influence.

In the breath of persons labouring under Bright's disease, Frerichs constantly detected ammonia, and this circumstance is advanced as an additional argument in favour of his hypothesis. Allusion has already been made to the fact that ammonia can generally be found in the pulmonary exhalation of dogs. It is perhaps even a more constant constituent of the human breath, even in that of persons who, as far as can be perceived, are in perfect health. Richardson,¹ in many examinations, failed to find it only in one person. Schottin² detected it in the breath of persons suffering under other affections than Bright's disease, and in sixteen cases of uræmic poisoning found ammonia in the breath but once, and then, he thinks, it was probably derived from the mouth.

Mettenheimer³ found white fumes produced by a rod moistened with chlorhydric acid, as readily with the breath of healthy persons as with that of subjects of uræmia; and, in a note to his memoir, Beneke states that he has arrived at similar results. Viale and Latini,⁴ by numerous delicate experiments, have also shown that with each act of expiration ammonia is exhaled from the lungs of the healthy human subject.

From my own investigations, it has resulted that I have scarcely ever failed to find ammonia in my own breath, or in that of very many healthy persons whom I have examined.

I have thus considered, at some length, the arguments and experiments brought forward by Frerichs in support of his theory, and have, I think, sufficiently shown that it is not tenable. It is because, if generally adopted, it may lead to very grave errors in diagnosis and practice, that I have deemed it important to show how many facts tend to disprove the hypothesis in question.

Another view of the cause of uræmia, inferentially supported by Bernard,⁵ requires some notice. According to this hypothesis, the condition in question is produced by decomposition of the tissue of the kidneys, and the retention in the blood of the elements arising from their putridity. The

¹ Op. cit.

² Virchow's Archiv, 1853, Heft I. s. 170.

³ Archiv des Vereins für gemeinschaftliche Arbeiten u. s. w. Band I, Heft iv. 1854, p. 605.

⁴ American Journal of the Medical Sciences, April, 1855, p. 488. From L'Union Médicale, t. viii, No. 98, Août 1854.

⁵ Leçons sur les Propriétés Physiologiques et les Altérations Pathologiques des Liquides de l'Organisme. Paris, 1859, t. ii. p. 34 et seq.

experiments of Müller and Peipers,¹ and of Marchand,² are adduced as tending to establish the correctness of this theory. These observers induced mortification of the kidneys by ligating the renal nerves, and though it is asserted that death ensued from uremia, it is by no means clear that this condition was not due to the cessation of the function of the kidneys, rather than to the entrance into the blood of septic matters from the mortified organs. Moreover, when the kidneys of an animal are removed, there is certainly no retention in the blood of putrid matters arising from their decomposition, and yet death invariably follows. The entrance into the blood of substances in a state of putrefactive fermentation would more probably induce a pyæmic or typhoid condition of the system than one of uremia. Finally, in Bright's disease, of which uremia so frequently forms a prominent concomitant, pathology shows that the existing condition of the kidneys is not one at all analogous to putrefaction.

It is perhaps unnecessary to consider other objections which are applicable to this hypothesis.

The theory which much observation and numerous experiments have led me to think most probably correct, is that which ascribes uræmic intoxication to the direct action of the elements of the urine retained in the blood upon the brain and nervous system, in a manner which we do not at present understand. Of these elements, we have strong reasons for deeming urea the most poisonous.

It is true, that urea has been directly introduced into the circulation of healthy animals without death ensuing. Thus Vanquelin and Segalas³ injected a solution of this substance into the veins of dogs and cats, without the production of any remarkable effect other than an increase in the amount of urine excreted. They then injected urine, and death followed with symptoms of uræmia. Hence they drew the conclusion, that it is the urine as a whole which, when retained in the blood, induces intoxication.

Frerichs⁴ thinks that death in these cases arose from the urine being unfiltered; that is, from the mucus and epithelium derived from the urinary passages. He has repeatedly injected from twenty to forty grammes of filtered urine, sometimes even adding urea to it, into the veins of animals, without causing death. An increased quantity of urine was evacuated, but the normal condition of the organism was not otherwise disturbed. A warm saturated solution of urate of soda, likewise produced no untoward result. The amount of urea excreted by the kidneys was, however, greatly increased.

Other observations have also been made relative to the point in question,

¹ Archiv für Physiologie, 1836.

² Journal für praktische Chemie, Band xi. s. 149.

³ Journal de Physiologie de Magendie, t. ii. p. 354.

⁴ Op. cit., p. 105.

and leading to the same general conclusions. It may therefore be regarded as a well-established fact, that urea, in considerable amount, may be introduced into the blood of healthy animals, without death being necessarily produced.

It has been definitely shown that urea is a normal constituent of the blood. It has been found in the chyle, the lymph, the saliva, the bile, the aqueous and vitreous humours, the perspiration, the liquor amnii, the fluid of blisters, in dropsical effusions, in fecal evacuations, and even in the milk. It cannot therefore be regarded as poisonous, unless when, from defective excretion, it accumulates in the blood.

The amount of urea formed within the organism is, as has already been shown, subject to very great variation; but so long as the kidneys continue to perform the function of elimination in accordance with the requirements of the system, the normal balance between the urea and the blood is not disturbed. Thus it is that urea, and even urine, may be directly introduced into the circulation without inducing continued uræmia; for so soon as these substances reach the kidneys (as Vanquelin and Segalas, Frerichs, and many other investigators have shown), there is an increased elimination of urine, they are expelled from the body, and the coma, convulsions, and other accompaniments of intoxication cease.

Occasionally, it happens that when the kidneys do not properly depurate the blood, other organs assume their office, and then uræmic intoxication does not occur.

Several cases are on record in which, from total suppression of the function of the kidneys continuing for a long time, this condition existed. One of the most remarkable of these is that reported to the French Academy of Sciences, several years since, by Monte-Santo,¹ of Padua, the accuracy of whose statements was confirmed by MM. Graefe and Frank from personal observation. In the case in question, there was complete constipation of the bowels and suppression of urine for fourteen years. There was always vomiting in from two to five hours after each meal, and about once a month a large quantity of fecal matter was discharged in the same manner. Although it is stated that there was no odour of urine in these egesta, it is more than probable that the stomach vicariously performed the function of the kidneys, and perhaps the skin also shared with it this office. Some years previously, a case had been reported to the Academy, in which there had been no discharge of feces or urine (by the ordinary channels, at least), for a period of seventy-two years.

Whatever doubt may be attached to such cases as the above, it is very certain that in several diseases it frequently happens that other organs than the kidneys eliminate some of the elements of the urine from the blood. This is especially the case in Bright's disease, in yellow fever, and in cho-

¹ Medico-Chirurgical Review, July, 1833 (American edition), p. 236.

lera. Immediately previous to the accession, and during the continuance of the stage of collapse, in this last named disease, the cutaneous transpiration often contains urea, which is deposited by evaporation on the skin. I have frequently had occasion to notice this circumstance, and in one case in particular, which fell under my charge, the skin of the face, chest, and arms presented the appearance of being dusted with a fine white powder from the large quantity of urea which covered it. The urine contained scarcely a trace of this substance, but it was found both in the fluid vomited and that discharged *per anum*. I do not cite this case as one at all singular, but merely for the bearing which it has upon the subject under consideration. In the numerous cases of cholera which have fallen under my observation, I have generally, whenever an examination was instituted, detected urea in the rice-water discharges from the stomach and bowels.

The experiments of Bernard and Barreswil,¹ which show that when the kidneys are removed from an animal urea is excreted by the gastro-intestinal canal, may also be adduced. These observers extirpated the kidneys from dogs, and found that in those animals which survived but a short time no urea was to be detected in the blood, but that the matters ejected by vomiting and purging contained large quantities of ammonia, the product of the decomposition of the urea through the action of mucus and other gastro-intestinal secretions. If, however, death did not soon follow, the stomach and intestines lost their vicarious office, and then urea was found in the blood.

The fact that urea may occasionally exist in large quantity in the blood without giving rise to uræmic intoxication, is no proof that this substance is not generally poisonous. No one will deny the poisonous properties of arsenic. Dr. Taylor² gives the opinion, that a medical witness would be justified in stating it to be fatal in doses of two or three grains, yet subsequently refers to cases in which half an ounce, an ounce and a half, and even two ounces, had been taken without causing death. In the first of these instances, there was not even vomiting. So in relation to opium, as small a quantity as four grains produced death in a robust man, whilst on the other hand, as much as five ounces of laudanum has been taken without even causing sleep. Similar instances might be brought forward in relation to almost every other poisonous substance.

It may perhaps be objected, that in such cases the poisons were not absorbed into the blood; but toxic agents have been introduced directly into the circulation, and like differences in the extent of their action have ensued. I might bring forward numerous examples in support of this asser-

¹ Sur les voies d'élimination de l'urée après l'extirpation des reins. Archives Générales de Médecine, 1847, t. xiii. p. 449. Also, Leçons sur les Liquides de l'Organisme, &c., t. ii. p. 36.

² On Poisons. Second American edition, 1859, p. 341.

tion, but it will probably be sufficient to recall the fact of the total insusceptibility of some persons to the action of the vaccine virus.

In Bright's disease, the disorganization of the kidneys is generally of slow progress. They continue to perform their function, though imperfectly, and consequently the amount of urea contained in the blood is not, in the first stages, very excessive. Its accumulation is gradual, and there is therefore time for the system to become in a measure habituated to its presence in such an amount that, if suddenly introduced into the blood and not eliminated, uræmia would in all probability ensue. We find this ability of the system to adapt itself to gradual changes generally present in all animals, and with reference to the action of poisonous substances exceedingly well marked. Thus, by progressively increasing the doses, large quantities of arsenic, opium, strychnia, hydrocyanic acid, and other toxic substances may be taken without the production of the least poisonous effect.

When, however, in Bright's disease, the structure of the kidneys becomes so greatly disorganized as to unfit them entirely for depurating the blood, the elements of the urine continue to accumulate, and if not otherwise excreted, almost invariably give rise to uræmia.

As to the assertion that uræmic intoxication may exist without the accumulation of urea in the blood, I have only to say that there is no evidence whatever to support such a conclusion.

The following investigations were undertaken with the hope of being able to contribute somewhat to a fuller and more exact understanding of the cause of uræmia. I have endeavoured to avoid every source of fallacy, and though (knowing the difficulties which attend researches of this character) I can perhaps scarcely assume to have succeeded, I am, nevertheless, unaware of any circumstances which would invalidate the conclusions drawn.

To say that I entered upon the inquiry without certain preconceived opinions would be far from correct. That such views as I had conceived have, however, blinded me to the truth, or warped my judgment of things as they actually were, I do not believe. Theories are true but for the time being, and physiological hypotheses are even more ephemeral than any others. We should therefore be prepared to yield our convictions without regret, when they do not accord with the results of experiments better devised and more accurate than our own, for only by so doing can we entitle ourselves to be considered useful labourers in the fields of science.

The chemical processes used in the several determinations were as follows:—

In examining the blood for urea, a weighed portion was mixed with its volume of strong alcohol, and evaporated over sulphuric acid or chloride of calcium, *in vacuo*, to dryness. The residue was pulverized, and extracted

with cold alcohol. The alcoholic extract was filtered, and carefully evaporated to dryness at a low temperature in the water-bath, and the residue washed repeatedly with small portions of ether. The ether extract was filtered and evaporated to dryness. Nitric acid was then added to the residue, the whole thrown upon a filter of known weight, and subjected to strong pressure. It was then dried at 100°C ., and weighed. The difference between the weight of the whole and that of the filter gave the amount of nitrate of urea, and from this the quantity of urea was calculated.

Whenever the amount of urea was too small to determine quantitatively, the dried ether extract last obtained, as in the foregoing process, was placed upon a glass slide, and nitric acid added to it under the microscope. The production of rhombic and hexagonal tablets, the opposite acute angles of which measured 82° , was deemed sufficient evidence of the presence of the substance sought for.

The same methods were used to determine the existence of urea in the vomited and fecal matters, whenever they were examined for it.

For ascertaining the amount of urea in the urine, Liebig's volumetric process with the proto-nitrate of mercury was always employed.

In the determination of ammonia, Richardson's process, already detailed, or Reuling's, was made use of. Whenever negative results were obtained by the one, recourse was always had to the other.

In the first place, I was desirous of ascertaining more definitely than had hitherto been done, the action of urea when injected into the blood of sound and healthy animals.

Expt. A large adult dog, weighing 65 pounds, was fed for three days on fresh meat. During this period, ammonia was constantly found in the breath.

On the fourth day the jugular vein of the left side was opened, and a sufficient quantity of blood abstracted. 100 grammes of this contained 0.019 gramme of urea. Ammonia was present, as it was likewise in the expired air.

The urine passed on this day amounted to 1025 cubic centimetres, and contained 11.28 grammes of urea.

The food was the same as on the preceding days.

On the morning of the fifth day, at 9 A.M., 3 grammes of urea dissolved in 30 cubic cent. of distilled water, were injected into the jugular vein. The animal, from the very first, appeared to suffer pain. It moaned; the breathing became laboured; and it trembled violently, as if from fright or cold. At the end of about 20 minutes, the animal became more quiet, and even appeared to be somewhat stupefied. After nearly an hour had elapsed, convulsions ensued. These were confined almost entirely to the posterior extremities, though at times the other portions of the body were in spasms.

At 10 o'clock, whilst the convulsions were still present, I abstracted 100 grammes of blood. It contained 0.135 grammes of urea. Ammonia was also present, though in no larger amount than on the previous day. Examined microscopically, the red blood-corpuscles were found to be of

normal size, shape, and colour. They appeared, however, to be diminished in quantity. The white corpuscles were very evidently increased in amount.

The animal continued to be convulsed till about 2 o'clock P. M., when coma ensued. This lasted $3\frac{1}{2}$ hours. The dog then awoke, and passed a large quantity of urine. It amounted to 280 cubic centimetres, and contained 2.15 grammes of urea.

Before, during, and after the convulsions, ammonia was exhaled with the breath.

Immediately on the dog awaking, I again abstracted 100 cubic centimetres of blood from the jugular vein. This contained 0.014 of urea.

The total amount of urine voided on this day was 1381 cubic centimetres, containing 14.63 grammes of urea.

The dog ate as much on this day as on any previous one. It recovered perfectly.

Expt. For this experiment, a dog, weighing 38 $\frac{1}{2}$ pounds, was used. As in the preceding experiment, it was fed for three days on fresh meat. During this period, at only one examination (at 10 o'clock A.M., on the second day), was ammonia detected in the expired air. On the fourth day, 100 grammes of blood were abstracted from the jugular vein, and found to contain 0.027 gramme of urea. Ammonia was also present, and likewise in the pulmonary exhalation.

The urine voided on this day amounted to 834 cubic centimetres, and contained 4.09 grammes of urea.

On the fifth day, at 10 o'clock A. M., 5 grammes of urea dissolved in 30 cubic centimetres of distilled water, were injected into the jugular vein. No immediate effect was produced. After the lapse of 45 minutes, there were slight spasms of the muscles of the eyelids; and 50 minutes after the injection, a severe general convulsion ensued. The vein was now reopened, and 100 grammes of blood taken. Upon analysis, this was found to contain 0.254 grammes of urea. The convulsions continued with great violence for 15 minutes. Coma followed, and lasted till 6 o'clock P. M., when the animal died. There was no excretion of urine after the injection of the urea. The breath was examined every hour for ammonia, but at no time was it detected; neither was it present on this day before the urea was injected. It was, however, found in the blood last drawn. There was neither vomiting nor purging.

Immediately after death the post-mortem examination was commenced.

The substance of the brain appeared to be perfectly healthy; but there was considerable injection of the vessels of the meninges. The ventricles contained about 15 cubic centimetres of serous fluid. Urea was detected in this by the process mentioned, and microscopical examination. It was likewise found in the blood from the sinuses.

The vertebral canal was laid open, and the spinal cord examined. Its substance presented a normal appearance, but there was some congestion of the vessels of its membranes.

The chest contained a small quantity of serous fluid. The lungs were congested, but were otherwise healthy. The heart was of normal size, and did not appear to be in the least diseased. It contained a considerable quantity of fluid blood; 100 grammes were collected from it and the large vessels. The urea in this quantity amounted to 0.873 grammes.

Upon microscopical examination of this blood, the red corpuscles were found to present a crenated margin, and to be in decidedly less than the

normal quantity. The white corpuscles were very much increased in quantity; as much as in well-marked leucocythemia.

The cavity of the peritoneum contained a small quantity of serous liquid. The membrane was in places slightly congested.

The liver was healthy in appearance, but the spleen was considerably enlarged, and contained much more than the normal quantity of blood. The tissue of this latter organ, when examined microscopically, was found to present several important deviations from the normal structure. The Malpighian corpuscles were almost entirely absent, and there was a very great increase in the number of parenchyma cells. These latter were much larger than I have ever found them in the spleen of the dog. The red blood-corpuscles in the splenic blood were generally aggregated in groups, and were of irregular forms.

The stomach was opened, and presented nothing abnormal. The contents, consisting of mucus with a few pieces of bone, were of alkaline reaction, and contained both urea and ammonia, the latter in considerable amount.

The kidneys were enlarged and very much congested. Upon cutting into them, the blood poured out from innumerable orifices. There was no obstruction to either the renal arteries or veins that was discovered after death. The tissue of the kidneys, when submitted to microscopical examination, showed excessive congestion of the capillaries, and enlargement of the Malpighian bodies. Into many of these latter extravasation of blood had taken place, and the tubes were gorged with this fluid.

The bladder contained a small quantity of bloody urine.

Death in this case was, I think, obviously due to non-elimination of urea, and perhaps of the other elements of the urine, through excessive hyperæmia of the kidneys. The cause of the kidney affection I do not know. It was undoubtedly caused either directly or indirectly by the injection of the urea, for up to the time of that operation the function of these organs was perfectly effected.

The experiment cannot but be regarded as exceedingly instructive. There was complete arrest of excretion from the kidneys, and the blood, besides retaining the elements of the urine, received in addition a large quantity of urea, which remained in the organism. In many respects, therefore, the experiment resembled those in which the kidneys have been extirpated, and urea subsequently introduced into the blood. In all such experiments, death has invariably taken place within a few hours. There is, however, this important difference, that the system was saved the shock of a serious operation, and therefore one source of error was eliminated.

In relation to the alteration in the form of the blood-corpuscles, it can scarcely (having Kölliker's experiments in view) be ascribed to the direct physical action of the urea, as the proportion of this substance present in the blood was altogether too small (less than 1%) to effect the change. Kölliker¹ found that a solution containing 30% of urea caused the red cor-

¹ Zeitschrift für wissenschaftliche Zoologie, B. vii. s. 183. Also, Quarterly Journal of Microscopical Science, vol. iii. 1855, p. 229.

puscles of the frog to assume the form of stellate cells, and finally to melt down and disappear. These alterations were also produced, though much more slowly, by solutions of 15% and 12%. When weaker solutions were used, they were not caused. Human blood-cells were only rendered smaller and colourless. The phenomenon observed in the case under consideration, taken in connection with the diminution of the number of red and increase in that of the white corpuscles, must probably be ascribed to some defect in the process of sanguification.

For the purpose of ascertaining the effect of introducing repeated quantities of urea into the blood, I proceeded as follows:—

Expt. A dog weighing forty-eight pounds was fed as the others for three days. Ammonia was always found in the breath.

On the fourth day, at 10 o'clock A. M., one hundred grammes of blood were abstracted, and upon examination found to contain 0.021 grammes of urea. The urine voided during the twenty-four hours amounted to 1224 cubic centimetres, and contained 8.15 grammes of urea. Ammonia was present both in the blood and expired air.

The following morning, at 10 A. M., I introduced into the jugular vein five grammes of urea, dissolved in thirty cubic centimetres of distilled water. No immediate effect was produced, the animal remaining perfectly quiet. At 11.15 convulsions ensued. One hundred grammes of blood were now taken, and yielded 0.193 grammes of urea. The convulsions, at first slight, became more violent. They continued about twenty minutes, and were succeeded by stupor. At 12 M., I injected, as before, five grammes of urea. Immediately afterwards, the animal voided 365 cubic centimetres of urine, in which were contained 3.17 grammes of urea. The coma continued, and at 2 P. M. I again injected five grammes of urea into the blood. At 3.20 P. M. the dog passed 425 cubic centimetres of urine, containing 4.06 grammes of urea. The coma was still present. At 4 P. M., I injected ten grammes of urea, dissolved in thirty-five cubic centimetres of distilled water, into the blood. The stupor was now very profound, the heart beat slowly, and the respiration was laboured and stertorous. I again, at five P. M., abstracted one hundred grammes of blood for examination. It contained 1.683 grammes of urea.

At 5.30, the dog evacuated *per anum* a small quantity of yellow, serous fluid. Urea and ammonia were detected in it.

At 5.45 the animal died, having in eight hours received directly into the blood twenty-five grammes of urea.

Ammonia was found in the breath during the whole course of this experiment, and likewise in the blood.

The urine voided from the commencement to the end of the experiment amounted to 890 cubic centimetres, and contained 7.23 grammes of urea. In the twenty-four hours, there were evacuated 1625 cubic centimetres of urine, in which were contained 12.37 grammes of urea.

On post-mortem examination, the brain and spinal cord were found healthy; the membranes of both were of perfectly normal appearance. About five cubic centimetres of fluid were collected from the ventricles of the former. By simply placing a drop on a glass slide, and adding nitric acid, crystals of nitrate of urea were formed.

The lungs were found congested, but there was no effusion into the pleural cavities. The pericardium was very much congested. The heart contained

a large quantity of blood. Its lining membrane, both in the auricles and ventricles, was redder than natural.

One hundred grammes of the blood from the heart and large vessels contained 1.385 grammes of urea. The blood-corpuscles were of normal size and shape, but, as in the former experiments, were remarkably diminished in number, whilst there was an increase in the number of white corpuscles.

The peritoneal membrane was found congested in patches. The spleen was enlarged, and contained a large quantity of blood. Beyond this the structure, when examined with the microscope, exhibited no abnormal appearance. The kidneys were healthy.

The stomach contained about one hundred and fifty cubic centimetres of fluid, resembling the rice-water discharges of cholera. The intestines also contained a quantity of the same kind of fluid. Both urea and ammonia were present in it.

For the purpose of still further determining the action of large quantities of urea, when introduced into the circulation, the following experiment was performed:—

Expt. A small adult dog, weighing thirty and a quarter pounds, was fed as the others, for three days, on fresh meat, before any investigations were commenced. On the fourth day, at 10 o'clock A. M., one hundred grammes of blood were drawn from the jugular vein. This quantity contained 0.024 grammes of urea. The total amount of urine evacuated during the twenty-four hours was 830 cubic centimetres, containing 5.12 grammes of urea. On this and the preceding days, ammonia was always found in the breath. The food remained the same.

On the fifth day, at 10 A. M., twenty-five grammes of urea, dissolved in thirty cubic centimetres of distilled water (thus forming a nearly saturated solution), were injected into the jugular vein. The animal lay down quietly in its box, and at first did not seem to be greatly disturbed. After a few moments slight twitchings of the muscles ensued, and at 10.30 there was a strong convulsion. Some of the spasms, subsequently, were decidedly tetanic. During the convulsions, I reopened the jugular vein and allowed 100 grammes of blood to flow out. The amount of urea contained therein was 2.005 grammes. The convulsions lasted with undiminished violence till 11.10 A. M., when 481 cubic centimetres of urine, containing 7.50 grammes of urea, were excreted. They then became slighter, and at about 12 M. were succeeded by coma. This continued without intermission till 4.15 P. M., when the animal quietly died in most profound stupor. Two hundred and twenty-five cubic centimetres of urine, containing 3.12 grammes of urea, were excreted a short time before death. There was neither vomiting nor purging. Ammonia was constantly found in the breath, but not in greater quantity than during the previous days.

The blood drawn during the convulsions was examined with the microscope. The red corpuscles were altered in shape, and had become much paler than natural. Scarcely one could be found which was not more or less irregular in outline. They, besides, appeared to have lost their ordinary consistence, and when two or more came together they fused, forming an irregularly formed mass. It was thus impossible to determine microscopically their relative numbers. The white corpuscles were very much increased in quantity. The blood coagulated firmly.

The cavities of the body were opened immediately after death. The

membranes of the brain were found in a state of intense congestion, and the sinuses and large vessels at the base of the cranium were gorged with blood. The substance of the brain, when cut into, exhibited a uniform pink tinge from excess of blood, and the red spots indicating the situation of capillaries were greatly increased in number. This was the first case in which the substance of the brain presented direct evidence of hyperæmia. About twenty cubic centimetres of fluid were collected from the ventricles.

The membranes of the spinal cord were likewise congested, especially in the lumbar region, though more or less throughout their whole extent. Into the cavity of the arachnoid there was an effusion of serous fluid, amounting to fifteen or twenty cubic centimetres.

The lungs were also congested, and there was a considerable amount of bloody fluid effused into the pleural cavities. The pericardium contained a quantity of liquid. The right side of the heart was distended with blood. The left contained but a small quantity. The heart did not exhibit any indications of pre-existing disease of any kind. I was prevented examining the blood chemically. The blood-corpuscles presented the same appearances as those in the blood abstracted before death.

The peritoneum was not diseased; there was no effusion. The liver was of normal appearance. The spleen was enlarged, and contained a large quantity of blood. The red corpuscles were here found almost entirely broken down into a liquid substance. No Malpighian corpuscles were discovered. Large quantities of acicular crystals were scattered through the substance of the spleen, and were visible by microscopical examination.

The kidneys were slightly congested, and some of the tubes contained blood. There were no other appearances of disease.

Two other experiments, similar to the last, were performed. As the results were almost identical, I refrain from detailed descriptions of them.

From the foregoing experiments, it is perceived that there is a limit to the power of the system to eliminate urea, and that when this substance is introduced into the blood in large quantity, it causes death by uræmia. By this I mean that the urea induces such an abnormal condition of the blood, that the brain primarily, and subsequently other organs (the kidneys included), are brought into an abnormal condition, and are thereby prevented performing their functions. From the results of the *post-mortem* examination, it is apparent that this state is one of congestion. I am therefore disposed to think that if the brain had been able to resist the toxic power of the urea for a considerably longer period, the kidneys would have eliminated the surplus urea, and death in these latter experiments would not have ensued.

It is well known that in Bright's disease death frequently occurs from congestions and inflammations of important structures. Thus this termination may be caused by œdema of the glottis, by pericarditis, by pneumonia, by peritonitis, by apoplexy, &c. Have we not reason to regard these several affections as due to an abnormal condition of the brain and nervous system, induced by the retention in the blood of excrementitious matters which in health are removed?

It is, I think, very evident that in neither of the foregoing experiments

was there the least reason to suppose that there was any decomposition of urea into carbonate of ammonia. In the second experiment, this substance was not found in the breath after the injection of the urea into the blood, although it was present at two examinations before this operation.

The alterations in the blood observed with the microscope are very important, and constitute one link in the chain, connecting the retention of urea with derangement of the brain and nervous system. They, perhaps, show that it is not necessary that a toxic condition of the blood should consist altogether in disturbances of the chemical balance existing between its several component parts. The morbid condition of the spleen which was found to exist, is also an interesting circumstance, taken in connection with the changes in the form of the red, and alterations in the relative number of these and the white corpuscles.

Leaving, for the present, the further consideration of these experiments, I proceed to the detail of those constituting the second series, and having relation to the effects following ligation of the renal vessels, or removal of the kidneys.

Exp. A large adult dog, weighing 68½ pounds, was selected. At 10 A. M. the breath was examined for ammonia, and this substance was found to be exhaled from the lungs in considerable quantity. 100 grammes of blood taken from the jugular vein contained 0.026 gramme of urea.

At 3 P. M. the animal was placed under the influence of chloroform, and the kidneys removed, the abdomen being opened to the smallest possible extent. Scarcely a drop of blood was lost; the anaesthesia passed off without the least untoward effect.

The next morning the dog was in apparently good condition, but manifested no desire to eat, or to move about. At 3 P. M., twenty-four hours after ablation of the kidneys, 100 grammes of blood were drawn, and found to contain 0.083 gramme of urea. The animal refused all food; ammonia was constantly in the breath.

On the third day, at 10 A. M., the dog seemed weaker. Up to this period, however, there had been no convulsion, or coma, neither was there any vomiting or purging. At about 2½ P. M. there was a slight spasm, succeeded in a few moments by a violent general convulsion. At 3 P. M. 100 grammes of blood gave 0.093 gramme of urea. At 4 P. M. the animal vomited a quantity of alkaline mucus, containing both urea and ammonia—the latter in large amount. After the vomiting, the convulsions abated in violence, but soon became as intense as at first. At intervals, however, the animal was free from spasm, and appeared to be also free from pain and uneasiness. At about 8 P. M. coma ensued, alternating with the convulsions till 10½ P. M., when it became persistent, and very profound. Death ensued at about 4 A. M. the next day, sixty-one hours after the removal of the kidneys.

The post-mortem examination of the body was commenced at 9 A. M. The membranes of the brain were intensely congested, and there was an effusion of about twenty-five cubic centimetres of serum into the cavity of the arachnoid. The substance of the brain, when cut into, exhibited a pinkish hue, and numerous bloody points appeared, showing enlargement, and increase in the number of capillaries. The sinuses and vessels at the base of

the brain were turgid. Fifteen cubic centimetres of fluid were found in the ventricles.

The spinal cord was apparently healthy, but its membranes were slightly congested. There was no abnormal amount of fluid found.

Both lungs were congested, and both pleurae bore evidences of recent incipient inflammation. There was slight effusion. The heart was gorged with blood. The pericardium was healthy. 100 grammes of blood from the heart and large vessels contained 0.097 gramme of urea.

When submitted to microscopical examination, the blood taken from the heart exhibited appearances similar to those previously noticed. The white corpuscles were increased, and the red diminished in number. These latter were also of irregular shape, and decidedly paler than natural.

The peritoneum exhibited traces of recent inflammation; a small amount of bloody serum was found in its cavity.

The spleen was very much enlarged, being at least three times the size of the organ in its normal condition, as noticed when the kidneys were removed. Upon cutting into it, the substance was found entirely disorganized, and of semi-fluid consistence. No traces of Malpighian corpuscles were to be found, and the finer trabeculae and many of the larger, were entirely detached from their connections. These, with masses of blood-pigment, broken-down corpuscles, a few muscular fibre-cells, and numerous acicular crystals of hæmato-crystallin, were all the morphological elements to be discovered. The parenchyma cells, white corpuscles, free nuclei, &c., had been destroyed.

The liver was also enlarged. It was not examined microscopically. The stomach, on being opened, exhaled a strong ammoniacal odour. It contained a quantity of yellow, alkaline mucus. Ammonia was present in large quantity, and traces of urea. The intestines contained a like substance.

Exp. At 9 o'clock A. M., 100 grammes of blood were abstracted from the jugular vein of a large dog, weighing 60½ pounds. The urea contained therein amounted to 0.014 gramme. Ammonia was present in the breath. At 1 P. M. the renal arteries were ligated, the animal being under the influence of chloroform. As soon as the anaesthesia passed off sleep ensued, from which the dog did not awake for several hours. At 10 P. M. it appeared to be in good condition, and lapped a little milk. It was quiet, but when spoken to, manifested undoubted signs of intelligence.

The following morning at 8 o'clock the animal was quite lively. It stood up, and even walked a few steps. It ate a little bread and milk. At 4 P. M. it was somewhat drowsy, though it could be easily roused by speaking loudly, or knocking on the side of its box. 100 grammes of blood taken from the jugular vein contained 0.038 of urea. Ammonia was present in the breath. The stupor continued to increase, and at about 8 P. M. was profound. When last seen for the night, at 12 M., the animal was in a very comatose condition.

The next day at 8 A. M., the coma was still present. 100 grammes of blood contained 0.043 gramme of urea. Ammonia was exhaled from the breath, but not to a very abnormal extent. The dog remained in the same condition till about 11½ P. M., when it died, 58½ hours after the ligation of the vessels. There was no vomiting, nor purging, and no visible spasms of any kind.

The following morning, at 9 A. M., I made the post-mortem examination. The membranes of the brain were congested, and about twenty-five

cubic centimetres of fluid were effused into the cavity of the arachnoid. The substance of the brain likewise exhibited evidences of having been in a hyperemic condition. The ventricles were distended with fluid, which, however, owing to an accident, was not measured. The sinuses and vessels at the base of the cranium were turgid with blood. The spinal cord was not examined.

The pleura, the lungs, the pericardium, and heart, were healthy; 100 grammes of blood collected from the latter, contained 0.069 of urea. Examined microscopically, the white corpuscles were found in largely increased quantity, but no other abnormal condition was discovered.

The peritoneum was congested in spots, and exhibited evidences of recent inflammatory action. The spleen was large, and softer than natural. No Malpighian corpuscles could be found. Masses of extravasated blood, in larger quantity than usual, were met with. The liver was not markedly affected. The stomach and intestines contained the residue of undigested food, with some yellow, alkaline mucus. Ammonia and urea were both present.

From the foregoing experiments, it is seen that, after the removal of the kidneys, or ligature of the renal arteries, the amount of urea in the blood was increased threefold within a short period, and that there was no reason to suppose any conversion of this principle into carbonate of ammonia. The pathological changes are interesting, and congestion and inflammation of important organs are seen to be produced as well after ablation of the kidneys, as after the direct injection of urea into the blood, or during the course of Bright's disease. The immediate cause, in each of these instances, is probably the same—derangement of nervous influence through a morbid condition of the blood.

Four other experiments, similar to the two foregoing, were performed. In all, the quantity of urea in the blood was greatly increased after removal of the kidneys, or simple ligature of their arteries, and the post-mortem appearances were in general the same. In three of these there were convulsions and coma, in one coma alone—as in the second experiment of this series. In one experiment, the animal lived forty-nine hours after the operation, in one fifty-three, in one sixty-eight, and in one seventy-three hours. Ammonia was found in the breath both before and after the operation. In none was there any vomiting or purging.

But that these last-mentioned results do sometimes happen, there can be no doubt, and, in fact, judging from the investigations of these observers, they are usual concomitants. In one experiment which I performed subsequently to the above cited, they were present from the first, and doubtless by these means the urea was removed from the blood, and this fluid preserved in a comparatively normal condition. In no other way can we account for the lengthened continuance of life after the kidneys were extirpated. As this experiment is important in several respects, I give the details of it in full.

Expt. A dog weighing thirty-eight pounds was selected for operation.

Before the extirpation of the kidneys, the breath was ascertained to contain ammonia. In 100 grammes of blood were 0.009 grammes of urea.

The kidneys were removed at 9 A. M., the dog being under the influence of chloroform. After the operation, the animal fell into a quiet sleep, and did not awake for six or seven hours. At about 6 P. M. it ate a little bread and milk.

The following morning at 8 o'clock it was in apparent good condition, wagged its tail when spoken to, and ate quite freely of bread and milk. It remained in the same condition all day, manifesting, however, no desire to move out of its box, though it occasionally turned from one side to the other.

The next morning it was found that in the night a little mucus had been vomited. This contained ammonia, but no urea. No other circumstances worthy of note occurred during the day.

On the fourth day, at 7 A. M., the animal seemed somewhat uneasy from nausea. Several efforts to vomit occurred, but nothing was ejected. At about 4 P. M., there was a fecal evacuation of a thin, serous fluid, of a yellow colour. Ammonia was present, but no urea. Crystals of ammonio-magnesian phosphate in large quantities were visible by the microscope. No food was taken on this day.

At 7 o'clock the following morning, it was found that there had been both vomiting and purging of ammoniacal matters during the night. One hundred grammes of blood were abstracted at 9 A. M., and found to contain 0.011 grammes of urea.

On the sixth day, there were both vomiting and purging. The animal was sensible, but refused food.

On the seventh, eighth, and ninth days, vomiting and purging occurred several times. Ammonia and urea were present in each evacuation. On each of these days, a pint of milk was conveyed to the stomach through a tube.

On the tenth day, at about 12½ P. M., a slight convulsion occurred. A pint of milk was injected into the stomach. There was neither vomiting nor purging.

On the eleventh day, at 9½ A. M., another convulsion took place, more violent than the first. The animal was, however, still sensible.

On the twelfth day, at 7 A. M., the dog was found in a comatose condition. One hundred grammes of blood were abstracted at 12 M. This quantity contained 0.041 grammes of urea. The wound in the abdomen had nearly healed. The ligatures on the renal vessels came away. Coma was present during the whole day, and death took place some time in the night after 11 o'clock. The animal was cold the following morning at 7 o'clock.

Thus life had remained for at least 278 hours after the kidneys were extirpated.

Ammonia was detected in the breath on every day but the last. The post-mortem examination was commenced at about 11 o'clock A. M. The membranes of the brain were apparently healthy, but there was slight sub-arachnoidal effusion. The substance of the brain was of normal appearance. The spinal cord and its membranes were healthy. The lungs were congested, and there were several recent pleuritic adhesions. The pericardium was in several places adherent to the heart. This latter organ was in a state of incipient fatty degeneration. It was full of uncoagulated blood, 100 grammes of which contained 0.046 grammes of urea.

The red corpuscles, both in this blood and in that abstracted on the day of death, were very much diminished in number, whilst the white corpuscles were correspondingly increased. The former were also broken down, and softer than is normally the case.

The peritoneal membrane was congested, and several intestinal adhesions had taken place. The spleen was enlarged, and felt like a bag of water. When the enveloping membranes were cut, the substance flowed out, like molasses in colour and consistence. Examined microscopically, nothing was perceived but shreds of white fibrous tissue, masses of decomposed blood-corpuscles, blood pigment, and large quantities of acicular crystals. The liver was enlarged, and in a state of congestion. The stomach and intestines contained nothing but a little mucus.

It is seen from this experiment that so long as vomiting and purging continued, there was no accumulation of urea in the blood, and no consequent uræmic intoxication. It was only when these ceased that the latter event ensued. It is therefore strongly confirmatory of the conclusion arrived at by MM. Bernard and Barreswil, which has been previously referred to. The connection between the retention of urea in the system and the occurrence of uræmia is so well marked, that it is difficult to deny to these events the relation of cause and effect.

In the third and last series of experiments, the kidneys were removed, and urea or urine, subsequently directly introduced into the blood.

Expt. From the jugular vein of a medium sized dog, 100 grammes of blood were abstracted at 10 o'clock A. M., and found to contain 0.021 grammes of urea. Ammonia was detected in the expired air. At 11 A. M. the animal was placed under the influence of chloroform, and the kidneys removed. At 12 M., five grammes of urea dissolved in thirty cubic centimetres of distilled water, were injected into the jugular vein. No immediately noticeable effect was produced. At 1½ o'clock P. M., convulsions of great violence suddenly ensued. One hundred grammes of blood from the jugular vein yielded 0.042 grammes of urea. A large quantity of highly ammoniacal fluid was vomited. It contained urea. The convulsions continued with undiminished violence till about five o'clock, when coma gradually ensued, and remained present till death, which occurred at about 8 o'clock P. M.

Before death, and during the height of the convulsions and coma, the breath was examined for ammonia, which, although constantly found, was not present in larger quantity than before the kidneys were removed.

At 1½ o'clock the following morning, the post-mortem examination was commenced.

The membranes and substance of the brain were healthy, but there was a little sub-arachnoidal and ventricular effusion.

The lungs were highly congested, as were also the pleurae in several places. The pericardium was healthy, and the heart was of normal appearance. It contained a small quantity of uncoagulated blood. One hundred grammes taken from it and the large vessels, yielded 0.032 grammes of urea.

The peritoneum was anteriorly much congested, and there was about one hundred cubic centimetres of bloody serum in its cavity. The spleen

was hyperæmic, and its normal structure disorganized, as in the previous experiments. The liver was not perceptibly diseased.

The stomach and intestines contained a small quantity of alkaline fluid. It was highly ammoniacal. Urea was not detected in it.

Three other experiments, similar to the foregoing, were performed. The results were almost identical, and it is therefore, perhaps, unnecessary to refer to them more in detail.

In the two following experiments, urine was introduced into the blood.

Expt. A dog weighing somewhat less than forty pounds was used for this experiment. The breath examined for ammonia gave distinct evidence of containing it. Previous to removing the kidneys, it was determined that 100 grammes of blood from the jugular vein contained 0.017 grammes of urea.

Anæsthesia was induced by chloroform, and the kidneys extirpated at 9 o'clock A. M. As soon as the insensibility had passed off, one hundred cubic centimetres of fresh urine (voided during the induction of the anæsthesia), unfiltered, were injected carefully into the jugular vein. No immediate effect was produced. At 12 M., thirty cubic centimetres more of the same urine were introduced into the blood. The animal remained quiet in its box till about 2 o'clock P. M., when a slight convulsion ensued, lasting only a few seconds, and confined to the anterior muscles of the body. Shortly afterwards, another occurred more violent than the first. This was followed by others. At about 4 P. M. they ceased, and coma commenced to make its appearance. One hundred grammes of blood from the jugular vein were found to contain 0.030 grammes of urea. The coma continued till after 12 P. M. The dog was found dead the ensuing morning at 5 o'clock. There had been neither vomiting nor purging. Before death, the breath was frequently examined for ammonia, and always with affirmative results.

The post-mortem examination was made at 8½ o'clock A. M. The appearances observed were not materially different from those noticed in the case just detailed, except that the pericardium was congested, and there was some little effusion into its cavity; 100 grammes of blood from the heart and large vessels, contained 0.036 grammes of urea.

Expt. A dog, weighing 49½ pounds, was the subject of this experiment. Ammonia was found in the expired air; 100 grammes of blood from the jugular vein gave 0.027 grammes of urea. The animal was at 10 A. M. brought under the influence of chloroform, and whilst anæsthesia was present, the kidneys were extirpated. When insensibility had passed off, 100 cubic centimetres of filtered urine—evacuated during the process of inducing anæsthesia—were injected into the jugular vein.

After the expiration of about two hours, convulsions ensued. These at first were slight, but more violent ones soon followed. They lasted till a few minutes before 3 P. M., when coma supervened; 100 grammes of blood taken from the jugular vein were found to contain 0.035 grammes of urea. The coma persisted, and at 8½ P. M. the animal died. There was no vomiting nor purging. Ammonia was found in the breath throughout.

The post-mortem appearances were not essentially different from those observed in the preceding experiment. The contents of the stomach were however acid, and did not contain ammonia.

From the experiments, constituting this last series, it is clearly seen that the introduction of urea, or filtered or unfiltered urine into the circulation of animals deprived of their kidneys, induces death more speedily than if such substances had not been thrown into the blood; for, we have perceived from the experiments of the immediately preceding series, that animals, the kidneys of which have been extirpated, or the renal arteries ligated, live for from forty-nine to 278 hours after the operation; whereas, in the series last detailed, death occurred in from eight to fifteen hours, in a condition of system not to be distinguished from that known as uræmia. Taken, therefore, in connection with the experiments of the first series, in which urea was introduced into the blood of sound animals, and with those of the second, in which, through extirpation of the kidneys, the elements of the urine were retained in the blood, and we have almost demonstrative proof that the resulting uræmia was directly due to the operation of these causes.

Taken as a whole, from an attentive consideration of the foregoing investigations, I think the following conclusions are legitimately deducible:—

1st. That the injection of urea, in limited quantity, into the blood of animals, produces a certain amount of disturbance in the nervous system, similar in its symptoms to the first stages of uræmia, but that this condition even disappears, if the kidneys are capable of so deparating the blood as to eliminate the toxic substance.

2d. That urea, when introduced into the circulation in larger quantity than can in a limited period be excreted by the kidneys, induces death by uræmia.

3d. That by ligature of the renal arteries, or removal of the kidneys, the elements of the urine being retained in the blood, render this fluid unsuitable to the requirements of the organism, and, consequently, induce a condition of system not essentially distinguishable from the uræmic intoxication of Bright's disease, or that caused by the direct introduction of urea into the blood. As, however, was pointed out by Bernard and Barreswil, so long as the urea, or the products of its metamorphosis, are discharged by the stomach or intestines, uræmia does not take place, but, that when these channels become closed, convulsions and coma are produced, and death soon follows.

4th. That the introduction of urea or urine into the circulation of animals, the kidneys of which have been ablated, shortens the life of such animals, as Frerichs and others have already shown.

5th. That there is reason to believe that the urine, as a whole, is more poisonous than a simple solution of urea, for, in those cases in which urine was injected into the blood, the amount of urea thus introduced was much smaller than that previously thrown in, in a pure state, and yet symptoms of as great intensity followed.

6th. That urea, or the elements of the urine, as a whole, induce such a condition of the nervous system, as strongly to predispose to congestion

and inflammation of the viscera, especially the lungs, pericardium, and spleen.

7th. That urea, when directly injected into the blood, or suffered to accumulate in this fluid by extirpation of the kidneys, deranges, in some manner, the process of sanguification, so as to disturb the normal relation of proportion existing between the white and the red corpuscles, and either to hasten the decomposition of these latter, or to interfere with the due removal from the blood of such as are broken down and effete.

8th. That there is no reason to suppose that, under the circumstances specified, urea undergoes conversion into carbonate of ammonia, but that, on the contrary, there is sufficient evidence to warrant the conclusion that no such process ensues. The fact that in the foregoing experiments a larger amount of urea was generally found in the blood taken from the body after death, than in that abstracted during life, is, of itself, conclusive against any such hypothesis.

ON THE USE OF ALCOHOLIC STIMULANTS

IN

HOSPITAL MEDICAL PRACTICE.

WITH

ILLUSTRATIONS FROM THE RECORDS OF THE ROYAL INFIRMARY
OF EDINBURGH.¹

By W. T. GAIRDNER, M.D.,

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HAVING now for some years acted as one of the ordinary physicians of the Royal Infirmary of Edinburgh, with a full complement of wards for males and females, for fever and for general cases, it occurred to me quite recently, in consequence of the discussions that have taken place as to the treatment of acute disease by stimulants, that it would be of some public advantage were every hospital physician to attempt to arrive at an idea of the general average of stimulants employed by himself in his public practice; keeping in view, at the same time, the character of the cases treated, and the circumstances that have guided him in the employment of these remedial agents. It can hardly be doubted, I think, after what has been already published upon this subject, that the differences between individuals would be very great; so great, indeed, as to reveal real differences of opinion and principle of no small importance. It is probable, therefore, that, from the careful study of these differences, and the calm and scientific discussion of them, some considerable benefit might accrue to general medical practice. I was glad, accordingly, to find, after inquiry, that the books of the Royal Infirmary—carefully kept, from week to week, by the Superintendent, with a view to regulate the consumption of alcoholic liquors—present unimpeachable data on which to base the present investigation. For a series of years the allowance of wines, spirits, and malt liquors—i.e., ale and porter (exclusive of table beer)—in every ward in the house, and the average population of the ward, have been duly registered in a manner that precludes

¹ Read to the Medico-Chirurgical Society of Edinburgh, April 3, 1861.

error, within reasonable limits, as to the consumption of alcoholic liquors.

Of course there is a chance of error to a limited extent. The regulations of the house may have been evaded, and spirits or wine may have been surreptitiously brought in by patients and their friends, or by the officers of the house, in some instances; or, in a very few instances, they may have been openly brought in with the consent of the medical officers. So far as my own wards are concerned, I do not believe that this has taken place in any appreciable degree—certainly not to such an extent as to vitiate the official average—except in one ward, which was only occasionally under my control, and in which a very considerable abuse was detected and remedied about a year ago. I have avoided including within the present statement any of the data so vitiated.

Another source, not indeed of error as to facts, but of possible fallacy as regards conclusions, is to be found in the circumstance that the administration of alcoholic liquors is to a considerable extent regulated by the resident physician, to whom the making up of the diet rolls is confided, and by whom, in the absence of his principal, or when uncontrolled by the express instructions of the latter, wines and spirits may be ordered and kept on the rolls at discretion. There is little doubt that, in the management of a large number of patients, through the instrumentality of young physicians who have in the first instance often their duty to learn, and whose period of service rarely much exceeds six months, there may be occasionally a certain latitude in the administration of stimulants, not sanctioned by the opinion of the principal; and, in particular, that spirits and wine, once given, are apt to be continued on the rolls too long, that is, after the emergency for which they were ordered has passed away. Still, as errors of this kind are the direct consequence of deficient control, or of a want of a proper understanding between the principal and his subordinate, I see nothing for it but for the former to take the responsibility of what is done in his name by the latter. Of late years, inadvertent errors in this direction have been in some degree checked by the monthly statement submitted on the part of the managers to the physicians and surgeons, of the total expenditure of alcoholic stimulants in each ward; and I believe, in my own case, that the variations from this cause are within moderate limits, though, doubtless, it has often happened that the allowances would have led me to consider expedient. It is only necessary, therefore, to regard the averages, which will be presently cited, as being in some degree *in excess* of the direct instructions of the physician, and the true bearing of this source of fallacy will be appreciated as nearly as may be.

I have now before me the rather difficult task of attempting to trace, retrospectively, the principles by which the administration of alcoholic stimulants has been guided in my wards, in connection

with the details which I shall present, and of which it may very safely be said that they have accumulated without the least thought on my part of the use that was to be made of them. In doing so, I shall have to claim the indulgence of my friends among the "nephelists," or teetotallers, as well as among the stronger heads of the opposite party, for not a few exhibitions of what they may regard as want of clear reasoning up to conclusions, or insufficiently firm hold of principles. But if I am compelled to treat many important questions as undecided, I am at least still open to conviction, inasmuch as I do not pretend to put forth these details of practice as models, but rather as landmarks and bases for future investigation. The truth is, that, not having as yet formed, or pretended to form, any ultimate theory of the action of alcoholic stimulants in disease, I have always regarded their administration in practice as a sort of compromise of physical and moral considerations, often opposed to each other; and I am quite willing, therefore, in submitting candidly my own motives of action, to consider them as open to modification in either direction.

My object will be, in the first instance, to make the figures before you speak for themselves rather than to make any broad statements of personal opinion, which might be open to the objection of being framed after the view of the facts. The tables now presented to your notice are the condensed result of some much more elaborate ones in my possession, giving the total consumption of wines, spirits, and malt liquors in each of my wards during each month of the last five years, with the average population of the wards, and the daily average consumption per patient carefully calculated from the two preceding data. I have thought it sufficient to exhibit here the annual averages, founded on the total consumption for the year, divided by the number of days and by the mean number of patients in each ward, as ascertained by the regular weekly census of the whole hospital.

The results are, I confess, in some respects such as I did not anticipate. In two particulars, indeed, they are most unexpectedly opposed to all my previous impressions. I fully believed that the use of alcoholic stimulants in my hands had been at least stationary, if not decreasing. It appears, on the contrary, that there has been a nearly uniform increase (taking wines and spirits together) over the whole period under review. The increase is greater, too, in the female ward than among the males. The quantity of wine in both male and female general wards reaches its maximum in 1859; but in 1860, with a slight decrease in the average of wine, there is a decided and much more than commensurate increase in the quantity of spirits. So that, on the whole, the consumption of 1860, in the general wards, is decidedly greater than that of any previous year.¹

¹ It is just possible that one cause of the increase may be a greater urgency and importance of the cases under my care, as a longer period of service naturally brings with it a larger proportion of such cases. I cannot, however, posi-

In another point of view the results of the table are rather startling. It appears that, among the women in the general ward, the consumption of wine during the entire period of five years is decidedly greater than in the corresponding male ward; while that of spirits is very little less, on the whole, than in the male ward, and malt liquors are at least as much consumed among the women in sickness as among the men. As the two wards are very much alike as regards the class of cases admitted, and have been during very nearly the whole period subject to exactly the same administration, it is hardly possible that this fact can be accounted for otherwise than by the apparent need for the exhibition of stimulants, and the demand for them, having been actually greater among the women under my care than among the men. I am quite certain, at least, that there is nothing in the personal convictions of myself or my assistants to account for the fact of so liberal a comparative expenditure of wine on the female side, inasmuch as all my own prejudices and those of most other people were assuredly in the opposite direction, viz., to the effect that women ought to require, and in the better classes of society do require, a less quantity of alcoholic stimulation than men when suffering under disease, and especially acute disease.

It is matter for serious consideration, and on which I prefer to avoid dogmatizing too much at present, how far this increase of alcoholic stimulants in my hands has been a genuine and necessary result of experience of their usefulness; how far, therefore, it is an example to be followed; or how far, on the contrary, it may have gone at any period in the direction of an excess. It is probable that more accurate comparisons with the results of the hospital practice of others similarly recorded may one day lead to more fixed views on this subject. In the meantime, I can only assure the Society that I am conscious of no prejudice, either for or against the administration of stimulants, which should have interfered with the teachings of experience in the matter. Both on economical and on moral grounds, certainly, it has always been my object to keep the supplies as low as appeared to me consistent with the comfort and welfare of the sick. This has been particularly the case with the stronger spirits, which, I believe, have never been given under my orders without what was considered to be an urgent necessity, founded either on the previous habits of the patient, or the extreme character of the symptoms. I have been particularly careful, in most instances, to avoid giving whisky, or even wine, to the young; and, even in fevers, have never done so without a very positive and urgent reason. By far the greater number of the persons under 20 or even 30 years of age who have passed through my hands have

tively assert that this is so to such an extent as to account for any considerable part of the increase; and, on the whole, I incline to view the increase as indicating a really increased confidence in the employment of these agents as remedies within the limits assigned in the sequel.

indeed, I am very sure, had no stimulants at all; and, when these have appeared to be required, a few ounces of wine for a short period, or a little table beer or light ale, has usually amply served the purpose. The same rule of caution in the administration of the stronger stimulants has been followed, so far as appeared reasonable or possible, at the more advanced ages; but it has not been considered expedient in all cases to deal rigorously with habits already in existence, and confirmed by years of indulgence previously to admission; nor has it been considered right to deny the reasonable use of the stronger stimulants to the dying, when this indulgence has obviously tended to the relief of suffering without producing any approach to intoxication.

In accordance with these views, it will be observed that the proportion of the stronger spirits to wines is, on the whole, much lower in these returns than, I believe, will be found usual in hospital practice. I have always, in fact, preferred the more expensive luxury to the cheaper one in all cases in which a choice was at all possible, and especially in the case of convalescents and young persons of sober habits. The risk of implanting bad habits of habitual indulgence (which every physician should most religiously guard against by every means in his power) has always appeared to me to be less when the stimulant used is one beyond the ordinary resources of the class frequenting the hospital; when, therefore, it is clearly seen to be a temporary allowance, granted for special and temporary ends during the period of sickness. On this ground chiefly, or indeed almost exclusively, I have not hesitated to charge the Infirmary with the heavy expense of wine in many cases where spirits would possibly have answered every purpose of mere stimulation; and, on the same ground, I recently felt it to be a duty to oppose the suggestion of a committee of the managers as to the substitution of spirits for wine in most cases. A good bitter beer or light ale would probably be far preferable to either, whether in convalescents from acute diseases, or in most chronic affections. But the habits of the Scotch labouring population are not readily changed; and beer is, unfortunately, seldom relished as a substitute for either wine or whisky. Still, it should be observed that table beer is used in considerable quantities, which do not appear in the official returns submitted with this paper.

It would be going beyond the bounds proposed in this review of facts to enter into a general theoretical dissertation on the medical uses of alcohol, or on its physiological action. But I can hardly avoid stating, what indeed it is no less than the duty of every one who so believes to state, that I have been throughout guided in the use of alcoholic stimulants by the conviction that they are really *stimulants* and *tonics*—i.e., *medicines*—and not food, properly so called. The maintaining of the opposite view by so high an authority as the late Dr Todd, and the practice founded upon it of giving these stimulants, as a general rule, at an early period of acute

diseases, and in very frequently repeated doses all through the day and night, I cannot but regard as a grave error, leading to the probability, almost the certainty, of an injurious excess in their use. My own habitual practice has been to give stimulants, if at all, only in very moderate quantities along with the food, and, in general, as an aid to the digestion of food—the only exceptions being in the case of persons largely and habitually dependent upon stimulants from old and formed habits, and in a comparatively small number of acute cases for a very few days, sometimes only a few hours, to help the system over a dangerous crisis, or to co-operate with other needful remedies, such as antimonials in pulmonary inflammation. I feel quite assured that there have been no such facts in my experience of alcoholic stimulants as are mentioned in Dr Todd's last volume—*e.g.*, brandy, at the rate of 6 drachms every hour, given to a girl of 17 years of age in rheumatic inflammation (Case of Jane Cook, LXXIV); or, in another case (Sarah Butcher, LXXI, age and habits not stated), a pint a day of brandy for a month together in pyæmic inflammation. Such facts are, I cannot help thinking, the indications of a great excess, if not of an entirely wrong direction, in the use of these powerful remedies; which, to be powerful for good and not for evil, must be maintained strictly within the limits of their medicinal action, and given, not as *being food* in themselves, but rather as *adjuncts to food*—*i.e.*, as aids to the gastric digestion, and stimulants of the nervous system and circulation. I quite agree, however, with Dr Todd in thinking that when stimulants really act beneficially in acute disease, they diminish the frequency of the pulse and restrain the tendency to delirium, while improving the appetite and producing an amendment in all the general symptoms. On the other hand, I cannot but demur to his inference that delirium and other bad symptoms, even if increased under small doses, are to be kept down by giving much larger quantities. To regard flushing of the face and increased feverishness, for example, as not a contra-indication, but a reason for increased administration (Lecture viii., p. 269), is opposed entirely to the practice I have followed; for, under such circumstances, or even when stimulants have not been obviously followed by reasonably good effects after a cautious trial of small quantities, I have nearly always abandoned them at once as being unsuitable remedies, at least for the time; and, accordingly, it has never occurred to me to have to "sluice the head well with cold water," or to use any of the other means recommended in Dr Todd's fourteenth lecture, in order to distinguish "the coma of alcohol" from "the coma of disease" in cases of accidental over-stimulation.

Let me add, that another opinion prominently put forward in Dr Todd's book appears to me to demand qualification, *viz.*, that "it is far more dangerous to life to diminish or withdraw alcohol than to

¹ *Clinical Lectures on Certain Acute Diseases.* London, 1860.

give too much." So far from having had constantly before me the fear of sacrificing life by diminishing or withdrawing a habitual allowance of stimulants, I have made it part of my regular practice to do so in most cases of persons accustomed to the use of ardent spirits in excess, and especially in many cases of delirium tremens, or of other acute diseases modified by alcoholic excesses; and though not committed to the treatment of any disease entirely without stimulants, I can entirely corroborate the remarkable statements made by Dr Pridie in his very important memoir on delirium tremens,¹ *viz.*, that the suppression of the habitual allowance is not, *per se*, dangerous in most cases; but, on the contrary, extremely conducive to the cure. In regard to other diseases, I believe that much more mischief is done by the routine administration of stimulants than could possibly result even from their entire suppression in hospital practice. In the case of young persons affected with fever, indeed, it has been almost made matter of demonstration by statistical data, that the stimulating practice, pursued on Dr Todd's plan, was the opposite of useful in the saving of life.² Nor can it be doubted that in pneumonia, as well as in fever, Dr Todd's practice, as recorded by himself, was less successful than that of many practitioners who are less liberal in the administration of alcoholic stimulants. It is quite true that comparisons cannot always, or indeed often, be drawn with accuracy from limited numbers of cases in different spheres of observation; nevertheless, I feel well assured that the recorded mortality from the cases of pneumonia or fever occurring under Dr Todd's care in King's College Hospital, would be regarded as excessive in the Edinburgh Royal Infirmary. On this subject it is sufficient for the present purpose to refer to the "Five Years' Hospital Experience of Pneumonia," published in the *Edinburgh Medical Journal* for March 1860; and to the "Clinical Notes on Fever," in the same Journal for July 1859, although neither of these papers were compiled with a view to the present inquiry; nor would it be fair to Dr Todd to assume that his cases were parallel to those referred to in these papers.

I may, in conclusion, be permitted to refer to the *Edinburgh Medical Journal* for February 1858. The review of the arguments at p. 735, *et seq.*, in regard to the medical uses of alcohol, is so expressive of the views I have all along held on this subject, that I am well satisfied now simply to lay claim to the authorship of this anonymous paper as evidence that the opinions now put forward had been carefully considered long before this paper was thought of. The recent progress in the physiology of the subject, and especially the re-

¹ *On the Pathology of Delirium Tremens, and its Treatment without Stimulants or Opium.* Edinburgh, 1854.

² See Dr Murchison's interesting and convincing arguments in the *British and Foreign Medical Review* for October 1860. The subsequent controversy in the *British Medical Journal*, and *Lancet* for November, appears to me only to strengthen the original case.

searches of Lallemand, Perrin, and Duroy in France, and of Dr Smith of London, appear to me to be in general confirmatory of the ideas expressed in that review; although, no doubt, some of the questions in controversy may still for a long time remain open to discussion on points of detail.

The object of this paper will be served if it shall be the means of procuring more accurate records than hitherto of the actual expenditure of alcoholic stimulants in hospital practice. Considering the vast moral issues involved in this question, and considering also the important economic interest which the governors of our public charities have in keeping within reasonable bounds the administration of stimulants, it is surely not too much to suggest that in every hospital in this country monthly returns should be made, exhibiting, as in the Edinburgh Royal Infirmary, the aggregate expenditure in each ward, and also a calculated average of the amount supplied to each individual patient. By such averages, physicians would be insensibly guided to the truth; and the results of various practice would, when carefully compared, supply data hitherto wanting for the settlement of a great many scientific questions connected with alcoholic stimulants.

AVERAGE DAILY CONSUMPTION OF ALCOHOLIC STIMULANTS PER PATIENT DURING FIVE SUCCESSIVE YEARS, IN THE ROYAL INFIRMARY, WARDS 4, 15, AND 16.¹

	1856.	1857.	1858.	1859.	1860.
GENERAL WARD, Males—					
Wines (ounces) . . .	0.158	0.465	0.710	0.928	0.739
Spirits (ounces) . . .	0.056	0.312	0.287	0.184	0.154
Malt liquors (pints) . .	0.039	0.040	0.023	0.053	0.058
GENERAL WARD, Females—					
Wines (ounces) . . .	0.446	0.534	0.799	1.498	1.200
Spirits (ounces) . . .	0.295	0.312	0.223	0.164	0.510
Malt liquors (pints) . .	0.064	0.069	0.048	0.061	0.048
FEVER WARD, Females—					
Wines (ounces) . . .	0.715	1.256	1.734	1.725	1.140
Spirits (ounces) . . .	0.069	0.083	0.346	0.052	0.153
Malt liquors (pints) . .	0.023	0.029	0.135	0.069	0.027

¹ In regard to these latter wards, it is to be observed, that in the middle of 1858 a change took place in the distribution of fever and general cases, which has been carefully kept in view in reducing the monthly to annual average. The tabular statement, therefore, does not correspond with the results of either ward separately calculated.

ON

THE OCCURRENCE OF DEPOSITS OF CRYSTALLISED PHOSPHATE OF LIME IN HUMAN URINE.*

BY

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DR. HASSALL seems, so long ago as 1852, to have satisfied himself that phosphate of lime appeared occasionally in urine in a crystalline state; for, in his little work on the urine, the eighteenth plate contains a delineation of "crystals of phosphate of lime," and the date appended to the drawing is August, 1852.

In the beginning of last year, Dr. Hassall read a paper before the Royal Society, "On the Composition and Pathological Importance" of the calcareous phosphates occurring in the urine as a spontaneous deposit of stellar crystals. He found these crystals to consist of biphosphate of lime; he also considered them of more frequent occurrence and of far graver signification than the triple phosphate of ammonia and magnesia.

* This Paper was published without the figures in the *British Medical Journal* for March 30th, 1861.

My attention had been directed to these crystals with curiosity for some years; and recently I have had an opportunity of subjecting them to chemical analysis. Before the publication of Dr. Hassall's observations they were universally considered to have the same composition as the prismatic phosphate, which contains ammonia and magnesia, but no lime; and it was believed that the phosphate of lime never assumed the crystalline state in urine, but was always deposited as an amorphous sediment.

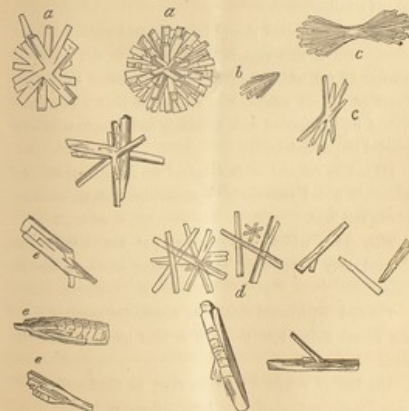
The forms of the crystals in question present considerable variety; but they are, nevertheless, easily recognised by the practised eye. Good figures of them are given by Dr. Beale in his twenty-second plate, although erroneously designated "A Rare Form of Triple Phosphate;" still better ones have been published by Dr. Hassall in the little work already alluded to; also in the *Lancet* for 1857, as well as in the paper communicated by him to the Royal Society, January, 1860.

Dr. Bird had evidently seen these crystals, and he gives imperfect figures of them. (Fig. 45.) He held them to be of the same composition with the triple phosphate.

In the atlas accompanying Lehmann's *Physiological Chemistry*, Funke gives two capital figures in his twelfth plate, but he names them uric acid.

The prevailing appearance is that of crystalline rods or needles grouped round a centre, so as to form a more or less perfect star. Some of these stars are of great beauty, as large as the finest seen of uric acid, and not unlike them, except in the absence of a brown colour. Sometimes, the rays of the star are so close set that the appearance of a rosette is pro-

duced (See fig. aa). Again, not unfrequently, the crystals, instead of radiating equally in all directions from the centre, shoot out in one direction only, so as to form a fan (b); or, in two directions, on opposite sides, so as to present a rude resemblance to a wheat-sheaf (c). Sometimes, too, the rods lie confusedly across each other (d), or crystallise round hairs or fibres. Frequently, the rods are wedge-shaped, and thicker at their outer extremity than where they join in the centre. Other forms are club-shaped and bottle-shaped (e), and abundantly marked with lines of secondary crystallisation, reminding one of the same appearance on the hexagonal plates of cystine.



In a case of diabetes recently under my care, these crystals formed a constant deposit. The urine had been brought down by appropriate treatment to fifty ounces a day, and the patient was steadily gaining strength and flesh. The deposit was often mixed with oxalate of lime, and sometimes with uric acid; but never, except as the result of putrefactive decomposition, with the triple phosphate. I managed to collect about two grains of the crystals in a pure state, unmixed with oxalate of lime or triple phosphate, and subjected them to examination, as follows:—

I.—Treated with caustic potash they evolved no ammonia; nor were they in any way affected even by boiling in the caustic alkalies.

II.—They dissolved slowly in acetic acid without effervescence. On adding oxalate of ammonia to the solution a precipitate fell, which, two days after, presented the familiar forms of minute octahedra of oxalate of lime. The supernatant liquor, treated with ammonia and phosphate of soda, yielded no precipitate.

III.—The mineral acids dissolved the crystals rapidly, and ammonia added afterwards in excess threw down an amorphous sediment.

They did not lose weight by drying at 212° . When incinerated they lost weight to the extent of twenty-six per cent.

The first experiment shows the absence of ammonia; and the second shows that the base is lime unmixed with magnesia.

The loss of weight on incineration, in the fourth experiment, is not entirely due to water. Some of it must be laid

to the account of the organic matter entangled in the crystals, which caused them to assume a deep brown colour when burned. Taking this abatement into consideration, the loss of water is about what would take place if the crystals had the same composition as the crystallized phosphate of lime, described by Berzelius as formed when common phosphate of soda is added drop by drop to a solution of chloride of calcium. This salt contains twenty-two per cent. of water, or four atoms, one only of which is basic, the other three being water of crystallisation. On this supposition, therefore, the stellar phosphates have the following formula: $2\text{CaO}, \text{HO}, \text{PO}' + 3\text{HO}$.

By adding a little chloride of calcium to healthy urine, and reducing its acidity to near the neutral point with caustic soda, I have often succeeded in obtaining an abundance of crystals closely resembling those occurring spontaneously in urine. The re-action of the urine, in which I have found the crystallised phosphate of lime has been generally faintly acid, often nearly neutral, and more rarely alkaline from fixed alkali.

The appearance of a crystalline deposit of phosphate of lime in urine is not common. It is, in fact, a rare deposit as compared with oxalate of lime, uric acid, or the amorphous lithates. Dr. Hassall thinks it is more common than the triple phosphate, if decomposed urines be excluded. Practically, however, the reverse is true. Triple phosphate is frequently seen in the urine of the sick, even a few hours after emission, in a multitude of cases where there are none of the ordinary signs of decomposition or putrefaction. Triple

phosphate is also very commonly seen in urines made alkaline by remedies, after standing a short time.

The presence of the stellar phosphates in quantity is, so far as my experience enables me to judge, an accompaniment of some grave disorder. In addition to the case of diabetes already mentioned, I have seen the stellar phosphates in cancer of the pylorus, once in phthisis, and more than once in patients exhausted by obstinate chronic rheumatism. They may, however, under peculiar conditions, be precipitated in a healthy urine. When the urine is rich in lime, and its acidity is, at the same time, depressed to near the neutral point, stellæ of phosphate of lime may form quite independently of any grave disorder, merely as the result of a coincidence in the chemical composition and re-action of the urine. For example, after a full meal, the acidity of the urine becomes greatly reduced, and lime derived from the food is in excessive proportion. In such circumstances, I have several times detected stellæ of phosphate of lime, but only in scanty numbers. A depressed acidity of the urine is an essential contingent to the formation of these crystals; and if the urine, subsequently to their formation, increase in acidity, they may spontaneously disappear.

The persistent occurrence of this deposit in the urine may lead to the formation of stone in the bladder. Calculi of pure phosphate of lime are exceedingly rare, and have quite a different history from the common phosphatic stone of triple phosphate or fusible matter. The latter is always precipitated upon some previous formation as a secondary deposit, and is produced by decomposition of the urine. Hence, vesical

calculi that have long resided in the bladder, and have been the source of violent and repeated catarrh of that organ, are covered externally with a phosphatic crust. But the pure phosphate of lime is a primary formation; and, like other primary formations, it may alternate with uric acid or oxalate of lime. There is a beautiful specimen in the museum of the Manchester Infirmary, where phosphate of lime alternates with pure uric acid. The condition of urine accompanying a pure phosphate of lime stone has not, I believe, been made the subject of direct observation; but one would expect that it would be highly significant of the nature of the concretion. I should expect the following: a urine of pale colour, either feebly acid, or alkaline from fixed alkali (not from ammonia), with a deposit of stellar phosphates mixed with amorphous phosphate of lime, and absence of triple phosphate and signs of vesical catarrh.

SOME OBSERVATIONS
ON THE
SENSIBILITY OF THE EYE TO COLOUR.

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SOME OBSERVATIONS

ON THE

SENSIBILITY OF THE EYE TO COLOUR.*

If, closing one eye—say the right—any highly luminous white ground, such as some portions of the sky on a sunny day, is viewed with the left through a dark tube, so as to exclude all extraneous light, after a little the eye will begin to feel fatigued, and a vibrating circular smoky spectrum will be perceived at the end of the tube. When the tube is laid aside and both eyes are directed to the sky, a similar spectrum will be observed, projected, as it were, on the surface of the heavens, but much darker. But if after a time each eye is alternately opened and closed, a rose-coloured spectrum is seen with the left eye, a pale-green one with the right. These appearances are seen still better if, instead of the sky, a white screen is used as the plane of projection in the second part of the experiment. At first an almost black circular disc is seen; this becomes lighter and lighter, till it is finally succeeded in the left eye by a bright rose-colour disc, surrounded by a violet border; in the right eye by an equally bright green, with a rose border. These spectra sometimes appear as if upon the surface of the screen, sometimes, on the contrary, as if originating within the eyeball itself, and indeed may be even seen with both eyes closed. To see the above phenomena in all their intensity, a slightly different plan must be adopted. As the field of projection, a sheet of dead black paper in a dark room is to be used; the spectra then seen with either eye are the same, and their colours most splendid, both as regards brightness and tint. At first an emerald-green disc appears, surrounded by a narrow carmine, or perhaps, more accurately, magenta border; the magenta tint is then seen to encroach more and more upon the green, till the whole disc is of the former colour, surrounded by a bluish-violet border; this last, in its turn, invades the magenta, till the final spectrum is of one uniform indigo-violet colour.

* Since writing this paper, my attention has been directed to a series of elaborate disquisitions by Brücke and Fechner in Poggendorff's *Annalen der Physik u. Chemie*, vols. lxxxiv., l., and xlii., to which I beg to refer my readers.

The above is the general sequence of colours which I, and other persons whom I have asked to perform the experiment, have observed; but these are liable to exceptions. Occasionally, the librating spectrum observed at the end of the tube in the first part of the experiment, acquires a faint rose, green, or violet tint. Sometimes, I have seen the spectra of the right and left eyes, in the second part of the experiment, reversed as regards colour.

These facts appear to prove the following propositions:—

1. That colour sensations may be excited in the retina, or brain, altogether independently of any external colour-stimulus.

2. That as an optical analysis of white light may be effected by a prism, so with the eye we possess the power of effecting, what may be called, its physiological analysis.

3. The last proposition tends to the conclusion that white light consists of three fundamental colours—magenta, emerald-green, and indigo-violet—corroborating in a remarkable manner the opinions of Professor Maxwell and Dr. Young on the same subject.

4. That a colour sensation excited in one eye is generally felt in the other, although this latter has not been exposed to the influence of light in any part of the experiment; that, in a word, a very close sympathy exists in the two retinas, of which the simultaneous action of the two irides is probably but a reflex nervous consequence.

I may here allude to a distinction in ocular spectra, which has, I believe, not been taken much account of by observers of these phenomena. Some spectra seem as if projected on the plane to which we direct the eye, and in that case appear, as I have found from numerous measurements, linearly magnified in proportion to the distance of the eye from the plane of projection. Other spectra, on the contrary, are perceived, so to say, in the eyeball itself, and are of a subjective nature. Independent of the differences of their apparent seats, the two classes of spectra present certain other well-defined distinctions. Projected spectra are only perceived with the eyes open, and are generally but faint in colour; while subjective ones may be seen with the eyes shut, and are always intense in colour. At the same time I am disposed to ascribe the differences of colour, in a certain degree, to the diluting influence of extraneous light; for projected spectra are always seen more vivid in a dark room than in daylight.

The green spectrum observed on a sheet of white paper, after prolonged contemplation of a red wafer, has been commonly explained thus—"When the eye has been for some time fixed on the red wafer, the part of the retina occupied by the red

image is deadened by its continued action, and insensible to the red rays which form part of the white light from the paper; consequently will see the paper of that colour which arises from all the rays in the white light of the paper, but the red; that is, of a bluish-green colour, which is therefore the true complementary colour of the red wafer."

That this explanation is not correct seems to me to be proved by the following experiment:—

I, at night, made a room (which is provided with thick American-leather blinds for ophthalmoscopic purposes), to all appearances, absolutely dark, then viewed with the left eye a small aperture in a dark box covered with a piece of emerald-green glass, behind which was the nearly white flame of a lamp. The right eye was kept closed, and covered with a thick handkerchief. After a time I blew out the light in the box, and looked at a screen covered with a sheet of dead-black paper. With the left eye a large carmine-coloured projected spectrum of the flame could be seen; with the right eye I generally perceived no spectrum at all, or if any, but of a very faint tint. But, if the latter eye was exposed to a white light during the first part of the experiment, I invariably perceived the same spectrum with this eye as I did with the left one.

This experiment shows that the presence of white light is not necessary for the perception of complementary ocular spectra, and further would appear to indicate that for a sympathetic spectrum to be excited in the eye which has not been exposed to the colour-stimulus, the excitation of some light is necessary.

M. Plateau painted one half of a piece of paper red, the other green; and after alternately directing the eyes to each half, covered them with a handkerchief, and observed a black image, having on each side a complementary coloured image.† He hence inferred that "the combination of accidental colours produces black." Sir D. Brewster very properly objects to this conclusion, "because the eye has been in succession rendered insensible to the two colours which compose white light itself."‡ Elsewhere the same author says, "If we take the two complementary colours, namely, the red and the green tints forming the ordinary and extraordinary pencils in the polarized ring, which, by overlapping, form white light, then it is manifest that the accidental colour of the overlapping part is black, and hence the sum of the action of the red and green acting separately must also be black."§

* Brewster's Optics, 1831, p. 305.

† Annales de Chimie for 1833.

‡ Lond. and Edinb. Phil. Magazine for May, 1839, p. 335.

§ Op. Cit. for December, 1839, p. 437.

Notwithstanding, however, the authority of Sir D. Brewster, the following experiment which I have performed appears to me rather to corroborate Plateau's view. If the two halves of a card painted red and green respectively, be illuminated by a green or red light, they appear black. In the same way, but depending on a different cause, the two halves of the card, if viewed through green or red glass, appear black.

Another set of observations, connected in a degree with the preceding, may be here noticed. Chevreul* distinguishes two chief species of contrast of colours, *simultaneous* and *successive* contrasts. But an examination of these distinctions shows them in my judgment to be more apparent than real, and but the expression of one fundamental fact, viz., that the eye on perceiving any one colour acquires a tendency to see its complementary. Thus, to take an example of Chevreul's simultaneous contrast:—If a strip of red and one of yellow paper be viewed side by side, near the line of contact the red paper inclines to violet, the yellow to green. The rationale of this is at once obvious: the red mingling with the complementary of yellow, i.e., blue, produces the violet tint; whilst the yellow mingling with the complementary of red, i.e., green, produces a light green; and this same law holds good in the juxtaposition of any two colours whatever. By the term successive contrast Chevreul designates the familiar phenomena of complementary ocular spectra, of which a most comprehensive history has been given by Darwin in the *Philosophical Transactions*, vol. lxxvi. p. 33, *et seq.* Du Tour† thought that the two eyes cannot perceive each a separate colour at once. He says that if, e.g., a blue disc be presented to one eye and a yellow one to the other, the result is that the mind perceives alternately the one or the other colour, but not the two at once. But I would submit that these two statements do not include the whole facts of the case. I took two tubes, each 10½ inches long, and applying the end of one to each eye, viewed the sky through them. I found that when the contiguous edges of the tubes at their further ends were some inches apart, two distinct white circles of sky were seen; these circles touched when the edges of the tubes were from 2¼ to 2½ inches apart, and, when closer, the two circles appeared as one. If now the further end of one tube was covered with a piece of green glass, the end of the other with a piece of red, as long as the ends of the tubes were kept not closer than 2¼ to 2½ inches asunder, the two coloured discs were perceived perfectly distinct

* "The Principles of Harmony and Contrasts of Colours," by M. E. Chevreul.
† *Mémoires de Mathématique et de Physique présentés à l'Académie Royale des Sciences*, iii. p. 514, iv. p. 429; Paris, 1760-63.

from one another; no alternation of either colour to the exclusion of the other, as in Du Tour's experiment, ensued, so long as the tubes were inclined to each other at this or any greater degree of divergence.

Another very interesting series of phenomena depending on the intrinsic sensibility of the eye to the impressions of colours, are those of coloured shadows. The first exact observations on these were made by Count Rumford.* He observed that the two shadows of an object placed in front of a white ground, from a white and a coloured light, were of the two colours complementary to the latter. I have investigated this fact a little more closely. The method adopted has been to throw a white and a coloured (red) circle of light from two magic lanterns on a white screen, before which a slender wooden rod was placed. It is easy to satisfy ourselves that the red shadow is produced by the (otherwise colourless) shadow cast from the interception of the white light being simply illuminated by the other red light. The green shadow is the shadow produced by the interception of the red light, illuminated by the white light. These coloured shadows have, by Rumford and many subsequent observers, been ascribed to the effect of contrast. But this appears an inadequate explanation; for if, with one magic lantern, a half-white and a half-red circle of light be thrown on a screen, a shadow thrown across the two fields is simply dark, without any colour at all. If, again, a red and a white disc of light be thrown from two magic lanterns respectively on a screen, so as partially to overlap, where the overlapping takes place two complementary shadows of any object are seen, but in the other two parts of the field only one colourless dark shadow is seen.

The following facts seem to form the basis of the explanation of coloured shadows:—Firstly, the experiment of Rumford†—that a piece of grey paper placed next to a piece of coloured paper, both on a black ground with the exclusion of extraneous light, appears tinged with the complementary colour. Secondly, I found by my own experiments that if, in a dark room, the screen is illuminated with a red circle of light from a magic lantern, the greenness of the shadow and the redness of the ground on which it appears are inversely proportional to one another. By approximating the red light to the screen this becomes less redder, whilst the shadow of the rod placed before it becomes less green and darker, till it becomes an ordinary black shadow; that, on the other hand, removing the red light till it leaves the white screen but

* *Philosophical Papers* by Benjamin, Count of Rumford. London, 1802, vol. i. p. 333.

† *Op. Cit.*, p. 336.

faintly tinged with red, brings out the green shadow very prominently, and on admission of light into the room, a second faint red shadow comes out.

Meusnier observed "that when the sun shone through a hole a quarter of an inch in diameter on a red curtain, the image of the luminous spot was green." Another observer, Mr. Smith of Fochabers,* states, "If we hold a narrow strip of white paper vertically, about a foot from the eye, and fix both eyes upon an object at some distance beyond it, so as to see it double, then if we allow the light of the sun, or a light from a candle, to act strongly upon the right eye without affecting the left, which may be easily protected from its influence, the left hand strip of paper will be seen of a bright green colour, and the right hand strip of a red colour."

From all these facts, I think the conclusion arrived at by Sir David Brewster appears highly probable, that "as in acoustics, where every fundamental sound is actually accompanied with its harmonic sound, so in the impressions of light, the sensation of one colour is accompanied by a weaker sensation of its accidental or harmonic colour."† To this might perhaps be added, that there is a tendency in the eye to, as it were, decompose white light into two complementary colours; and further, that the predominant decomposition is into red and green.

Applying this theory to the phenomena of coloured (*e. g.*, red and green) shadows, the red shadow has already been shown to be simply due to the illumination of a colourless shadow by a red light; whilst, on the whole of the rest of the field of the white screen, the red tint cast from the magic lantern is sufficiently powerful to overcome the green tint which the eye would otherwise perceive, excepting at one spot—namely, that which does not receive any red light on account of the interposition of the opaque rod. Here the green (harmonic) colour, having no antagonistic red to overcome it, is rendered sensible to the eye.

* Brewster's Optics, p. 405; London and Edinburgh Philosophical Magazine for October, 1832, vol. i. p. 249.
† Brewster's Optics, p. 309.

ON THE

TIME AND MANNER OF CLOSURE

OF THE

AURICULO-VENTRICULAR VALVES.

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LONDON:

JOHN CHURCHILL, NEW BURLINGTON STREET.

MDCCLXI.

ON THE
TIME AND MANNER OF CLOSURE

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AURICULO-VENTRICULAR VALVES.

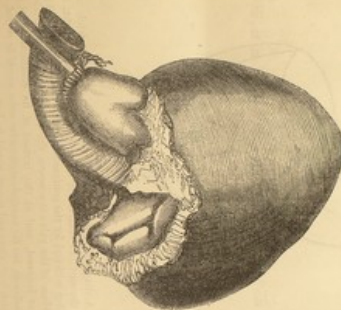
THERE are two things necessary to the closure of these valves; viz., the shutting down of the semilunar valves and auricular contraction.

When the auricle is about to inject the ventricle, the latter is empty and contracted, with its distal or ventriculo-arterial valves firmly shut down by the pressure of the blood upon their upper surfaces. Immediately the auricle contracts, its contained blood passes into (distending and lengthening) the ventricle; the force which it transmits not being sufficient to overcome the arterial pressure and weight of blood upon the upper or arterial surface of the semilunar valves, is expended in distending the ventricle and closing the auriculo-ventricular valve, which then forms one of the walls of the ventricle. (See figs. 1, 2, and 3.) To this succeeds the ventricular contraction; the auriculo-ventricular valve, being already closed, now becomes tense, the pressure in the ventricle overcomes that in the artery, and the semilunar valves are raised.

That the above is correct is proved as follows:—If we cut away the auricle and clear out the coagulum from the

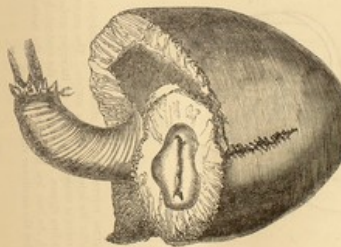
ventricle, on placing the heart in fluid we find that the flaps of the auriculo-ventricular valve are borne up to a certain extent towards the auriculo-ventricular opening (see fig. 4); but the latter does not become closed without employing force, as with a stream of water thrown with a syringe from the direction of the auricle, and this closure is not perfect nor sustained without first shutting down the semilunar valves, which is best done thus: for a human heart, an elastic self-sustaining syringe, with a tube of like diameter with the aorta or pulmonary artery, is adapted to either vessel, and its semilunar valves shut down, gentle pressure being maintained by the fingers upon the bottle in imitation of the elastic pressure of the aorta or pulmonary artery. If the ventricle be now injected, the closure of the valve is instantaneous, its upper surface becoming convex (not funnel-shaped), and its under surface deeply concave (see figs. 1, 2, and 3). For a bullock's heart a plumber's forcing pump was used, and for a bird's a smaller apparatus, and with the same result in all cases. These and many other experiments upon the valves have been for some years past many times repeated; they have been performed upon the largest hearts under water with powerful apparatus purposely constructed.

FIG. 2.



Front view of the heart; auricles removed. The pulmonary valves shut down and the artery distended. The right ventricle has been treated as the left in fig. 1, and the tricuspid valve closed.

FIG. 1.



Back view of the heart; auricles cut away. The semi-lunar valves having been shut down and the aorta distended with water. The ventricle was filled with water with a syringe from above, and the mitral valve closed.

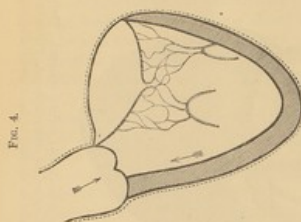


FIG. 4.

A diagram representing the condition of the ventricle and of the auriculo-ventricular valve, if, as is generally taught, the latter is closed by the contraction of the ventricle. The upper part of the valve is raised, the capacity of the ventricle is increased, and at the instant of its contraction the semilunar valves are open, but the force of the contraction would be lost, by which both time and power would be lost.

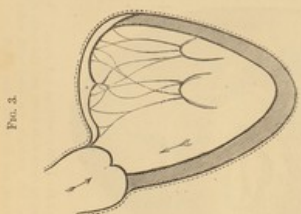


FIG. 3.

A diagram representing the state of the ventricle immediately after its injection by the auricle. The capacity of the ventricle has been increased, its base has been raised, and at the instant of its contraction the semilunar valves are open.

The dotted line represents the pericardium, and the arrows the direction of the pressure.

From this I think it will be at once seen, that the auriculo-ventricular valve must be closed previously to the systole of the ventricle, as in figs. 1, 2, 3, otherwise the ventricle could not thoroughly be filled, nor could the valve itself perform its office properly; for as the ventricle contracts and its base descends, the valve would be made to yield towards the auricle at the very time it is required to be unyielding. But with the valve closed by the end of the auricular contraction, as in fig. 3, there is not only no chance of yielding towards the auricle, but the ventricle is in that condition to exert its power most instantaneously and effectively, as the prime mover—the *fons et origo*—of the circulation. The rapidity and power of its action would be impaired were any of its force expended in a backward direction, had it indeed to close the auriculo-ventricular, previously to opening the semilunar, valves.

But it may be asked, How can the force of the auricular contraction be brought to bear upon the under or ventricular surface of the valve?

The following observations point to the means by which this process is effected:—

1st. If we open a ventricle and cut away the flaps of the auriculo-ventricular valve from their attachments, both to the zona tendinea and to the chordæ tendineæ, on placing the preparation in fluid, the chordæ tendineæ rise up like stems of aquatic plants from the muscoli papillares, and from the sides of the ventricle.

2nd. When the chordæ tendineæ are cut off close to the under surface of the flaps of the valve, the latter are still supported at a certain level in the fluid, and their delicate margins curl upwards towards the auricle. This latter fact is more evident in the right than in the left ventricle, and is attributed by Dr. Markham, who, I believe, was the first to remark it, to the presence of the elastic tissue entering into the structure of the valve. That this curling upwards exists is shown as follows:—

(a.) Supposing a whole valve with the upper part of the ventricle to be cut out so as to form a muscular ring with

the flaps of the valve hanging loosely; on being placed in fluid with the auricular surface *upward*, the flaps are supported and their delicate margins turn up as little scrolls.

(b.) The same preparation being placed in fluid with the auricular surface *downward*, the flaps still rise, but their edges now turn downward, which is not the case with any other part of the valve.

(c.) A piece of the right auriculo-ventricular valve having been cut out from its attachments and placed in water, the above-described curve was maintained for days.

Now, if to the above we add that the ventricle in filling is an enlarging cavity, and that this enlargement and elongation take place below the attachment of the valve to the zona tendinea, and that in this elongation and upward movement of the base the valve participates; and if we remember that, even to the last drop of blood from the auricle, there is a force causing the particles of blood within the ventricle to press upon the under surface of the valve, even after the auricular contraction has ceased (for it cannot be supposed that ever, even for an instant, the blood is at rest), then are we led to the following conclusions, viz. :—

The chordæ tendineæ by position serve to open out and prop up (as stems of water-plants do their leaves) the flaps of these valves; their firm connexion at the circumference with the zona tendinea tends also to their support; moreover, the elasticity producing the upward curl assists in the approximation of their edges; finally, the pressure exerted by the blood from the auricle brings all into play, and their closure is effected.

Who can refrain from admiring this instance of economy of time and power, by which the ventricle is assigned its one function of expulsion, and the very force which fills and distends the ventricle closes these valves? This force is auricular contraction.

17, VICTORIA-SQUARE, S.W.
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RESEARCHES ON THE CONSTITUENTS OF GASTRIC JUICE.

By WILLIAM MARCET, M.D., F.R.S.

In the spring of last year, being engaged with some investigations on gastric juice, I was induced to examine a sample of this secretion in Soleil's saccharimeter; when, to my astonishment, I observed it to act strongly on the polarized ray, rotating the plane of polarization of light to the left. I then undertook a series of investigations in order to determine which was the constituent of gastric juice possessed of the above optical property, and to what amount it exhibited this rotating power in the saccharimeter: these researches form the subject of the present communication.

The gastric juice used in my experiments was procured from two dogs, in whose stomachs I had established permanent fistule. One of these animals had been operated on in December, 1859; in the following month of October (1860), finding my supply of gastric juice insufficient, I established a gastric fistula in the second dog. The animals have both been in very good health ever since.

The gastric juice was obtained after allowing the dogs to fast for thirty or forty hours, and then feeding them with soft bones (i.e., bones containing cartilage) boiled and cut up, or pieces of the windpipes of oxen or sheep—those of the ox being boiled (though not softened), the tracheæ of sheep being usually given raw. Windpipes were most frequently used, as they produced a good supply of the secretion, without causing any gastric disturbance. The secretion was collected from time to time during an hour or an hour and a-half after the animals had been fed, and from two to five ounces were usually obtained from each of them.

The fluid was in every case turbid when first extracted, containing, especially, fat. When the amount of substance in suspension was great, I found it advisable to begin by straining the gastric juice through calico; at all events, it had to be filtered through paper over and over again, until sufficiently clear to admit light freely through a layer of the fluid equal in thickness to the length of the saccharimeter-tube.

Every sample of gastric juice obtained under the above-mentioned circumstances from either of the dogs, rotated the plane of polarization of light to the left to an extent indicated by from 15° or 20° to 40° of Soleil's saccharimeter. The acidity of the

fluid, which was frequently determined, varied in 10 c.c. from 0.011 to 0.039 gramme—these numbers representing the amount of potash necessary to neutralise 10 c.c. of the gastric juice, equivalent to from 0.085 to 0.303 per cent. of hydrochloric acid. This degree of acidity, although rather lower, agrees with that obtained by Bidder and Schmidt (*Verdaunungssäfte*, &c.), which is from 0.1708 to 0.3353 per cent. of hydrochloric acid.

My first impression was, that the optical properties of gastric juice were due to its acid. I therefore examined at once in the saccharimeter a sample of lactic acid prepared from sugar of milk; but it had no action on polarized light. Next, some lactic acid was extracted from flesh according to Liebig's process; this again exhibited no optical property in Soleil's polariscope.

After some consideration, I determined upon trying the separation of the constituents of gastric juice by means of hydrated oxide of lead, with the view of examining the action of light on the substances precipitated, and on those not precipitated, by this agent. Having boiled a sample of the secretion with hydrated oxide of lead in excess, and filtered the mixture, a fluid was obtained having a marked alkaline reaction, but containing only a comparatively small quantity of lead. I submitted this fluid to examination in the saccharimeter, and found it to act powerfully on polarized light. This proved that I had been mistaken in supposing that the action of gastric juice on polarized light was owing to its acid constituent.

Finding that the alkaline filtrate from the boiled mixture of gastric juice and hydrated oxide of lead contained the substance I was in quest of, I naturally believed it to be an alkali, and for a long while I endeavoured to isolate it under the form of crystals or of an insoluble salt. Failing in this, and finding that the substance was precipitated amorphous by alcohol, I determined upon availing myself of this means to prepare it in as pure a condition as possible; and, finally, I obtained a body having a definite rotating power on polarized light, and possessing the chemical properties of the substance known to result from the digestion of albuminous and gelatinous food, which Lehmann has called *peptone*.

Preparation.—The following is the process I adopted for the preparation of this substance, which, however, I could never obtain perfectly free from inorganic matters.

The gastric juice, excited, collected, and filtered as above, was boiled in order to coagulate the albumen (when, apparently,

according to Lehmann,* the pepsine is also precipitated), and then filtered, to be mixed with three times its bulk of commercial alcohol, 56 over proof (sp. gr. 842). A few hours later, or still better, the next day, the fluid was filtered from the precipitate which had formed, and then distilled or concentrated on the water-bath; the composition of the precipitate was subjected to a special inquiry. The residue from the distilled or concentrated fluid had a syrupy consistence; it was treated with a small quantity of sulphuric acid diluted with four or five times its bulk of water and added cold, the capsule containing the syrupy residue being at the time floated on cold water, to prevent any rising of temperature from the action of the acid; alcohol was next mixed with the thick acid fluid until the gelatinous precipitate it induced had ceased to increase, the mixture being at the same time agitated with a glass rod, to which much of the precipitate became attached. The gelatinous substance was now thoroughly washed with alcohol, and then dissolved in water, yielding a solution turbid from the presence of sulphate of lime, which had to be removed by filtration. I now boiled the clear fluid with an excess of hydrated oxide of lead, and after allowing the mixture to remain undisturbed till the next day, it was filtered. The filtrate was obtained clear, although on some occasions it had to be filtered repeatedly; it was then ascertained, with sulphuric acid and sulphide of ammonium, to be free from lead: it had an alkaline reaction, more or less strong, and contained a little lime with traces of soda-salts. I have succeeded, to a great extent, in getting rid of the lime, by adding cautiously to the fluid a very weak solution of oxalic acid, until it became nearly neutral, and then filtering. I also feel confident that the presence of a certain quantity of alcohol in the acid solution, previous to boiling it with oxide of lead, causes the precipitation of nearly all the lime as sulphate; indeed, I have quite recently ascertained that the acid solution in question, mixed with an equal bulk of alcohol, 56 o.p., in the course of two or three days, deposited the whole of its lime as sulphate; the fluid, after evaporation in order to get rid of the alcohol, giving no precipitate with ammonia and oxalate of ammonia, yet the amount of alcohol added had precipitated an insignificant quantity of the polarizing substance. In some instances I found the filtrate from the lead precipitate nearly

* *Berichte über die Verhandlungen der Königlich-Sächsischen Gesellschaft der Wissenschaften zu Leipzig*, 1849.

free from lime, which I attribute to the action of the alcohol which had remained mixed with the gelatinous substance after it had been washed with this fluid, or to all the lime having been precipitated at first by the addition of alcohol to the gastric juice: in these instances it was not necessary to use oxalic acid.

The alkaline filtrate from the lead precipitate, as free from lime as possible, was concentrated to the consistence of a thin syrup, and finally precipitated by alcohol, or a mixture of alcohol and ether. After a day or two the fluid, now become clear, was decanted, leaving a perfectly homogeneous substance, transparent, colourless, or nearly so, adhering to the sides of the receiver under the form of a thick, tenacious syrup, which was dried. The dry substance was very brittle, and exhibited no signs of crystallization; it could be easily scraped off from the sides of the receiver, from which it partly separated in scales; and, being very hygroscopic, had to be preserved in carefully-closed bottles.

Remarks on the above method of investigation.—In every case where gastric juice was obtained, as stated above, it yielded a precipitate by boiling. I made some experiments to determine whether the removal from gastric juice of the substance thus precipitated diminished its action on polarized light; but, on examining the fluid in Soleil's polariscope, before and after the precipitation, I could not discover any loss of its optical properties. When alcohol 56 o.p. (sp. gr. 842) is mixed with clear gastric juice obtained as stated above, which has been boiled and filtered, no turbidity or precipitate takes place until about an equal bulk of alcohol has been added. A further addition of alcohol causes a turbidity, and when the volume of mixture is equal to four times its original bulk, a cloudy precipitate gradually forms, leaving a clear fluid; but the next day the fluid may again have become turbid from the formation of a further quantity of the precipitate. This precipitate was collected on a filter, and, after being washed with a mixture of alcohol and water, was subjected to examination. I was surprised at finding it but partly soluble in water, yielding an acid solution; this solution acted, more or less, on polarized light, but much less than the original gastric juice, showing that some of the polarizing substance (if I may so call it) had been precipitated by the alcohol. For instance, a sample of eight ounces of gastric juice, showing 32.5° in the saccharimeter, being treated with three times its bulk of alcohol, gave a precipitate which exhibited in an aqueous solution measuring two ounces

13.5°; or a quantity of the polarizing substance in eight ounces of gastric juice, equivalent to 260°, lost a proportion equivalent to 27° by precipitation with three times its bulk of alcohol 56 o.p. This aqueous solution acting slightly on polarized light was also examined with respect to its inorganic salts; for this purpose, after evaporation to dryness, the residue was incinerated, and the ash was found to consist of phosphate of lime, with a little sulphate of lime: the addition of hydrochloric acid emitted no carbonic acid. I also examined the part of the precipitate insoluble in water, after it had been thoroughly washed with distilled water. It dissolved readily in gastric juice, but failed to increase its polarizing power. In one experiment where I used the precipitate obtained from no less than fourteen ounces of gastric juice, and dissolved this in one ounce of the secretion showing 23°, this solution gave 24° when examined. As an increase of only 1° is not worth taking into account, it is obvious that the precipitate insoluble in water did not increase the optical property of gastric juice when dissolved in the secretion. The precipitate under our present consideration, insoluble in water, was incinerated, and I found it still contained a comparatively large quantity of organic matter. When burnt to ashes, this inorganic residue contained carbonates, as shown by the ready evolution of carbonic acid on the addition of hydrochloric acid,—although, previous to the incineration, no carbonic acid could be obtained; therefore, a carbonate had been formed by the ignition, showing that the precipitate had contained a compound of an organic and inorganic substance. The ash was found to consist of phosphate and carbonate of lime, with traces of sulphate of lime. On considering the organic substance precipitated by alcohol in gastric juice and insoluble in water, it appears to bear a great analogy to the *parapeptone* of Meissner, a substance discovered by this chemist among the products of digestion of albumen.* Parapeptone is obtained by neutralising with potash a solution containing hydrochloric acid and pepsine, after it has been used for the artificial digestion of albumen, this substance (parapeptone) being, under these circumstances, precipitated. It may be surmised that the compound of lime and parapeptone, when present in gastric juice, is precipitated by the addition of alcohol to the secretion, and then by filtration it will lose much of the acid which kept it in solution in the gastric juice, so that when washed with water, the aqueous fluid is no longer sufficiently acid to dissolve it, and, therefore, it

* *Zeitschrift für Rationelle Medizin*, series 3, vol. vii.

remains on the filter. I found this substance not only soluble in gastric juice, but also soluble in dilute acetic and hydrochloric acids, which bears out this view.

It will now be readily understood why I have in every case precipitated the gastric juice with alcohol, for by so doing I removed the whole, or, at all events, the greater portion of the parapeptone (if it be this substance), and also a quantity of phosphate of lime, which would have interfered, more or less, with the subsequent operations; although by so doing it was impossible to avoid the loss of a small quantity of the polarizing substance.

After freeing the solution from alcohol by distillation, and concentrating the fluid until it assumed a syrupy consistence, I added to it a very small quantity of a mixture of sulphuric acid with four or five times its volume of water. This was done with the view of transforming all the inorganic bases into sulphates, and setting free every organic and inorganic acid which might be present. By washing the gelatinous precipitate with alcohol, nearly the whole of the soda was removed; and on several occasions, especially during cold weather, I observed fine crystals of sulphate of soda in the alcoholic fluid a day or two later. If too much sulphuric acid was added to the syrupy fluid, the subsequent addition of alcohol failed to produce the gelatinous precipitate, when, of course, the whole operation was lost; in these cases, nothing more than a turbidity occurred. The formation of the gelatinous precipitate by the action of alcohol on the acid aqueous solution, and the washing of the precipitate with alcohol, was the cause of a considerable loss of the polarizing substance, which could not, however, be avoided.

The boiling with hydrated oxide of lead I believe to be indispensable to get rid of the sulphuric acid, the phosphoric acid, and other acids which had remained adherent to the gelatinous precipitate, and also perhaps of any pepsine which may have escaped precipitation in the former operations. By boiling the mixture for about three quarters of an hour, and then allowing it to remain undisturbed till the next day, it yields a filtrate which can be obtained quite clear with comparative ease, and perfectly free from lead; showing that the whole of the acids of the gastric juice have been removed, obviously from the formation of basic salts. It sometimes happens, however, that some sulphates escape the action of the oxide of lead, and find their way into the solution, which now only contains the polarizing substance, with a small quantity

of lime, soda, and sulphuric acid. I constantly found this solution more or less alkaline.

The treatment with oxide of lead is another source of loss of the polarizing substance; indeed, a large proportion of it remains in the lead precipitate. The following experiments show to what extent hydrated oxide of lead may precipitate the substance under consideration. Eight ounces of gastric juice were found to show 32.5° rotation in the saccharimeter; three times this bulk of alcohol was added, and the precipitate being the next day separated by filtration, was washed with water. This aqueous solution, measuring two ounces, was examined in the saccharimeter, and showed 13.5° . The alcoholic fluid was mixed with hydrated oxide of lead, and evaporated on the water bath to get rid of the alcohol; the next day this fluid was filtered, and the filter washed till the washings were no longer alkaline. The volume of the fluid amounted to $31\frac{1}{2}$ ounces. I added half an ounce of water, and then examined the solution in the saccharimeter; it showed 7° . Now, if none of the polarizing substance had been lost by the evaporation in presence of oxide of lead, and subsequent filtration, the number of degrees found by observation in the saccharimeter ought to have been 10.6° , while, in fact, it was only 7° , a loss of about one-third had, therefore, been experienced.

In a second experiment, a solution of the dry substance was prepared in $1\frac{1}{2}$ ounce of distilled water, which showed in the saccharimeter between 15° and 15.5° . This solution was boiled for twenty minutes with hydrated oxide of lead, and again the next day for forty minutes; two days afterwards the fluid was filtered, and the filter washed with distilled water until the filtrate contained only a very small quantity of solid matters. The solution, concentrated to $1\frac{1}{2}$ ounce, showed 11° in the saccharimeter; consequently about one-third of the substance had again been lost in the lead precipitate. As it might be objected, however, that the substance I am alluding to may possibly be decomposed by the mere boiling of its aqueous solution, and not taken up by the lead, I dissolved a sample of it in one ounce of distilled water, which solution showed 4.5° in the saccharimeter. The fluid was boiled for one hour, water being added to replace that which had evaporated; it was made up to one ounce, and examined, when it showed again 4.5° ; consequently the substance is not decomposed under these circumstances. The next day the solution had become decidedly hazy, and the observation with the

saccharimeter more difficult; still it was seen to deviate the polarized ray to between 4° and 5° ; consequently no appreciable amount of the substance dissolved had been decomposed by putrefaction, although the temperature of the air was very high during the whole of the experiment.

I found the polarizing substance to exhibit the chemical properties of Lehmann's peptone; they were as follows:—

Properties.—The substance when dry is brittle, non-crystalline, very transparent, and nearly colourless; it attracts moisture by exposure to the air, becoming a viscous mass. It is very soluble in water, and is precipitated from this solution by the addition of alcohol, and more completely so by a mixture of alcohol and ether. Its aqueous solution has the property of rotating the plane of polarization of light to the left. Four observations made, in order to determine the extent of this rotating power, with different weighed samples of the compound dissolved in 25 cubic centimetres of water, yielded, for 1° of Soleil's saccharimeter, 0.020 grm., 0.023 grm., 0.027 grm., 0.029 grm.; average 0.024 grm. Samples belonging respectively to the same specimens as those examined in the instrument were burnt in order to determine the weight of their ash; and this was subtracted from the weight of the corresponding substance employed for the determination of its optical properties. Therefore, 0.024 grm. of the substance under consideration, or a weight closely approximating to that dissolved in 25 c.c. of distilled water, gives 1° rotation to the left in Soleil's saccharimeter. I need hardly add that the action of a solution of the substance on the saccharimeter is perfectly proportioned to the strength of the solution; this, however, I determined by actual experiment.

The substance exposed to heat burns away almost entirely, leaving small and varying proportions of ash. With tannic acid, its solution yields an immediate and abundant precipitate; it is not precipitated by nitrate of silver, and bichloride of mercury induces in its solutions a slight turbidity. Neutral acetate of lead, in some cases, gave no precipitate; in others, precipitates more or less abundant were produced; these precipitates were evidently accidental, since, with some specimens, I failed to obtain a trace of turbidity on the addition of neutral acetate of lead; and in others the turbidity was but very slight. When this reagent had exerted no action, the addition of a few drops of ammonia to the solution of this salt, without impairing its transparency, caused a precipitate to appear

in the solution of the compound under consideration, just as in the case of peptone. Basic acetate of lead gave a precipitate which in most instances was soluble in an excess. I have recently obtained a sample of the polarizing substance which, when dissolved in water, yielded no precipitate with neutral acetate of lead, and with basic acetate of lead, a precipitate soluble in an excess, behaving, therefore, precisely like peptone. The aqueous solution is not precipitated by alkalis or mineral acids.

With respect to its chemical composition, the substance under consideration contains both nitrogen and sulphur. Dissolved in water, it readily undergoes spontaneous decomposition, especially in summer, when, after two or three days, the fluid becomes turbid, and shortly afterwards begins to emit a putrid smell.

In addition to this substance possessing the chemical properties of peptone, I may observe, that the method adopted for its preparation is precisely that which would yield peptone, and this substance must exist in gastric juice, unless every precaution be taken to obtain the secretion perfectly free from the products of digestion.

In order to control the above chemical results, and determine with every degree of certainty, whether the polarizing constituent of gastric juice is secreted in the stomach, or is actually a product of digestion, I have obtained gastric juice after washing out of the stomach all that it contained, and then again exciting the secretion by causing the dog to swallow fragments of bones from which the flesh had been carefully picked. On two other occasions, with the view of avoiding even the possibility of the substance given to the animal undergoing digestion, I used small silicious pebbles instead of bones. When the secretion was excited by fragments of bones, I obtained, in two experiments, samples of gastric juice which acted slightly on polarized light, both showing 7° . In my two experiments with the pebbles, I obtained no rotation at all; the secretion contained, therefore, none of the substance under consideration, proving thereby, that it is not secreted with the gastric juice, and consequently that its invariable presence in the animal fluid, when excited by means of wind-pipes or soft bones, must be the result of some process of digestion.

The next point of importance and interest was to ascertain which is the substance transformed in the stomach into polarizing peptone. I am engaged at present with this part of the investigation. So far, I have obtained proofs that cartilaginous structures, such as the trachea (oxen and sheep), and also bones

containing cartilage, yield, under the action of gastric juice, the substance in question. The following are the principal experiments from which this result is derived.

Experiment 1.—I excited the secretion of gastric juice by giving to one of the dogs some oxen's windpipe, boiled, but quite hard. The last quantities of gastric juice extracted were not mixed with the first; the first quantity, filtered, gave two ounces and four drachms, the second quantity six drachms, to which one drachm of the first quantity was added, in order to procure enough for the saccharimeter. The first quantity showed 32° rotation to the left, the second quantity showed 40° . It appeared, therefore, that the wind-pipe used for causing the secretion had been partly digested, and transformed into polarizing peptone; for if the optical properties of the fluid had been due to any other food which had remained in the dog's stomach since it had been last fed (although it was fasting at the time of the experiment), instead of having an increase of deviation from 32° to 40° , we should rather have expected a decrease of the rotating power.

Experiment 2.—The first and second quantities of gastric juice extracted in the preceding experiments were mixed together, the fluid showing 32° . (It ought to have been 34.5° . I cannot account for the discrepancy.) I took two quantities of the secretion, each measuring $1\frac{1}{2}$ ounce, and placed them in two bottles of the same size; to one quantity I added 20 grammes of boiled wind-pipe cut up into small pieces, and both bottles were exposed for one hour to a temperature of 40° Centigrade, during which time the fluid containing the wind-pipe was frequently stirred. After an hour the mixture was filtered, when it appeared to me that the cartilaginous fragments had been partly dissolved, the edges having become somewhat rounded and worn off. The next day the filtrate was examined in the saccharimeter, when its action was found to be so powerful that the instrument was no longer available. In order to obviate this difficulty, I measured off 11 cubic centimetres of the gastric juice, and diluted it with an equal volume of distilled water. The diluted fluid was introduced into the tube of the instrument, and examined, when it showed 33° rotation. From this experiment, it is obvious that $1\frac{1}{2}$ ounce of gastric juice, showing 32° rotation, after digesting 20 grammes of boiled oxen's trachea for one hour at a temperature of 40° , exhibited 66° rotation; its optical power had, therefore, increased to more than double; the polarizing peptone, which had been originally 3 per

cent., being, consequently, at the conclusion of the experiment, 63 per cent. of the gastric juice. The other sample of gastric juice, which had been used for the sake of comparison, showed 31° at the close of the experiment; the difference of 1° is so small, that it need not be taken into consideration.

Experiment 3.—One of the dogs, after fasting for forty-two hours, was allowed to eat fragments of hard bone cut up and perfectly free from flesh. A first supply of about two ounces of gastric juice was obtained under the influence of these hard bodies, and this was thrown on a filter. I then gave the dog some more pieces of bone, but these contained pieces of cartilage adhering to them, or were partly cartilaginous. The animal ate these greedily, and I obtained a second quantity of the gastric fluid, more abundant than the first; this was filtered separately, and yielded a fluid nearly as colourless as water. The next day I examined both samples of gastric juice in Soleil's polariscope. That obtained by the first process, i.e. after giving the animal bones free from flesh and cartilage, showed between 6° and 7° rotation; the samples extracted after the dog had eaten the bones and cartilage, showed between 17° and 18° rotation; it is, consequently, evident that the cartilaginous part of the bones had undergone digestion, and had increased, or greatly contributed to increase, the quantity of polarizing peptone from 0.6 per cent. to 1.6 per cent. of the gastric juice.

Finally, I may state that the residue of gastric juice obtained with wind-pipe or soft bones, contains a very large percentage of polarizing peptone. In one instance, where the indication in Soleil's polariscope was 31° , the proportion of the substance amounted to no less than 78 per cent. of the dry residue; and in another instance, where the gastric juice showed only 7° rotation, the proportion of this substance amounted to 32 per cent. of the dry residue. In these determinations a known weight of gastric juice was neutralized with a normal solution of potash, and then evaporated to dryness; the weight of the potash added was afterwards subtracted from that of the dry residue.

I have so far assumed that there is no other substance in gastric juice which rotates the plane of polarization of light; indeed, this appears to me satisfactorily shown by the investigations related in the present communication.

The following results are derived from my researches on the constituents of gastric juice.

1. That gastric juice, when its secretion is excited by means of soft (cartilaginous) bones, or the substance of wind-pipes, is invariably possessed of the property of rotating more or less the plane of polarization of light to the left.

2. That this optical property of gastric juice is due to its containing a substance exhibiting the chemical properties of Lehmann's *peptone*, a product of digestion; and 0.024 grammes of the substance, or a weight closely approximating to this, dissolved in 25 c.c. of water, rotates the plane of polarization 1° in Soleil's saccharimeter.

3. That gastric juice, when obtained after washing out the animal's stomach with water, and exciting its secretion by means of non-digestible bodies, such as silicious pebbles, exerts no action whatever on polarized light, thereby confirming the second conclusion.

4. That there is every reason to believe the above substance to be the only constituent of gastric juice possessed of the property of acting on polarized light.

5. That the substance to which gastric juice owes its optical properties is a product of digestion of the wind-pipes, or of the soft or cartilaginous bones used for exciting its secretion.

STATISTICAL TABLES

OF THE

PATIENTS TREATED IN GUY'S HOSPITAL

DURING

1861.

*for Parkin
with Shute's report*

TO THE
PRESIDENT, TREASURER, AND GOVERNORS
OF GUY'S HOSPITAL.

GENTLEMEN,—I beg respectfully to submit to your notice the following numerical analysis of the patients treated in the Hospital during the year 1861.

The importance and practicability of annually publishing complete statistical records of the patients treated in the London Hospitals, was discussed at a meeting held with this object at Guy's Hospital in the summer of last year under the presidency of the Treasurer, and was attended by gentlemen delegated to represent the chief metropolitan hospitals. Prior to this combination, the subject had engaged the attention of the International Statistical Congress, which met in London in 1860; and a scheme for adopting one uniform system of registration was brought before the sanitary section of the Congress, by Miss Nightingale, through whose exertions the movement may be said to have originated. The accompanying Tables embody in the main, as far as this Hospital is concerned, the objects contemplated by the meeting of last year, and will be seen to present but little difference in their character from the annual summaries formerly made, and which were incorporated into a separate Report last year, giving the results of the previous septennium. It has been thought advisable, however, in addition to the meagre classification of diseases arranged in former Reports, to individualize each separate malady; and with this object a Table of Diseases has been appended, in which it has been found more conducive to accuracy to adopt the nomenclature in familiar use in the Hospital, in preference to that employed in the Mortuary Tables of the Registrar-general.

A reference to the retrospective summary at the close of the series of Tables, will show that the total numbers who have availed themselves of the Hospital benefits, have been considerably greater during the past than during any year of the preceding septennium, or in fact of any previous year in the history of the Institution; for at no period prior to 1850 did

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the total numbers treated in Hospital exceed 4000. The augmentation will be noticed to extend as well to the Out-patient Department, which last year numbered 50,311 cases, or 9260 more than in 1860. The total number under treatment inside the Hospital was 5360, and the daily average number resident as calculated for each month of the year, was 509, being an increase of 20 over a similar average made for 1860, which again exceeded by 12 the average of any previous year. As it is, the high average of the past twelve months may be considered the maximum to which the numbers can be carried with the present available accommodation, and such a limit may indeed be considered exceptional, as it could only have been arrived at through the urgency of the applications, which necessitated the entire occupation of the wards during the year. The repetition of a similar practice in future is manifestly very undesirable, as it is found materially to interfere with the means employed to promote the sanitary condition of the Hospital, and which consist chiefly in the periodical cleaning, painting, and vacation of the wards. It may have been partly owing to the cause alluded to, that I had occasion to report during the last months of the year, the recurrence of cases of local erysipelas in several of the wards, tending to retard convalescence, although in no instance were they attended with fatal consequences.

In addition to the ordinary Tables, I have added one, Table II., indicating the localities from which patients were brought to the Hospital during the year. In the absence of any better system of localization, the subdivision of the Metropolis into postal districts has furnished a very available means for calculation; and when it is kept in view that the admissions of patients are not influenced by Governors' orders or private recommendations, but are solely judged of on the grounds of symptomatic urgency, the Table must be considered to afford a good index of the comparative wants of the sick poor of the surrounding districts. It will facilitate reference to point out the fact, that, of all the districts, the South-eastern, or that in which two of the largest Metropolitan Hospitals are situated, furnishes not less than 488 of 1000 admissions from within the metropolitan boundary, even excluding in the calculation the numbers sent from Greenwich and Woolwich. The next in magnitude is the Southern or Lambeth district, which sends its quota of 165 per thousand admissions, making a total of 653 in every 1000 admissions from the Boroughs of Southwark and Lambeth. Of the districts north of the river Thames, by far the largest number arrive from the Eastern and North-eastern districts, which are notoriously ill supplied with hospital accommodation. These are calculated to amount to 187 per thousand, while the City and Finsbury, along with Islington and Pentonville, contribute 97 per thousand. The remaining four districts to the West, North-west, South-west, and West

Central, furnish, as might be expected, a very small portion of the total numbers—there being only 50 registered from these districts in every 1000 admissions from all quarters of the Metropolis. Of the 1097 persons received from the country, 325, or nearly 30 per cent., have arrived from Greenwich and Woolwich; and while the adjacent counties south of the river have contributed 39.47 per cent., the country districts of Middlesex and Essex have added 15.77 to the number specified. The number of patients received from distant parts of the country has amounted during the year to 166.

I have the honour to be,

GENTLEMEN,

Yours faithfully and respectfully,

JOHN CHARLES STEELE.

STATISTICS OF GUY'S HOSPITAL

FOR 1861.

I.—GENERAL STATEMENT OF THE NUMBER OF PATIENTS UNDER TREATMENT IN THE HOSPITAL DURING 1861.

Remaining 1st January, 1861,	493
Admitted during the year,	4867
Total,	5360
Discharged well or convalescent,	2553
Relieved,	1431
Unrelieved,	209
Discharged for special reasons,	126
Died,	458
Remaining 1st January, 1862,	483
	5360

MEDICAL WARDS.		SURGICAL WARDS.	
Remaining 1st January, 1861,	221	Remaining 1st January, 1861,	272
Admitted during the year,	2129	Admitted during the year,	2747
Total,	2341	Total,	3019
Discharged,	1834	Discharged,	2585
Died,	303	Died,	155
Remaining 1st January, 1862,	204	Remaining 1st January, 1862,	279
	2341		3019

Average number resident, daily, throughout the year, .. 509 { Males, 297
 Mean residence of each patient, 34.6 days { Females, 212
 Rate of mortality over all the cases, 9.4 per cent { Males, 10.4
 { Females, 8.3

MEDICAL CASES.		SURGICAL CASES.	
Average number in Hospital, 225 { M., 197 F., 98		Average number in Hospital, 284 { M., 170. F., 114.	
Mean residence, 35 days.		Mean residence, 34.3 days.	
Rate of mortality, 14.1 per cent { M., 16.3 F., 11.6		Rate of mortality, 9.6 per cent { M., 6.4 F., 44.	

II.—TABLE INDICATING THE LOCALITIES IN WHICH PATIENTS WERE
RESIDENT PRIOR TO ADMISSION.

PORTAL DISTRICTS AND EXTRA URBAN LOCALITIES.	Total.	England.	Wales.
North—Islington and Pentonville	119	57	62
South—The Borough of Lambeth	665	283	282
East—Whitechapel, St. George's East, Poplar, Limehouse, and Stepney	436	227	209
West	58	24	24
East Central—The City and Finsbury	247	119	128
West Central	52	23	29
North-west	27	17	10
North-east—Hackney and Bethnal Green	272	127	145
South-west	51	34	17
South-east—The Borough of Southwark, Rothenkirk, and Deptford	1843	1047	796
Greenwich and Woolwich	325	194	131
Adjacent Counties—North of the river Thames, Middlesex, and Essex	173	103	70
Adjacent Counties—South of the river Thames, Kent, Sussex, and Surrey	433	260	173
Distant Counties	166	122	44
Total	4867	2747	2120

III.—CLASSES OF DISEASE, AND RESULT OF TREATMENT.

	Total.	Cured.		Relieved.		Uncured.		Special.		Died.	
		M.	F.	M.	F.	M.	F.	M.	F.		
1. Nervous system	385	81	67	85	74	23	21	2	4	30	8
2. Respiratory organs	555	85	60	131	90	25	12	4	5	93	50
3. Organs of circulation	187	23	30	46	32	13	6	..	..	23	14
4. Digestive organs	300	92	48	49	40	8	9	3	1	39	21
5. Genito-urinary organs	522	146	69	88	109	14	22	8	11	22	13
6. Venereal diseases	229	209	222	22	55	..	..	12	9	..	..
7. Dropsies	126	12	8	37	24	8	4	3	1	15	14
8. Diseases and injuries of bones	503	264	87	63	19	14	9	2	2	38	5
9. Diseases and injuries of joints	474	149	105	96	73	15	12	7	3	9	5
10. External injuries of soft parts	296	177	47	18	4	3	3	..	1	31	12
11. Abscesses, ulcers, tumours	447	125	137	57	59	20	27	4	..	10	8
12. Diseases of the eye	266	72	70	44	37	17	10	11	5	..	1
13. Diseases of the skin	341	43	45	19	17	4	7	..	2	3	1
14. Fevers	99	39	29	11	3	1	..	..	2	2	10
15. Miscellaneous affections	67	8	4	15	14	..	2	15	7	2	..
Total	4877	1525	1028	781	650	165	144	73	53	297	161
	4877	2553	1431	369	126	458					

IV.—TABLE OF AGES OF PATIENTS.

DISEASES.	I.—DISCHARGED.														
	Total.	Under 5.	5 to 10.	10 to 15.	15 to 20.	20 to 25.	25 to 30.	30 to 35.	35 to 40.	40 to 45.	45 to 50.	50 to 55.	55 to 60.	60 to 65.	65 upwards.
1. Nervous system	357	12	24	20	39	39	36	69	66	32	14	..	..	..	..
2. Respiration	412	7	8	18	58	65	57	79	57	43	18	2	..	..	..
3. Circulation	150	6	3	19	30	26	12	22	24	11	6	..	..	..	..
4. Digestion	250	8	10	7	22	24	32	47	41	32	14	3	..	..	..
5. Genito-urinary	467	14	8	9	36	69	64	102	88	51	18	8	..	..	..
6. Venereal diseases	329	2	..	5	236	144	56	58	29	6	1	1	..	..	..
7. Dropsies	97	4	4	5	11	12	7	24	15	11	4	..	..	..	..
8. Bones	460	24	51	33	47	34	42	84	72	41	24	7	1	..	..
9. Joints	460	27	39	39	64	73	53	78	43	28	14	2	..	..	..
10. External injuries of soft parts	253	25	26	36	25	34	25	45	25	10	2	2	..	..	..
11. Abscesses, ulcers, &c.	429	13	17	26	57	60	52	79	69	38	14	4	..	..	..
12. Eye	296	11	13	23	43	53	28	32	21	71	19	9	3	..	..
13. Skin	137	14	12	11	17	12	11	19	22	9	7	3	..	..	..
14. Fevers	87	3	10	12	25	12	7	8	7	2	1	..	..	..	..
15. Miscellaneous	65	4	5	9	7	12	9	9	5	2	3	..	..	..	..
Total	4419	174	230	279	717	659	491	753	575	337	159	41	4	..	..

DISEASES.	II.—DIED.														
	Total.	Under 5.	5 to 10.	10 to 15.	15 to 20.	20 to 25.	25 to 30.	30 to 35.	35 to 40.	40 to 45.	45 to 50.	50 to 55.	55 to 60.	60 to 65.	65 upwards.
1. Nervous system	28	2	1	1	3	1	4	6	6	3	1	..	..	..	..
2. Respiration	143	4	2	2	16	16	18	39	24	15	5	1	1	..	..
3. Circulation	37	..	1	1	4	4	4	8	2	6	6	1	..	..	..
4. Digestion	50	..	1	5	..	1	6	9	9	11	8	..	..	..	..
5. Genito-urinary	35	..	..	..	3	5	3	7	7	4	6	..	..	..	..
6. Venereal diseases	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
7. Dropsies	29	..	4	1	2	2	1	9	7	2	1	..	..	..	..
8. Bones	43	2	4	1	4	2	1	9	5	11	3	1	..	..	..
9. Joints	14	1	1	2	..	2	2	2	1	2	1	..	..	..	..
10. External injuries of soft parts	43	10	10	..	2	3	2	6	3	..	1	..	..	..	..
11. Abscesses, ulcers, &c.	18	..	1	4	..	1	..	4	4	1	3	..	..	..	..
12. Eye	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
13. Skin	4	1	..	..	..	..	..	..	..	1	2	..	..	..	..
14. Fevers	12	..	1	..	3	1	3	..	..	..	1	..	..	..	..
15. Miscellaneous	2	..	..	..	..	..	..	..	..	..	1	1	..	..	..
Total	458	26	26	17	35	39	43	98	71	59	28	5	1	..	..

OPERATION LIST, 1861.

Kind of Operation.	Case	Disease or Injury, &c.	Fract. Complication.	Count of Unfavorable.	Result.
				M. F. M. F.	M. F.
Amputation, Primary—					
Shoulder joint—	1	Compound fracture and laceration	Tetanus	1	1
Arm	24	"	Pneumia	1	1
Fingers, one or more	1	"	"	1	1
Leg	1	"	"	1	1
Amputation, Secondary—					
Arm	24	"	"	1	1
Fingers, one or more	1	"	"	1	1
Leg	1	"	"	1	1
Amputation for Disease—					
Thigh	1	Compound fracture	Tetanus, shock	1	1
Forearm	1	Myeloid tumour	Phlegmasia	1	1
Fingers, one or more	11	Ulceration, ankylosis, necrosis	Tubercle	1	1
Leg	1	9 months' cancer, 1 malignant tumour, 2 necrosis	Pneumonia	1	1
Foot	1	"	Exhaustion	1	1
Exclusion of Tumours—					
Mammary, cancerous	12	"	Gangrene	1	1
Rectum, cancerous	1	"	"	1	1
Other	24	4 cancerous, 1 phlegmasia (3 cancerous, 2 fibrosis, 2 necrosis, 1 cholelith, 1 vascular, 2 warty, 10 fatty growths)	"	1	1
Excision of Diseased Bones—					
Upper extremity	1	1 clavicle, 3 humeri, 1 ulna, 1 thumb	Brucella	1	1
Head of femur	12	2 femur, 10 tibia	"	1	1
Leg and thigh	1	1 great toe, 1 metatarsus	"	1	1
Wrist and axis	1	1 tibia, 1 metatarsus, 1 metacarpus	"	1	1
Foot	1	1 thigh, 1 leg	"	1	1
Excision of Diseased Joints—					
Excision of Cervical stump	1	1 phlegmasia	"	1	1
Wrist	1	1 phlegmasia	"	1	1
Thigh	1	1 phlegmasia	"	1	1
Testicle	1	1 phlegmasia	"	1	1

OPERATION LIST—CONTINUED.

Nature of Operation.		Cases.	Result or History, &c.				: Total Complications.				: Special Results.				: Final.				
			M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	
Bony Operations—																			
Deficiency from hernia			8	6	2	3	5	3	3	3	0	1	1	0	1	0	1	0	1
" " " " from disease			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
Congenital deformities			4	4	0	4	0	4	4	4	4	4	4	4	4	4	4	4	4
" " " " " " " "			2	2	0	2	0	2	2	2	2	2	2	2	2	2	2	2	2
Vesico-vaginal fistula			2	2	0	2	0	2	2	2	2	2	2	2	2	2	2	2	2
Exploratory operations			6	6	0	6	0	6	6	6	6	6	6	6	6	6	6	6	6
" " " " " " " "			2	2	0	2	0	2	2	2	2	2	2	2	2	2	2	2	2
Ligature of artery			10	10	0	10	0	10	10	10	10	10	10	10	10	10	10	10	10
Sections of anal fistula			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
" " " " " " " "			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
Operations on ureters			10	10	0	10	0	10	10	10	10	10	10	10	10	10	10	10	10
Urethrotomy			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
" " " " " " " "			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
Fracture of bladder per rectum			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
" " " " " " " "			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
Lithotomy			12	12	0	12	0	12	12	12	12	12	12	12	12	12	12	12	12
" " " " " " " "			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
Laryngotomy			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
" " " " " " " "			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
Tracheotomy			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
" " " " " " " "			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
Tracheotomy & ligature			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
Operations for Hernia			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
" " " " " " " "			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
Unilateral			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
" " " " " " " "			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
Bilateral			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
" " " " " " " "			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
Herniotomy			15	15	0	15	0	15	15	15	15	15	15	15	15	15	15	15	15
" " " " " " " "			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
Femoral Hernia			13	13	0	13	0	13	13	13	13	13	13	13	13	13	13	13	13
" " " " " " " "			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
Herniotomy			13	13	0	13	0	13	13	13	13	13	13	13	13	13	13	13	13
" " " " " " " "			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
Operations on the Eye—																			
Extraction of lens			20	20	0	20	0	20	20	20	20	20	20	20	20	20	20	20	20
" " " " " " " "			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
Artificial pupil			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
Strabismus			6	6	0	6	0	6	6	6	6	6	6	6	6	6	6	6	6
" " " " " " " "			1	1	0	1	0	1	1	1	1	1	1	1	1	1	1	1	1
Excision of globe			4	4	0	4	0	4	4	4	4	4	4	4	4	4	4	4	4
Total			342	342	0	342	0	342	342	342	342	342	342	342	342	342	342	342	342

VI.—MONTHLY TABLE OF ADMISSIONS, DISMISSIONS, AND DEATHS,
DISTINGUISHING THE SEXES.

	MEDICAL PATIENTS.						SURGICAL PATIENTS.					
	Admitted.			Discharged.			Admitted.			Discharged.		
	M.	F.	N.	M.	F.	N.	M.	F.	N.	M.	F.	N.
January . . .	110	100	84	77	16	17	130	97	121	79	12	5
February . . .	103	78	89	71	11	10	135	92	121	75	7	4
March . . .	78	71	75	73	9	6	126	71	134	83	10	5
April . . .	88	78	98	72	14	10	132	75	112	79	9	6
May . . .	118	83	74	64	15	7	166	101	149	84	9	4
June . . .	89	71	86	69	22	5	122	94	122	84	3	1
July . . .	104	86	65	73	18	9	153	101	151	95	4	6
August . . .	83	70	87	68	16	10	153	95	158	90	9	4
September . . .	77	72	61	53	17	11	114	67	97	77	4	3
October . . .	120	105	84	109	19	6	160	103	138	100	11	3
November . . .	103	82	83	68	19	9	144	90	127	74	17	2
December . . .	76	75	82	78	13	14	143	83	146	89	13	4
Total . . .	1149	971	968	866	189	114	1678	1069	1576	1009	108	47
	2120	1834	303	2747	2565	155						

VII.—THE FOLLOWING TABLE GIVES THE CAUSES OF THE ACCIDENTS, WITH
THE MORTALITY.

CAUSE OF ACCIDENT.	Total Cases.	Discharged.		Died.		Remains.	
		M.	F.	M.	F.	M.	F.
1. Accidents on the river . . .	41	28	..	5	..	8	..
2. Assaults . . .	28	17	7	..	..	2	2
3. Attempts at suicide (excluding poison) . . .	7	1	4	2	..	..	..
4. Burns from clothes taking fire . . .	35	10	6	8	..	6	1
5. Burns from heated fluids . . .	27	14	7	3	1	1	1
6. Burns from explosion of gas . . .	3	3	..	..	..	..	..
7. Burns from gunpowder . . .	1	1	..	..	..	..	..
8. Collisions between opposing forces . . .	7	7	..	..	..	..	..
9. Collisions with street vehicles . . .	102	64	15	12	8	6	2
10. Cuts and lacerations from sharp instruments . . .	37	25	8	2	..	1	..
11. Falls down stairs . . .	16	6	7	..	..	3	1
12. Falls from a height . . .	138	99	10	14	1	13	1
13. Falls on the ground . . .	121	79	24	3	1	10	..
14. Falls of weights . . .	72	59	1	2	..	..	..
15. Gunshot wounds . . .	5	2	1	2	..	..	..
16. Machinery accidents . . .	37	29	1	3	..	3	1
17. Poisoning, accidental . . .	1	..	..	1	..	..	..
18. Poisoning, intentional . . .	8	3	2	3	1	..	..
19. Railway accidents . . .	18	12	..	4	1	1	..
20. Tensions of body . . .	9	7	1	..	..	..	..
Total . . .	713	467	94	64	14	59	15

VIII.—TABLE I.—DISEASES OF NERVOUS SYSTEM.

	Total.		Went.		Relieved.		Uncured.		Other Causes.		Died.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Cephalalgia . . .	12	4	3	2	..	1	..	..	..	..	..	..
Cerebral disease . . .	36	2	12	3	6	2	..	..	..	..	9	2
Concussion of brain . . .	16	12	3	..	1	..	..	..	..	..	..	..
Concussion of spine . . .	3	2	..	1	..	..	..	..	..	..	..	..
Chorea . . .	30	6	14	3	6	1	..	1	..	..	..	..
Dalium tremens . . .	7	4	..	1	..	..	..	1	..	..	1	..
Epilepsy . . .	27	4	3	8	7	2	2	..	..	..	1	..
Hemicrania . . .	4	2	..	1	1	..	..	..	..	..	..	..
Hemiplegia . . .	23	2	..	8	6	4	3	..	..	..	..	..
Hydrocephalus . . .	2	..	..	..	..	..	..	..	..	..	1	1
Hypochondriasis . . .	6	1	..	4	1	..	..	..	..	..	..	..
Hysteria . . .	53	2	26	1	17	..	5	..	2	..	..	..
Mania . . .	3	..	..	1	1	1	..	..	..	..	2	2
Meningitis . . .	11	3	1	1	2	..	..	..	..	..	..	..
Neuralgia . . .	14	2	1	1	6	2	2	..	..	..	..	..
Paralysis, partial . . .	19	4	2	7	3	1	1	..	1	..	3	2
Paraplegia . . .	49	7	5	14	8	7	3	..	..	..	..	..
Pneumonia . . .	9	2	2	3	1	..	..	..	..	..	1	..
Sciatica . . .	10	6	2	2	..	..	..	..	..	..	..	..
Spinal irritation . . .	4	..	..	4	..	..	..	..	..	..	..	..
Spinal disease . . .	10	2	..	7	..	..	..	..	..	..	1	1
Tetanus . . .	3	1	..	..	..	..	..	..	..	..	..	..
Vertigo . . .	3	1	1	..	1	..	..	..	..	..	..	..
Poisoning, opium and lead . . .	12	5	4	1	..	..	..	..	..	..	1	..
Other nervous affections . . .	19	7	4	6	2	..	..	..	..	..	..	..
Total . . .	385	81	67	85	74	23	21	2	4	20	8	..

2.—DISEASES OF RESPIRATORY ORGANS.

	Total.		Cured.		Relieved.		Uncured.		Other Causes.		Died.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Asthma and emphysema . . .	21	2	3	9	4	1	..	..	..	..	2	..
Bronchitis and catarrh . . .	123	37	23	22	14	2	2	..	..	..	13	10
Diphtheria . . .	4	..	2	..	..	..	..	..	..	..	1	1
Laryngitis and croup . . .	19	2	3	4	2	1	..	..	..	..	1	1
Other throat affections . . .	10	3	..	2	2	..	..	..	..	..	2	1
Empyema and hydrothorax . . .	5	..	..	3	..	1	..	..	..	..	1	..
Hemoptysis . . .	12	4	1	5	2	..	..	..	..	..	..	..
Pleurisy . . .	45	19	17	4	2	..	..	..	..	..	2	1
Pneumonia and broncho-pneumonia . . .	25	16	6	5	3	..	..	..	..	..	4	1
Pneumo-thorax . . .	3	..	..	..	2	1	..	..	..	..	..	..
Poisoning, foul air . . .	1	1	..	..	..	..	..	..	..	..	1	..
Other chest affections . . .	19	1	6	5	2	2	..	..	..	..	1	1
Total . . .	555	85	60	131	90	25	12	4	5	93	50	..

3.—DISEASES OF ORGANS OF CIRCULATION.

	Total.		Cured.		Relieved.		Uncured.		Other Causes.		Dead.
	M. & F.	M. F.	M. F.	M. F.	M. F.	M. F.	M. F.	M. F.			
Anaemia and chlorosis . . .	32	2	11	2	15	1	1	..	..	..	..
Aneurism, aortic . . .	13	..	..	6	..	4	..	..	..	..	..
Aneurism of other vessels . .	9	3	..	2	1	1	..	..	..	2	..
Aortic disease . . .	13	..	..	7	..	1	2	..	..	2	1
Epistaxis . . .	2	1	..	1	..	..	..	..	..	..	..
Heart disease . . .	62	1	3	17	11	5	1	..	..	19	11
Heart disease, pericarditis . .	9	2	..	3	1	..	1	..	..	1	1
Leucocythemia . . .	1	..	..	..	..	1	..	..	..	..	..
Pilebitis . . .	5	1	3	..	..	..	..	..	..	..	..
Purpura . . .	11	4	4	2	..	..	..	..	..	1	..
Pyæmia . . .	3	..	2	..	..	..	..	..	..	1	..
Hæmorrhoids . . .	13	7	3	3	..	..	..	..	..	..	..
Nævus . . .	4	1	3	..	..	..	..	..	..	..	..
Varicose veins . . .	8	..	3	4	..	1	..	..	..	..	..
Other affections . . .	2	1	1	..	..	..	..	..	..	..	..
Total . . .	187	23	30	46	32	13	6	..	..	23	14

4.—DISEASES OF ORGANS OF DIGESTION, WITH MOUTH AND ANUS.

	Total.		Cured.		Relieved.		Uncured.		Other Causes.		Dead.	
	M. & F.	M. F.	M. F.	M. F.	M. F.	M. F.	M. F.	M. F.	M. F.	M. F.	M. F.	
Abdominal tumours . . .	8	..	3	2	1	1	..	..	1	..	..	
Anal fistula and abscesses . .	22	10	5	2	2	1	1	1	..	..		
Colic . . .	8	5	2	..	1	..	..	..	..	..		
Constipation . . .	14	6	2	4	2	..	..	..	..	..		
Diarrhoea . . .	9	4	2	1	2	..	..	..	..	..		
Dysentery . . .	12	4	..	3	1	1	..	..	..	3		
Dyspepsia . . .	39	16	8	12	2	..	1	..	..	..		
Enteritis . . .	4	2	1	1	..	..	..	..	..	..		
Gastric disease . . .	24	6	2	4	7	..	1	..	..	2		
Gastrodynia . . .	19	5	2	..	3	..	..	..	..	..		
Gastro-enteritis . . .	13	3	2	1	3	..	..	..	3	1		
Guinea worm . . .	1	..	1	..	..	1	1	..	..	6		
Hepatic disease . . .	21	3	1	3	2	1	1	..	..	3		
Hernia . . .	47	16	11	3	2	2	..	..	..	10		
Intestinal obstruction . . .	8	..	3	2	..	..	..	..	..	3		
Jaundice . . .	11	3	4	1	3	..	..	..	..	..		
Mesenteric disease . . .	5	..	1	1	..	1	..	..	..	1		
Pancreatic disease . . .	1	..	..	..	..	..	..	..	..	1		
Peritonitis . . .	18	5	4	..	..	..	..	..	..	4		
Pharynx and œsophagus, disease of .	5	..	1	2	..	1	..	..	..	1		
Rectum, disease of . . .	7	..	2	2	..	1	1	..	..	1		
Tongue and mouth, affections of .	6	1	1	..	1	1	1	..	..	..		
Other diseases . . .	7	4	..	2	..	..	..	..	..	1		
Total . . .	300	92	48	49	40	8	9	3	1	29	21	

5.—GENITAL AND URINARY ORGANS.

	Total.		Cured.		Relieved.		Uncured.		Other Causes.		Dead.
	M. & F.	M. F.	M. F.	M. F.	M. F.	M. F.	M. F.	M. F.	M. F.		
Bladder, diseases of . . .	23	3	1	4	7	2	2	..	..	4	..
Diabetes . . .	14	2	..	6	..	2	1	2	..	1	..
Hæmaturia . . .	7	3	1	2	1	1	..	..	..	..	..
Hydrocele . . .	43	31	..	11	..	1	..	..	..	..	..
Incontinence of urine . . .	6	2	..	4	..	..	..	..	..	..	..
Morbus Addisonii . . .	5	..	..	2	1	..	..	..	..	1	..
Nephritis . . .	6	2	..	1	..	..	..	..	..	2	..
Orchitis and diseased testicle .	39	19	..	11	..	..	..	..	..	..	..
Prostatic disease . . .	11	3	..	6	..	2	..	..	..	1	..
Perforated fistula . . .	19	3	..	2	..	2	..	..	..	3	..
Phymosis and paraphimosis . .	5	5	..	..	..	..	2	..	..	..	..
Renal calculus . . .	7	..	3	..	2	1	2	..	2	3	..
Renal disease . . .	29	2	2	3	2	1	2	..	2	3	..
Stricture and extravasation . .	88	52	..	27	..	2	..	..	..	7	..
Uterine calculus . . .	16	14	..	1	..	1	..	..	..	..	..
Supposed calculus . . .	8	1	..	3	..	..	..	4	..	..	..
UTERINE AFFECTIONS.											
Aménorrhœa . . .	20	..	6	..	13	..	..	..	1	..	..
Leucorrhœa . . .	8	..	1	..	7	..	..	..	..	..	..
Menorrhagia . . .	14	..	8	..	4	..	2	..	..	..	..
Obstetric complications . . .	14	..	5	..	4	..	..	..	5	..	..
Ovarian disease . . .	30	..	7	..	16	..	2	..	1	..	4
Pelvic cellulitis . . .	14	..	3	..	7	..	..	..	..	..	..
Ruptured peritoneum . . .	3	..	2	..	1	..	..	..	..	..	..
Uterine disease . . .	31	..	8	..	12	..	6	..	2	..	3
" Polypus . . .	7	..	7	..	..	..	..	..	..	..	..
" Prolapsus . . .	12	..	1	..	10	..	1	..	..	..	..
" Retroversion . . .	9	..	3	..	5	..	1	..	..	..	..
" Tumour . . .	14	..	..	..	14	..	..	..	..	..	..
Vesico-vaginal fistula . . .	4	..	2	..	1	..	1	..	..	..	..
Other affections . . .	23	4	8	2	4	..	4	..	..	1	..
Total . . .	502	146	69	88	109	14	22	8	11	22	..

6.—VENEREAL DISEASES.

7.—DROPSIES.

		Total.	Cured.	Relieved.	Uncured.	Other Causes.		Dead.
		M. & F.	M. F.	M. F.	M. F.	M. F.	M. F.	
Veneral diseases	Total	329	209	222	32	55	..	12 9 ..
DROPSIES.								
Anasarca		8	1	..	1	3	2	.. 1 ..
Albuminuria and renal dropsy		73	5	5	29	5	5	2 2 .. 11 9
Ascites and hepatic dropsy		11	3	..	2	2	..	1 .. 1 2
Cardiac and general dropsy		13	..	2	3	2	1	1 .. 2
Ovarian dropsy		10	..	1	9	..	..	
Scirrhal dropsy		11	3	..	2	3	..	 1 2
Total		126	12	8	37	24	8 4	3 1 15 14

8.—DISEASES AND INJURIES OF BONES.

	Total.		Cured.		Relieved.		Uncured.		Other Causes.		Dead.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Necrosis and caries of												
Bones of Upper extremity	22	7	7	5	1	2	..	..	..	..	..	..
" Lower extremity	65	24	9	18	3	6	3	..	..	2	..	..
" Head and face	17	3	1	6	4	1	..	..	1	..	..	..
" Spine and ribs	20	4	2	6	3	3	1	..	..	1	..	..
Exostosis	10	3	3	2	..	1	..	..	..	..	1	..
Periostitis	13	6	3	3	..	..	..	..	1	..	..	..
Spinal curvature	8	..	3	1	2	2	..	..	..	..	..	..
Fracture simple of Arm and forearm	33	18	9	5	1	..	..	..	1	..	..	..
" Thigh	80	32	20	3	2	..	..	..	1	1	..	..
" Leg	107	76	18	5	1	..	..	..	1	4	2	..
" Patella	12	8	3	..	1	..	..	..	..	..	..	..
" Bones of foot	9	6	..	3	..	..	..	..	..	..	..	..
" Bones of hand	3	2	..	1	..	..	..	..	..	..	..	..
Comp. and comminut. fractures of												
Arm and forearm	6	4	..	..	..	..	..	..	..	2	..	..
Head	11	7	1	2	..	..	..	..	..	1	..	..
Thigh	2	..	..	..	..	..	..	..	..	..	..	..
Leg	18	8	4	..	..	..	..	..	..	5	1	..
Foot	7	4	2	..	1	..	..	..	..	..	..	..
Fractured Skull	23	7	1	..	..	..	..	..	..	14	1	..
" Spine	2	..	..	..	..	..	..	..	..	2	..	..
" Jaw	5	5	..	..	..	..	..	..	..	..	..	..
" Ribs	23	16	4	1	1	..	..	..	..	1	..	..
" Pelvis	7	4	..	..	..	..	..	..	..	3	..	..
Total	503	264	87	63	19	14	9	2	2	38	5	..

9.—DISEASES AND INJURIES OF JOINTS.

	Total.		Cured.		Relieved.		Uncured.		Other Causes.		Dead.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Gout	7	3	1	2	1	..	..	..	..	..	..	..
Rheumatism	169	72	35	34	20	4	2	..	..	..	..	..
Rheumatic gout	12	3	1	4	2	..	2	..	..	..	..	..
Diseased Ankle	32	6	8	8	5	2	2	1	..	..	..	..
" Hip	52	2	4	15	18	4	1	3	1	1	3	..
" Knee	82	12	30	18	14	..	2	..	1	4	1	..
" Elbow	12	2	3	2	3	1	..	..	..	1	..	..
" Other joints	8	4	..	2	..	1	..	..	1	..	..	..
Injury to Elbow	6	4	..	1	..	..	..	..	..	1	..	..
" Knee	23	13	3	1	2	1	1	..	1	1	..	..
" Hip	7	2	2	1	1	..	..	..	..	..	1	..
" Ankle	20	14	2	3	1	..	..	..	..	..	..	..
" Other joints	7	2	2	2	..	..	..	..	..	1	..	..
Bursitis	22	1	12	1	6	..	2	..	..	..	..	..
Dislocations	10	7	1	2	..	..	..	..	..	..	..	..
Talipes	5	2	1	..	..	2	..	..	..	..	..	..
Total	674	349	105	96	73	15	12	7	3	9	5	..

10.—EXTERNAL INJURIES OF SOFT PARTS.

	Total.		Cured.		Relieved.		Uncured.		Other Causes.		Dead.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Bruises	77	58	7	7	..	..	..	..	..	..	4	1
Burns and scalds	59	28	13	..	..	..	..	..	..	..	11	7
Wounds of the head and face	40	29	4	3	1	..	..	..	..	..	3	..
" Incised and poisoned	46	18	12	4	1	3	2	..	..	..	4	2
" Lacerated and contused	62	41	6	4	2	..	1	..	..	..	6	2
" Gunshot	5	2	1	..	..	..	..	..	..	..	2	..
Cut throat	7	1	4	..	..	..	..	..	..	1	1	..
Total	296	172	47	18	4	3	3	..	1	31	12	..

11.—ABSCESSSES; ULCERS; TUMOURS.

	Total.		Cured.		Relieved.		Uncured.		Other Causes.		Dead.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Abscess of Head and neck	17	5	3	3	..	..	2	..	..	..	..	1
" Upper extremity	26	11	8	3	2	..	2	..	..	..	..	..
" Lower extremity	41	20	4	5	4	2	1	..	..	5	..	..
" Lumbar and psoas	15	..	1	6	4	2	1	1	..	..	..	..
" Mammary	11	..	8	..	3	..	..	..	..	..	..	..
" Other parts	42	14	10	6	6	1	..	..	..	3	2	..
Tumours Cancerous, of Breast	24	..	11	..	..	10	..	..	..	..	..	..
" " " " " " " "	20	9	3	8	..	4	1	3	..	..	2	..
" of miscellaneous characters	65	15	27	5	16	3	4	..	..	..	..	..
Ulcers of leg	106	30	53	8	11	2	2	..	..	..	..	..
Ulcers of other parts	15	4	1	3	3	..	2	..	..	2	..	..
Diseased glands	19	6	4	..	5	3	1	..	..	..	..	..
Strumous sores	21	5	1	6	7	2	..	..	..	..	..	..
Sinus	15	5	3	4	1	1	1	..	..	..	..	..
Total	447	125	157	57	59	20	27	4	..	10	8	..

12.—DISEASES OF THE EYE.

	Total.		Cured.		Relieved.		Uncured.		Other Causes.		Dead.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Anisotropia	13	1	..	4	4	3	1	..	..	..	..	..
Cataract	57	16	12	8	4	3	2	8	4	..	..	..
Conjunctivitis	28	8	9	5	4	1	1	..	..	..	..	..
Cornea, Opacity of	4	2	..	..	1	..	1	..	..	..	..	..
" Staphylococci of	6	1	3	..	1	..	1	..	..	..	..	..
" Ulcer of	13	4	4	..	2	2	1	..	..	..	..	..
Exotropia	1	..	..	..	1	..	..	..	..	..	..	..
Entropion	1	..	..	..	1	..	..	..	..	..	..	..
Glaucoma	1	..	..	..	1	..	..	..	..	..	..	..
Gonorrhoea eyelids	9	3	3	1	1	1	..	..	..	..	..	..
Hypertropia	12	2	3	4	3	..	..	..	..	..	..	..
Injury to eye	22	7	1	8	4	..	..	1	1	..	..	..
Iritis	22	10	6	2	3	1	..	..	..	..	..	..
Ophthalmia, Acute	8	3	2	1	1	1	..	..	..	..	..	..
" Chronic and strumous	40	13	19	4	3	1	..	..	..	..	..	..
Strabismus	4	..	3	..	1	..	..	..	..	..	..	..
Staphylococci and closed pupil	10	..	1	4	2	1	..	2	..	..	..	..
Other ophthalmic affections	15	2	4	3	1	3	2	..	..	..	..	..
Total	266	72	70	44	37	17	10	11	5	..	..	..

13.—DISEASES OF THE SKIN.

	Total.		Cured.		Relieved.		Uncured.		Other Causes.		Dead.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Acne	1	..	..	..	1	..	..	..	..	..	..	..
Carbuncle	7	4	3	..	..	..	..	..	..	..	..	..
Cheloid disease	2	..	1	..	..	..	1	..	..	..	..	..
Cicatrizes from burns	10	1	3	3	2	1	..	..	..	..	..	..
Eczema	18	4	8	2	1	2	1	..	..	..	..	..
Erythema nodosum	2	..	2	..	..	..	..	..	..	..	..	..
Erysipelas	28	11	13	..	1	..	..	..	..	3	..	..
Herpes	6	2	1	..	2	..	..	..	1	..	..	..
Impetigo	2	1	1	..	..	..	..	..	..	..	..	..
Leprosy	12	3	4	2	3	..	1	..	..	..	..	..
Lupus	6	3	1	1	1	..	..	..	..	..	..	..
Psoriasis	4	1	..	1	2	..	..	..	..	..	..	..
Pustula	4	2	1	1	..	..	..	..	..	..	..	..
Rupia	16	4	2	5	3	..	1	..	1	..	..	..
Urticaria	2	2	..	..	..	..	..	..	..	..	..	..
Deformity of mouth	4	1	..	..	1	..	2	..	..	..	..	..
Bedsore	1	..	..	..	..	..	..	..	..	..	1	..
Other affections	14	3	4	3	2	1	1	..	..	..	..	..
Total	141	43	45	19	17	4	7	..	2	3	1	..

14.—FEVERS.

	Total.		Cured.		Relieved.		Uncured.		Other Causes.		Dead.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Typhoid fever	50	19	15	5	1	..	..	..	..	..	1	9
Continued fever	22	10	9	2	1	..	..	..	..	..	..	..
Scarlatina	7	2	1	..	..	..	..	..	1	1	1	1
Small-pox	3	1	..	..	..	..	..	..	1	1	..	..
Intermittent fever	17	7	4	4	1	1	..	..	..	..	..	..
Total	99	39	29	11	3	1	..	2	2	2	10	..

15.—MISCELLANEOUS AFFECTIONS.

	Total.		Cured.		Relieved.		Uncured.		Other Causes.		Dead.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
General debility	24	8	3	5	6	..	2	..	..	..	..	..
Destitution and starvation	7	..	..	4	3	..	..	..	..	..	..	..
Convalescence after fever	6	..	1	3	2	..	..	..	..	..	..	..
Intoxication	4	..	..	2	2	..	..	..	..	..	..	..
Glanders	1	..	..	..	..	..	..	..	..	1	..	..
Gangrene	4	..	..	1	1	..	1	..	..	1	..	..
Obesity	1	..	..	..	..	..	1	..	..	..	..	..
Malingering	2	..	..	..	..	..	2	..	..	..	..	..
No disease	9	..	..	..	..	..	8	1	..	..	..	..
Disease unascertained	9	..	..	..	..	..	3	6	..	..	..	..
Total	67	8	4	15	14	..	2	15	7	2	..	..

OUT-PATIENT DEPARTMENT, 1861.

	Total.	Men.	Women.	Children.
Surgeon's cases	3091	1061	1378	652
Physician's cases	3247	1126	1588	533
Eye cases	1675	576	676	423
Obstetric cases	1903	..	1903	..
Total	9916	2763	5545	1608

Number of surgical cases registered, but not furnished with regular cards, has amounted to 26,074

Dispensary cases 6008

Number of minor accidents and urgent cases not requiring indoor treatment, and entered in the surgery books 1548

Teeth extracted in the course of the year 4060

Number of women confined and attended by the pupils during the past year has amounted to 1055

DETAILS OF MIDWIFERY DEPARTMENT.

Number of women delivered during the year	1505
Number of single births, 1485; twin births, 18; triplets, 2; total children	1527
Living male children 753	1st Confinement 225
Living female children 723	2nd " 252
Still-born male children 27	3rd " 217
Still-born female children 24	4th " 187
Total 1527	5th " 157
	6th " 127
	7th " 109
	8th " 94
	9th " 50
	10th " 44
Natural presentation 1474	11th " 14
Breech 24	12th " 13
Face 8	13th " 8
Footling 11	14th " 4
Arm 4	15th " 1
Fetus 4	16th " 3
Transverse 1	17th " 1
Placental 1	18th " 1
Total 1527	Total 1505

Deaths among mothers, 4. Causes—1, puerperal convulsions; 1, acute rheumatism; 1, hemorrhage; 1, uræmia.

RETROSPECTIVE SUMMARY FOR THE LAST EIGHT YEARS.

	1884.	1885.	1886.	1887.	1888.	1889.	1890.	1891.
Patients resident in } hospital	5,080	4,760	5,073	5,226	5,209	5,149	5,128	5,360
Dispensary out-patients	8,350	8,694	10,325	9,889	11,137	10,526	10,120	9,916
Dispensary casualties	17,518	21,285	21,036	25,886	22,637	24,764	27,858	37,342
Minor accidents	2,334	2,268	2,262	1,549	1,570	1,735	1,669	1,548
Women confined ..	1,738	1,733	2,011	1,731	1,651	1,649	1,494	1,505
Total ..	35,149	38,670	40,707	44,281	41,624	43,814	46,179	55,671

ON THE
TEMPERATURE, UREA, CHLORIDE OF SODIUM, AND
URINARY WATER

IN

SCARLET FEVER;

AND ON

A CYCLE IN DISEASE AND HEALTH.

BY

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1862.

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Received Jan. 4th.—Read Jan 26th, 1862.

THESE observations were made on inmates of the Hospital for Sick Children. The patients were under the care of Dr. West, Dr. Jenner, and Dr. Hillier. I beg to avail myself of this opportunity of thanking these gentlemen for affording me every facility in the prosecution of these investigations.

These experiments were undertaken to ascertain what relationship exists between the urinary excreta and the temperature in scarlet fever; and this paper is intended to be a sequel of my former paper on 'Ague' published by this Society.

The temperature is given in charts. The variations of each day are separated from each other by a red line, and

at the head of the column thus formed the day of the disease is given.

Above the temperature, five columns are given; the first, gives the hour of the day; second, gives the day of the month; third, the daily amount of urea; fourth, the daily amount of chloride of sodium; fifth, the daily amount of urinary water. The urea and chloride of sodium are given in grammes; the urinary water in cubic centimetres.

The urine was analysed daily; and the amounts of the different constituents for twenty-four hours are put down on the day on which the urine was collected, and therefore belong to the previous day; thus, when subsequently I refer to the urine of any day, I of course mean the urine collected on the morning of the following day.

The urea and chloride of sodium were analysed by Liebig's volumetric method. The chloride of sodium was not got rid of, but the usual allowance was made in estimating the quantity of urea.

The following are the subjects treated of:—1st. The temperature and its relationship to the urinary excreta. 2nd. The urea. 3rd. The chlorides. 4th. The urinary water. 5th. The albumen in the urine. 6th. The blood in the urine. 7th. A cycle in disease. 8th. A cycle in health, and its relation to the cycle in disease.

On the temperature in scarlet fever.

The duration of the elevation of the temperature varies; thus, in seven cases it fell on the fifth day of the disease, in four cases on the tenth day, in two cases on the fifteenth day, in one case on the twentieth day, in two cases on the fourth day, in one case on the sixth day, and in two other cases the temperature fell so gradually, that it was impossible to say exactly on which day it reached its normal point.

It thus appears that in the large majority of cases the temperature falls on the fifth, tenth, fifteenth, or twentieth day, that is either on the fifth or a multiple of the fifth day of the disease; so constant indeed is this, that if the

temperature should remain high on the fifth, tenth, or fifteenth day, we may look forward to a continuation of the fever for another five days. Further, in those cases in which the temperature continues high till the tenth, fifteenth, or twentieth day, it generally experiences a depression on the fifth day preceding its complete fall; for instance, if the temperature falls permanently on the fifteenth day, there generally occurs a depression of variable amount on the fifth and tenth days.

After each of these falls the temperature may rise again, reaching a point during the second five days, as high as, or higher than it did during the first five days, as in Cases 8, 17, 18, and 22. In other cases, after the fall on the fifth day, the temperature remains at this point till the following tenth day, on which day it experiences another abrupt fall, or it may gradually decline, passing almost imperceptibly into the normal temperature, reaching this point on the tenth day, as in Cases 5, 7, 16, 8.

Each of these falls in the temperature is accompanied by an improvement in the state of the patient, which is always in proportion to the extent of the fall. If the temperature again rises, the patient again relapses, but if it remains at the point it reached at its first fall the improvement is permanent.

The temperature does not usually attain the same height daily during each of the five days, but the point reached gradually increases during the second and third days, and then falls again towards the fifth; in some cases, however, it goes on increasing till the fourth day, and then experiences an abrupt fall on the fifth. It thus appears that the temperature (*i. e.* the fever) forms arcs or cycles, lasting over a period of five days. The height the temperature attained varied greatly in different cases; in some the elevation was just appreciable, in others it reached 105° Fahr.

Of seventeen cases that came early under notice, the average maximum temperature was a little over 103°. As might be expected, the temperature reached a higher point in the cases of long duration.

In some cases the highest elevation was attained on the first day, but in the severer cases it generally reached its highest point on the second or third, and perhaps not till the sixth or even the eleventh day.

Subsequent to the great fall experienced on the fifth, tenth, or fifteenth day, the temperature often remained over a variable period rather too high, the period being in some cases fourteen or fifteen days; the degree of elevation also varied, in some cases (Case 1) being between 100° and 101° , but more frequently between 99° and 100° . This slighter elevation of the temperature in most cases formed cycles of five days, as in Cases 2, 6, 10, and 11. In Case 11 this occurred after the second elevation on the twenty-sixth day.¹

This slight subsequent elevation of the temperature, if of any persistence, is coincident with a continuance of the lesions produced by the scarlet fever; as, for instance, of the sore throat, or of the enlargement of the gland, or from discharge from the ears, &c.

There is no connection between the duration of the primary high temperature and the subsequent slighter one; thus, it can be seen (Case 1) that in some cases, where the great fall in the temperature occurred on the fifth day, a long subsequent slight elevation occurred; it must, therefore, have depended on the constitutional state of the patient. It certainly sometimes precedes Bright's disease.

At a variable period after the scarlet fever, a subsequent high rise in the temperature not unfrequently occurred, due to intercurrent disease. In many cases the nature of the intercurrent disease was determined, in other cases nothing capable of causing the elevation of the temperature could be discovered.

The day of the disease (counting from the commencement of the scarlet fever) varied. In the cases of albuminuria

¹ It must be borne in mind that in counting the second cycle of five days we must not take in the day of the fall, as this belongs to the previous cycle, but must begin to count from the day after this.

it began on the seventh, nineteenth, twentieth, twenty-third, twenty-ninth, and thirty-third days.

In those cases in which the elevation was probably due to heart disease, it began on the eighth day. In those cases in which the cause could not be determined, it began in one case on the eighth and in the other on the tenth. In one case of urticaria and chicken-pox it began on the sixth day. In a case of tuberculosis, on the ninth. The degree of the elevation of the temperature varied thus:

4 cases	ranged between	102° and 103°
3 "	"	103° and 104°
4 "	"	104° and 105°
1 case	it was	$105\frac{1}{2}^{\circ}$

The duration of the elevation varied; thus, in—

2 cases	the temperature fell on the	second day.
1 case	"	fourth day.
3 cases	"	fifth day.
1 case	"	sixth day.
1 "	"	tenth day.
1 "	"	eleventh day.
1 "	"	thirteenth day.

The cause of the elevation also varied.

In 6 cases	it was due to	albuminuria.
In 1 case	"	tuberculosis.
In 1 "	"	chicken-pox.
In 2 cases	"	heart disease.
In 2 "	it was	uncertain.

On examining the previous cases, it will be seen that the rise in the temperature due to an intercurrent disease either fell on the fifth day from the commencement of its rise, or on the fifth day from the commencement of the scarlet fever; that is to say, the secondary rise in some cases adopts the cycle existing previously, or checks this and determines another.

Thus, in Case 1 the elevation lasted five days, and fell on

the twenty-seventh day of the disease, counting from the commencement of the scarlet fever.

In Case 17 the elevation fell on the fifth and tenth days from its own commencement, and as it began on the sixth day of the scarlet fever, it fell on the fifteenth day of that disease.

In Case 19 the secondary elevation fell on the fifth day from its own commencement.

In Case 22 it fell on the fifth, tenth, and twentieth days from its own commencement, and on the twelfth, seventeenth, and twenty-seventh of the scarlet fever. In this case I have counted the second rise from the eighth day, the two previous days being part of a previous cycle of the scarlet fever.

In the following cases the elevation caused by the intercurrent disease lasted a variable number of days, but fell on the fifth day, counting from the commencement of the scarlet fever.

Case 6.—It fell on the fourth day from its own commencement, on the tenth from the commencement of the scarlet fever.

Case 12.—It fell on the second day from its own commencement, on the twentieth from the commencement of the scarlet fever.

Case 20.—It fell on the thirteenth day from its own commencement; on the twentieth from the commencement of the scarlet fever.

In this case I have considered the second rise due to the intercurrent disease; to begin on the eighth day, and the two previous days to belong to the previous cycle of the scarlet fever.

Case 25.—It fell on the second day from its own commencement; on the twentieth from the commencement of the scarlet fever.

Case 26.—It fell on the sixth day from its own commencement; on the fifteenth, and again on the twentieth, from the commencement of the scarlet fever.

It is thus seen that the subsequent rise in the temperature

due to intercurrent inflammations, &c., strictly follow out the cyclical law, only one exception occurring; this was Case 23, in which the temperature fell on the sixth day from the commencement of the secondary rise, and on the thirteenth day from the commencement of the scarlet fever.

Sometimes after the fever had declined a sudden slight elevation occurred, limited to one day, and often to a few hours of the day, the patient being next day in the usual health and the temperature normal.

In 1 case it rose to 102½ on the twenty-fifth day,	} Counting from the commencement of the scarlet fever.
In 1 " " 100½ " fifteenth day,	
In 1 " " 101 " eleventh day,	

On the daily variations of the temperature.—In some cases the temperature remained at much the same height throughout the day; in other cases it fell a variable amount in the morning, and again rose during the day, forming a daily arc or cycle. The former generally occurred in the severer cases and at the commencement of the disease, and the latter at the termination, and may be taken as a sign of improvement. The time of the day at which the temperature reached its highest point varied between 9 a.m. and 11 p.m.; from 2 to 8 appears to be the most frequent hours. Two patients died from the immediate effects of the fever; one died on the fourth day of the disease; one died on the eighth day of the disease.

On the urea.

From the above cases it appears that the quantity of urea undergoes no increase with the elevation of the temperature, that is, during the fever days. If, indeed, we accept .800 grms. of urea per kilogramme of weight as the amount normal per diem in children, it often suffered a great diminution.

It may, however, be objected that this is too high; and, secondly, that in many of the previous cases the urea falls

with the decline of the temperature, and that this shows that it had previously undergone an increase.

It may be fairly assumed, I think, that the amount of urea voided by these patients during the last ten or fifteen days of their residence in the hospital was not in excess of the quantity in health, especially where the examination extended over a long period (thirty or forty days), at which time the quantity of urea could not have been affected by any previous retention during the fever. Indeed, it appears to me that, leading a quiet, hospital life, and often spending the time in bed, that this amount is too low; but even accepting this amount as normal to the patient, on no occasion was the quantity of the urea increased during the elevation of the temperature; often, indeed, it was below it. This is shown in the following table, in which I have first given the daily average amount of urea per kilogramme of the fever days, and that of several of the last days during which the analyses were made.

Case 3.	From the second to the fourth day inclusive	0.477
"	" tenth to the twenty-eighth day inclusive	0.657
Case 4.	" sixth to the ninth day inclusive	0.527
"	" nineteenth to the thirty-second day inclusive	0.706
Case 10.	" second to the third day inclusive	0.569
"	" twelfth to the twenty-seventh day inclusive	0.642
Case 18.	" second to the thirteenth day inclusive	0.552
"	" twentieth to the thirty-first day inclusive	0.634
Case 20.	" tenth to the twelfth days inclusive	0.561
"	" twentieth to the thirty-sixth day inclusive	0.551

That the subsequent decrease in the amount of the urea is not due to the decline of the temperature is seen in some cases, as the 18th and 20th, in which the urea falls about the same time as in the other cases, in spite of a continuation of the elevation of the temperature, as the temperature in these cases continued high a long time after the decline of the urea; this disassociation of the urea and the temperature plainly shows that the decrease in the amount of the urea is independent of the temperature, and therefore their occa-

sionally occurring at the same time in no way shows that there was an increased elimination during the fever.

After the first few days the urea invariably falls, reaching point often considerably below the normal amount; thus, in—

Case 1.	The average daily amount per kilogramme, from the sixth to the fourteenth day inclusive, was	0.417
"	The normal amount was	0.642
Case 3.	The average daily amount per kilogramme, from the sixth to the ninth day inclusive, was	0.400
"	The normal amount was	0.687
Case 4.	The average daily amount per kilogramme, from the eighth to the fourteenth day inclusive, was	0.422
"	The normal amount was	0.706
Case 10.	The average daily amount per kilogramme from the fourth to the eleventh day inclusive, was	0.457
"	The normal amount was	0.642
Case 18.	The average daily amount per kilogramme, from the fifth to the sixth day inclusive, was	0.439
"	The normal amount was	0.634
Case 20.	The average daily amount per kilogramme, from the tenth to the nineteenth day inclusive, was	0.417
"	The normal amount was	0.551

A diminution of this kind usually occurs after the subsidence of fevers, due probably to the appetite not being completely restored, and also to the food being more thoroughly assimilated and tissue change not being so active.

But in these cases of scarlet fever the above explanation will not, I think, hold; for, first, the diminution in the urea usually lasts only over a few days after other fevers, whilst in these cases it often remained for many. Secondly, we have previously seen that there is no increase in the amount of the urea coincident with the increase in the temperature, and thus, probably, there is retention in the system, either of urea or some product capable of producing urea. In similar cases of retention in typhoid fever we have a subsequent increase in the amount of the urea, instead of the diminution. Thirdly, it has been before stated that the great diminution occurs quite independently

of the temperature, sometimes even when the temperature for several days remains at even 103°.

Thus I have tried to show that—First, there is no increase in the amount of urea during the fever. Second, that, subsequent to the fever, there is a great diminution of the urea, &c. Third, that this diminution cannot be explained in the usual manner. May the following be the explanation—namely, that the contagion has a similar action on the kidneys to that on the skin, tongue, and fauces, for we find in the urine epithelium from the bladder, pelvis of the kidney, and from the kidney itself, during the fever days. It may be objected that the impaired functional activity of the kidneys lasts longer than the rash, but the organs involved do not at once recover their normal condition on the disappearance of the rash, as is seen in the case of the tongue and fauces.

In most of the cases, especially where the convalescence was slow, the children became puffy about the face, there being, however, no pitting on pressure; this occurred without the appearance of albumen or blood in the urine; perhaps this may be accounted for by the retention of urea in these cases being unusually marked.

On the occurrence of acute Bright's disease after the fever the urea was variously affected; in some cases it was greatly diminished in quantity.

Case 1.—The temperature rose on the twenty-third day to 102½°, and then gradually fell during the following three days; on the fifth day it was normal again. Daily average amount per kilogramme, from the sixth to the twenty-first days inclusive, 0.464. During the following four fever (inflammatory) days, 0.373. The urea immediately regained its full quantity; thus, from the twenty-seventh to the thirty-fifth it was, per kilogramme, 0.641.

In the above case the albumen never amounted to more than a mere trace, and thus, probably, did not, to an appreciable degree, interfere with the quantitative analysis; the albumen lasted only during the five days of elevation of temperature, and hence, if it had any effect on the analysis,

it must be to give too great an amount of urea, and thus the diminution in the amount of urea will be greater than that given.

Case 12.—In this case, on the occurrence of the inflammation, blood (with a very large amount of albumen) was found in the urine; the albumen was removed by animal charcoal. The rise in the temperature occurred on the nineteenth day (105°); on the following day it fell to 99 again. From the eighth to the eighteenth days inclusive, average daily amount of urea was 9.185. On the twenty-second day it was 1.875; on the twenty-third day it was 0.510.

I have given the total amount above, not the quantity per kilogramme, as they are so small during the above two days.

Case 24.—The urea was estimated quantitatively only till the twelfth day; after this it was discontinued till the sixty-third day, but, during the time, the patient had a severe attack of Bright's disease, with much blood and a large amount of albumen. On the fiftieth day she was much improved, on the sixty-first there was scarcely any anasarca, and from the sixty-third to the sixty-eighth the merest trace of albumen in the urine. Average daily amount of urea, per kilogramme, from the fifth to the twelfth days inclusive, 0.582. Average daily amount of urea per kilogramme, from the sixty-third to the sixty-eighth days inclusive, 0.268.

The last case not only shows how large the diminution often is, but that it may continue over a very long period, and long after active disease in the kidney has ceased.

Case 25.—Observations were made for eight days; on each day there was a variable amount of albumen, which was removed by animal charcoal; the temperature on three occasions rose gradually, over a space of three days, from normal to 105°; the urea showed no corresponding increase on these days. The average daily amount for the eight days was only 10.241 grains. His weight was not taken, but assum-

ing 26 kilogrammes, for each kilogramme there was 0.093 grms.

In other cases no diminution occurred on the occurrence of the inflammation; thus, Case 25, on the twenty-ninth day, Bright's disease supervened, judging by the sudden elevation of the temperature, and the appearance of albumen and blood in the urine, yet, notwithstanding, the amount of urea continued as it was before or very nearly so. Thus, from the sixteenth to the twenty-eighth days inclusive, the average daily amount per kilogramme was 0.523 grms. From the twenty-ninth to the thirty-fourth inclusive it was 0.488 grms.

On the chlorides.

The chlorides were never absent from the urine that was analysed; the quantity, however, was always much diminished during the fever days; thus, in—

Case 3, the daily average amount per kilogramme, from the second to the fourth day, was	0.032
Case 10, the daily average amount per kilogramme, from the second to the fourth day, was	0.054
Case 20, the daily average amount per kilogramme, from the first to the fourteenth day, was	0.080
Case 24, the daily average amount per kilogramme, from the fifth to the twelfth day, was	0.093

During the three or four days following the fever the quantity of chloride increased gradually; thus, in

Case 3, the average daily amount per kilogramme, from the fifth to the eleventh day, was	0.093
Case 10, the average daily amount per kilogramme, from the fifth to the eleventh day, was	0.125

After this they became normal in quantity; thus, in

Case 3, the average daily amount per kilogramme, from the tenth to the twenty-fifth day, was	0.210
Case 10, the average daily amount per kilogramme, from the thirteenth to the twenty-seventh day, was	0.228

In one case of Bright's disease the chlorides suffered scarcely any diminution in their quantity during the fever days; thus, in Case 1—

Average daily amount per kilogramme, from the twenty-third to the twenty-sixth day (fever days)	0.110
Average daily amount per kilogramme, from the twenty-seventh to the thirty-fourth day (non-fever days)	0.134

It still remains doubtful whether the chlorides are eliminated as a secretion, or merely as a solution in the urinary water; if it be secreted by the kidney through any diminution in the secretive power of the kidney, it should show itself in a diminution of the amount of the chloride of sodium.

In the case given above there was a decided diminution in the amount of urea during the fever days of Bright's disease, and this, I have attempted to show, was due to diminution in the secretive power of the kidney. But in this case the chloride of sodium was scarcely diminished; thus, this single case would go to show that the urea passes out in the form of a solution. The close relationship also between the amount of chloride and the urinary water tends to prove the same conclusion.

On the urinary water.

During the fever days the amount of urinary water varies very greatly; thus, in—

Case 10, the average daily amount per kilogramme was	11 c.c.
" 3,	12 "
" 4,	18 "
" 20,	20.2 "
" 18,	35.3 "
" 24,	66 "

During the subsequent non-fever days the amount also varied somewhat; thus, in—

Case 2, the average daily amount per kilogramme was 17 c.c.

" 18,	"	"	22 "
" 24,	"	"	23 "
" 3,	"	"	25.9 "
" 4,	"	"	26 "
" 10,	"	"	28.4 "
" 20,	"	"	27.6 "
" 25,	"	"	29.4 "

These last quantities may, I think, be taken as the amount usual per kilogramme in children; this gives an average of 24.6. I am aware this is much below the amount usually received; thus, Dr. Parkes¹ gives 59 c.c. The above quantity agrees very closely to the amount given by Dr. Parkes per kilogramme in adults.

If this assumption be correct, it follows that often, during the fever days, there is no decline in the amount of urinary water, and in some cases there is a very large excess.

On the albumen in the urine.

The albumen appears at two different periods; first, during the fever, and second, later—about the third week—at which time it is often much larger in quantity, longer in its duration, and accompanied by a second elevation of the temperature.

1st. Its appearance during the fever days, out of twenty-one cases in which the urine was examined.—In only one did any albumen occur; it was slight in amount, and was present on the fifth and sixth days. It must be borne in mind, however, that in many of these cases several of the fever days had elapsed before their admission.

2nd. Its appearance during convalescence of eighteen patients who were in the hospital for a considerable time.—Seven had albumen during the period; three other cases are given; these were admitted with albuminuria.

¹ 'On Urine,' p. 44.

The time of its appearance varied; thus—

In Case 6 it appeared on the ninth day.

" 25	"	eighteenth day.
" 12	"	twentieth day.
" 1 and 19	"	twenty-third day.
" 11	it first appeared on the eleventh day, but after a day or two it discontinued, and reappeared on the twentieth.	

The duration of the albuminuria varied; thus in—

Case 1, it lasted three days.

" 6,	"	eleven days.
" 25,	"	twenty days.
" 11,	"	more than twenty-seven days.
" 19,	"	forty-three days.
" 12,	"	forty-nine days.

Taking the elevation of the temperature as a test of the severity of the inflammation, it is seen that there is no necessary connection between the intensity and duration of the inflammation and the duration of the albumen in the urine. This can be seen by comparing the following table with the one above:

Case 1, the temperature reached 102, and lasted four days.

" 6,	"	103,	"	three days.
" 25,	"	104,	"	one day.
" 19,	"	102½,	"	four days.
" 12,	"	105,	"	one day.

Moreover, there is no necessary relationship between the temperature and the amount of the albumen. This can be seen by comparing the following table with the one above:

Case 1, very slight trace.
" 6, an enormous amount.
" 25, very slight trace.
" 19, very large quantity.
" 12, a large quantity.

The albumen appears in the urine very soon after the elevation of the temperature; thus, in—

- Case 1, the temperature rose on the twenty-third day; albumen appeared on the twenty-third day.
 Case 6, the temperature rose on the seventh day; albumen appeared on the ninth day.
 Case 11, the temperature rose on the eleventh day; albumen appeared on the eleventh day.
 Case 12, the temperature rose on the nineteenth day; albumen appeared on the twentieth day.

On blood in the urine.

Relationship between the elevation of the temperature and the appearance of blood in the urine:

1st. No blood may follow the rise of the temperature. Case 1, there was no blood. Case 6, never smoky; urine not examined microscopically.

2nd. In those cases in which blood appeared its appearance was always preceded by a rise in the temperature. Case 11, temperature rose on the twentieth day; blood appeared on the twenty-first. Case 12, temperature rose on the nineteenth day; blood appeared on the twentieth. Case 25, temperature rose on the twenty-ninth day; blood appeared on the twenty-ninth.

The blood in some cases continued long after the decline of the temperature; thus, the continuance of blood cannot be taken as evidence of a continuation of the inflammatory action of the kidney; thus, in Case 11, the temperature fell on the twenty-seventh; the blood continued in the urine till the thirty-seventh day. Case 12, the temperature fell on the twentieth; the blood continued in the urine till the twenty-first day. Case 19, the temperature fell on the thirty-seventh; the urine contained blood till after the sixty-fifth day. Case 25, the temperature fell on the thirtieth day; the urine contained blood till the forty-first day.

Relationship between the blood and the amount of albumen in the urine.

1st. A very large amount of albumen may occur in the urine without any blood, as in Case 6.

2nd. Blood, even to a large amount, may exist with the slightest trace of albumen; and if the blood-corpuscles be allowed to settle to the bottom of the vessel, the supernatant fluid may give no evidence of any albumen by heat and nitric acid, as in Cases 25 and 30.

The cases given were seldom dropsical to the extent of pitting; they, however, often looked puffy in the face. Seven cases gave no pitting on pressure; one case very slight; one case the body pitted deeply on pressure.

In some cases the second elevation of the temperature was followed by a puffiness.

In one case the patient was puffy about the face, without any elevation of the temperature or other evidence of Bright's disease.

In some cases the puffiness about the face preceded the rise of the temperature one or two days. In other cases the rise in the temperature preceded the puffiness¹ for a variable number of days; thus, in Case 6, six days; Case 11, three days; Case 12, one day.

On a cycle in disease.

In these cases it appears that the temperature does not run an equable course, neither remaining at the same temperature throughout, nor observing a period of elevation and decline, but forms arcs or cycles composed of a variable number of days, each cycle, however, being composed of the same number of days in the same patient.

We see from the cases given that no number of days

¹ When I use the term puffy, there was no pitting of the body on pressure.

composing the cycle is peculiar to any disease, but that five days is the most common in all.

In rare cases, in the same patient the number of days composing the cycle may change.

For a further account of the cycle in disease, I must refer to the account given of the temperature.

On a cycle in health.

We have seen that in fevers the temperature forms cycles, and as the variations in the temperature are due to the variations in the amount of tissue change, it follows that we must have a cycle of tissue change. Does such a cycle exist in health? I think it probable, and that this determines the cycles in fever, and not the disease itself, for—

1st. In the same disease the cycle may be composed of a different number of days, the cycle, however, remaining constant in the same patient. If the disease determined the cycle, we should expect it to be composed of the same number of days in all patients, whilst we can readily conceive different people to have a different cycle.

2nd. Different diseases (as scarlet fever, followed by Bright's disease, heart disease, chicken-pox, &c.), occurring in the same patient, adopt the same cycle.

3rd. Not unfrequently after the decline or during the decline of the scarlet fever a second elevation of the temperature occurred, due to some intercurrent disease. This second elevation of the temperature lasted over a very variable number of days, but, with one exception, it fell on either a fifth day from its own commencement or on a fifth day from the commencement of the scarlet fever.

In the latter case the intercurrent elevation of temperature, if of some duration, forms cycles, the first of which cycles only is irregular, the subsequent ones being composed each of five days. If the intercurrent inflammation determined the cycle, we should have expected its first cycle to have lasted five days also, instead of which it appears to adopt a previously existing one; but having commenced in

the middle, it is composed of a shorter number of days. This will be more clearly shown by a typical case.

A case of scarlet fever in which the temperature forms cycles of five days, having again fallen to the normal temperature, is followed on the twenty-second day by another elevation, due to intercurrent disease; this experiences a fall on the twenty-fifth, thirtieth, thirty-fifth, and fortieth days from the commencement of the scarlet fever. The three last cycles of the intercurrent disease being each composed of five days, shows that the normal cycle is one of five days, and that the one which immediately preceded them, composed of only four days, is irregular, but only with regard to its commencement, as its fall takes place on a fifth day from the beginning of the scarlet fever.

The previous elevation of temperature of the scarlet fever affords data from which we can calculate the subsequent cycles, and the intercurrent inflammation corresponds exactly with these, with the exception of the elevation of temperature of the imperfect cycle, which begins in the middle of one, showing that diseases adopt previously existing cycles. In the other case, in which the intercurrent inflammation falls on the fifth day from its own, and not from the fifth day from that of the scarlet fever, it is probable that the intercurrent disease cuts short the immediately preceding cycle, but another one commencing it adopts this.

It may, however, be said that this cyclical condition preceding and influencing any subsequent inflammation was established by the preceding scarlet fever; this I think improbable, for the following reasons:

(A) If the disease determines its cycle, it appears difficult to me to conceive that it should be amenable to the influence of a previous disease, whilst it is readily explained by the supposition that both are influenced by a cycle existing in health.

(B) It is difficult to conceive how the scarlet fever should affect the cycle of a subsequent intercurrent disease, occurring

as it sometimes did, so many days afterwards, in some cases the subsequent elevation not occurring till the thirtieth day.

(c) In a case of pneumonia, the temperature of which I took, the first cycle was composed of six days; this was followed by a cycle of ten days (two five days' cycles) and by a five days' cycle. This is a case similar to those occurring after Bright's disease, in which the elevation began probably in the middle of a cycle; but in this case there was no preceding fever to give rise to a cycle which could modify the subsequent elevation, and as it began probably in the middle of a cycle, of course this implies its previous existence, that is, in a state of health.

It may be said this cyclical variation in health can be at once put to the test by taking the temperature in health, and seeing if any such variation exists. But in speaking of cyclical changes of tissue, I mean nitrogenous tissues solely, of which the temperature in fevers is the exponent; for in fevers probably the hydrocarbons are not at all attacked, for it is said the amount of carbonic acid is not increased, and I have attempted to show in a previous paper that in ague the temperature holds the closest correspondence to the amount of urea excreted, that is, to the amount of nitrogenous matter consumed.

But in health the temperature is largely derived from the combustion of the hydrocarbons of the body, and thus it cannot be taken as a guide to the change occurring in the nitrogenous tissues; indeed, in health it is probable that the hydrocarbons and the nitrogenous tissues are reciprocal.

Dr. Parkes tells me he has noticed that the urea in Bischoff's and Voet's experiments observed cyclical variations.

Thus, I think it probable that in health we have cycles of tissue (nitrogenous) changes, and in fevers we have the same greatly increased. But if this correspondence holds good between the cycle of health and disease, it is probable that it holds as to the constituent parts of the cycle; and if so, we have a daily cycle in health corresponding to that in fevers, but of a less degree.

If the above views be correct, it follows that in fevers we have but an increase of the normal tissue change, amenable to the ordinary laws of health.

On the temperature in fevers.

On looking over the cases given, it will be seen that in some the temperature remains almost permanent at an elevated point; in others there are daily elevations and falls of variable intensity; the persistence of the elevation of the temperature occurs during the most severe period of the fever; as it improves, the highest point the temperature reaches may remain the same, but the daily fall increases, this being often the earliest indication of improvement. This has been already noticed in typhoid, but it also holds good in scarlet fever, but to a less degree, partly owing to the fever throughout its course being severe, not having a period of slow increase and decline; however, even under these circumstances, it is not so marked as in typhoid fever.

The manner in which the temperature becomes persistent during the more severe time of the fever appears to me to be the following.

There is originally a daily rise, or perhaps it is limited to a part of a day; but as the fever increases in severity, not only does the extreme elevation of the temperature increase, but also the duration of the daily fit; indeed, after the temperature has attained a variable height, it ceases to rise higher, but the duration of the rise continues to increase.

Thus, suppose it possible to isolate a distinct fit, consisting of the rise and fall of the temperature, in a severe case the temperature, not having had sufficient time to reach its normal point on one day, would encroach on the following; but there occurring another rise on the second day, before the previous rise has had time to complete its fall, the temperature of the two days coalesce at a certain point, and the longer the duration of the separate fits the higher will be the point at which they will coalesce. These steps are best followed in typhoid fever, in which, at the commence-

ment and decline of the fever, we often have a complete daily fit, in some cases rising from the normal temperature to 105°, and again falling to the normal temperature; this may be repeated for several days;¹ as however, the fever increases in severity the daily falls in the temperature diminish till the fever becomes almost persistent in severity.

The same occurs also in miasmatic fevers. In these, when not severe, we have daily a complete fit; but as the fits increase in severity the fever becomes continuous, and as it again declines it first becomes remittent, and ultimately intermittent.²

From the above it follows that, theoretically, we have no such thing as continued fever.

On looking over the above case, it will be seen that the time of the day at which the elevation of the temperature begins varies greatly; thus, it may begin early in the morning, and in rare cases its commencement may be postponed till 4 p.m. or even 9 p.m. If fevers be merely an exaggeration of the normal daily cycles, it follows the commencement of the fever will correspond to the commencement of the normal cycle of that day; this will account for the various hours of the day at which the fever first begins. In scarlet fever we can fix the commencement of the attack with the greatest precision, as is well known; it may begin at almost any hour of the day; thus, in the cases given, it began in one case at 1 a.m., two cases during the morning, one case at 11 a.m., three cases at 12 a.m., one case at 2 p.m., four cases at 5 p.m., one case at 10 p.m., one case during the night.

By observing the variation in the elevation of the temperature, we may learn the variations in health; from this we see that the period of the day at which the temperature reaches its maximum varies greatly on different days of the

¹ Not only have we in these cases a distinct fit as regards the temperature, but also as regards the symptoms, there occurring in these cases a distinct hot and sweating stage daily.

² 'Wood,' vol. i, p. 240.

same case; thus, it may reach its maximum at 9 a.m., or as late as 10 p.m., or at any other intermediate hour.

Dr. Edward Smith has shown that there is a daily capacity for increased tissue change. He has endeavoured to show that this occurs during midday; from the above cases it appears that it may occur at any hour of the day.

The cases from which the preceding deductions are made are now added. I have been obliged to omit many of the charts of temperature, owing to the great expense. The Council of the Society, have, however, kindly allowed me to retain the most typical.

CASE 1 (see Diagrams 1, *a* and *b*).—Henry Maylin, æt. 6; weight, 16 kilogrammes. He was seized at 5 p.m., May 1st, with shivering, and complained of some sore throat; during the night he was heavy and feverish.

The rash appeared on the second day of the disease, and disappeared on the eighth. The rash was strongly marked on the second day of the disease; from that time it gradually faded.

Desquamation began on the sixth day of the disease, was completed generally between twenty-second and twenty-sixth days, though some still continued on the hands and feet on the twenty-ninth day. The desquamation was most marked under the clavicles and over the pubes.

The conjunctivæ were very much injected on the second day of the disease; this then declined, and ceased on the seventh day.

The glands at the angle of the jaw were found to be rather enlarged on the second day. From the sixth to the eighth days inclusive they were still more enlarged, and, indeed, increased in size till the twenty-third day. On the twenty-sixth day they had begun to diminish. Glands down the upper half of the neck posterior to the sternomastoid were rather enlarged and shotty till the twenty-sixth day of the disease, when they were found to be normal. Glands in groins were found to be enlarged on the second day; they increased in size till the seventh day inclusive,

after which they diminished, and became normal between the twenty-second and twenty-sixth day of the disease.

The tongue was thickly coated on the second day; from the fourth to the sixth inclusive it was of the well-marked strawberry kind; after this it improved, and became normal on the eleventh day.

The tonsils were vividly red and swollen till the fourth day; all redness had disappeared on the eleventh day, but they were found still enlarged on the twenty-fourth day.

The boy's appetite improved on the fourth day, and remained good till the twenty-second; on the twenty-eighth day it again became excellent.

Heart was healthy on the sixth day; no other note was made until the eleventh day, when systolic basic murmur was heard audibly at the second left cartilage; this continued audible till the fourteenth day, and disappeared suddenly on the fifteenth; from this time to the time of his dismissal from the hospital his heart sounds continued normal.

Some cough was noticed on the seventh day of the disease; on the twenty-second day it was very troublesome, and had all the character of hooping-cough, and continued so till the time of his discharge.

The boy was delirious only on the night of the second day of the disease.

Urine.—The urine was examined daily; no albumen was found till the twenty-fourth day, on which day there was a very distinct cloud on boiling and adding nitric acid; after this it decreased in quantity and disappeared on the twenty-seventh day; subsequent to this the urine always remained free from albumen; no blood nor casts were detected.

General appearance.—Till the tenth day the lips were dry, and the boy heavy; after this he steadily improved, though very slowly so, till the nineteenth day, when he flagged again, and on the twenty-second was puffy about the face, and the angles of his mouth and along the nose were ulcerated; after the twenty-third day he improved, though he still looked puffy till the twenty-sixth day. There was no distinct pitting on pressure any time.

Diet.—Second day, beef tea 1 pint, milk 1 pint, wine 4 oz.; seventh day, pudding diet, wine 3 oz.; eleventh, wine 2 oz.; thirteenth day, fish; twenty-first, second meal diet, 2 oz. of meal, 3 oz. of potatoes.

CASE 2.—Henry Johnson; height, 3 feet 1½ inch; weight, 10 kilogrammes. The child was seized during the night of the 23rd of May.

The rash appeared on the second day of the disease, disappeared on the seventh.

Desquamation, which was slight and very superficial, began on the tenth day, still continued on the feet on the twenty-ninth day. Glands at the angles of the jaw were rather enlarged, even on the twenty-ninth day. Glandula concatenata were not enlarged till the ninth day; on that and the following day both they and the glands at both angles were enlarged; on the twelfth day they were normal on the right side, but continued enlarged on the left till the twenty-third. Glands in groins were shotty till the tenth day.

Tongue was very red with prominent papillae from the fourth to the sixth day inclusive; it then improved, and became normal between the fourteenth and nineteenth days.

Fauces, &c., were red and swollen on the fourth; after this the parts improved, and were normal on the tenth day. Appetite was pretty good on the fifth day; after this it continued excellent.

Heart was healthy throughout.

Lungs.—There was some sonorous rhonchus on the fourth, fifth, and sixth days.

Urine was examined daily; it never contained any albumen.

General appearance.—Throughout the time he was under observation he looked quite well.

Diet.—May 26th, milk 1 pint, beef tea 1 pint, bread and butter; 31st, pudding diet; June 4th, broth diet; 10th, second meal diet; 16th, pudding diet, milk half pint.

Table showing the daily amount of urea, chloride of sodium, and urinary water, excreted.

Day of disease.	Urea.	NaCl.	Water.
	Grammes.	Grammes.	
Fifth	5.642	1.313	185 c.c.
Sixth	6.120	2.486	255
Seventh	8.100	4.050	360
Eighth	8.100	4.225	360
Ninth	7.965	1.485	270
Tenth	10.660	3.382	410
Eleventh	9.190	2.480	310
Twelfth	6.045	1.782	310
Thirteenth	7.695	2.389	405
Fourteenth	6.840	1.680	380
Fifteenth	Lost	Lost	Lost
Sixteenth	8.625	1.983	345
Seventeenth	8.407	1.778	295
Eighteenth	6.697	1.681	235
Nineteenth	4.702	1.650	165
Twentieth	7.055	2.695	245
Twenty-first	7.875	2.250	250
Twenty-second	7.370	1.650	220
Twenty-third	8.360	1.650	220
Twenty-fourth	8.750	2.625	250
Twenty-fifth	10.867	3.450	345
Twenty-sixth	7.095	2.257	255
Twenty-seventh	10.075	3.332	310
Twenty-eighth	9.440	2.802	295
Twenty-ninth	7.685	2.030	290
Thirtieth	8.000	1.875	250

The temperature reached its lowest point on the fifth day of the disease.

CASE 3.—John Franklin, æt. 11 years 6 months; assumed weight, 30 kilogrammes. The boy has felt poorly during the last fortnight, complaining of headache and sickness, but continued at his work till January 17th, on which day he vomited once or twice, and on the same evening the rash appeared.

Rash was general, second, third, and fourth days. Disappeared on fifth.

Desquamation began on the hand and leg on the eighth day, was general by the sixteenth; patient discharged on the

twenty-ninth. Desquamation then completed, except on the limbs.

Tongue thickly coated with white fur on the second day, was strawberry from the fourth to sixth inclusive. After this it improved.

The tonsils, pharynx, &c., were very red on the second and third days. They improved from the sixth.

The appetite improved on and after the sixth day.

The boy improved in appearance greatly on the fifth day.

The urine never contained any albumen.

Table showing the daily amount of urea, chloride of sodium, and urinary water excreted.

Day of disease.	Urea.	NaCl.	Water.
	Grammes.	Grammes.	
Third	11.160	0.465	310 c.c.
Fourth	15.624	1.176	420
Fifth	16.175	1.290	430
Sixth	Lost	Lost	
Seventh	11.882	2.425	485
Eighth	13.563	3.255	775
Ninth	15.960	3.657	665
Tenth	6.676	1.862	245
Eleventh	15.750	7.000	700
Twelfth	13.982	6.098	555
Thirteenth	14.965	8.200	820
Fourteenth	16.877	5.887	785
Fifteenth	15.562	7.750	750
Sixteenth	19.080	4.402	795
Seventeenth	22.370	4.698	895
Eighteenth	22.372	6.969	1010
Nineteenth	23.205	6.915	
Twentieth	19.550	6.375	850
Twenty-first	21.750	4.575	750
Twenty-second	22.230	6.184	780
Twenty-third	24.000	6.100	800
Twenty-fourth	22.500	8.190	900
Twenty-fifth	22.312	6.512	875
Twenty-sixth	25.440		1060
Twenty-seventh	23.010	6.016	885
Twenty-eighth	19.500	4.950	825
Twenty-ninth	27.502	6.755	965

The temperature reached its lowest point on the sixth day of the disease.

CASE 4.—William Stephens, *et. 5*; height, 3 ft. 1 inch; weight, 14 kilogrammes. On April 25th he vomited, complained of sore throat, and had a slight cough. On April 26th he had a slight blush of redness all over him; this increased in intensity till April 28th. Admitted on the sixth day of the disease; all the rash had disappeared, leaving a yellowish mottling. No desquamation occurred till the twenty-fifth day, when there was slight superficial but decided desquamation on the thighs, legs, and dorsum of feet; none on the soles of the feet, slight also on both shoulders. No further note was taken till the twenty-ninth day, when all desquamation had ceased.

Glands at the angles of the jaw were very much enlarged from the sixth to the nineteenth inclusive; they then declined, but still were too hard on the twenty-ninth.

Neck, first noted on the seventeenth, and to the twenty-second inclusive they were enlarged, that is to say, those close to the head, those immediately above the clavicle, were normal.

Groin, from the sixth to ninth inclusive, they were enlarged, hard, and shotty; they then declined, and on the twenty-fifth were normal in size, but too hard. On the twenty-ninth the left were normal, but the right were still too hard. On the seventh and eighth days were seen several red, hard elevations, about the size of peas, not unlike bug-bites, but without any central black spot or trace of bite; these were surrounded by a little ring of redness. The alae of his nose were excoriated, and discharged at the time of admission, and continued in much the same state till the twelfth day, when it was rather better, but still remained red and excoriated till the twenty-fifth day, when they were all but healed.

Tongue thickly coated; tip and edge red, with prominent papillae, sixth to eighth day inclusive; fur cleared off from the anterior part, but did not leave it red, on the ninth; from the eleventh day inclusive it was normal.

Tonsils.—Tonsils, uvula, &c., very deeply injected and swollen, whole posterior fauces covered with a thick, tena-

cious, opaque-looking membrane, from the sixth to eighth day inclusive. The redness then declined and the membrane thinned away, till, on the fourteenth, the membrane was replaced by a more purulent-looking matter. On the fifteenth the parts were decidedly better, and on the nineteenth they were normal.

The child's appetite was bad till the eighth day inclusive; it then improved, and after the ninth day continued excellent.

Heart healthy throughout.

Some sonorous rhonchus on the seventh, eighth, and ninth days; lungs otherwise healthy.

Throughout the time he was in hospital the child looked tolerably well; rather pale.

Urine at no time contained any albumen.

The child was admitted on the sixth day of the disease. On the sixth day the temperature rose to $102\frac{1}{2}$; on the seventh to $102\frac{1}{2}$; on the eighth to $100\frac{1}{2}$; on the ninth to 100 ; and on the tenth to 99 . After this it daily rose to 99 till the nineteenth day.

Table giving the daily amount of urea, chloride of sodium, and urinary water excreted.

Day of disease.	Urea.	NaCl.	Water.
	Grammes.	Grammes.	
Seventh	7.477		245 c.c.
Eighth	10.080		320
Ninth	5.740		205
Tenth	6.240		240
Eleventh	5.290		230
Twelfth	6.977		325
Thirteenth	5.945		260
Fourteenth	4.857		355
Fifteenth	6.300		420
Sixteenth	8.600		400
Seventeenth	7.540		580
Eighteenth	8.225		470
Nineteenth	8.505	2.301	315
Twentieth	10.360	3.420	380
Twenty-first	10.450	3.420	380
Twenty-second	11.895		390
Twenty-third	11.025		315

Day of disease.	Urea.	NaCl.	Water.
	Grammes.	Grammes.	
Twenty-fourth ...	12.425		350 c.c.
Twenty-fifth	10.920		380
Twenty-sixth	Lost		
Twenty-seventh...	8.540	2.754	270
Twenty-eighth ...	10.370	3.400	340
Twenty-ninth ...	9.715	2.827	290
Thirtieth	7.525	1.128	215
Thirty-first.....	6.880	1.612	
Thirty-second ...	9.310	4.650	490

CASE 5 (see Diagram 4).—Charles Macdonald, *et. 5*. Seized on September 25th; rash first appeared on the 27th; sore throat on the 26th. Rash was universal, and most remarked over the legs on the fourth day, then declined, and only a slight punctiform redness on the legs on the eighth day of the disease. Desquamation began about the ears and trunk on the eighth day. Glands at the angles of the jaw and in the groins were enlarged. Numerous sudamina noticed on the fifth day.

Tongue devoid of fur, rather red; papillae rather prominent on the fourth day; tongue pale on the fifth and eighth days, and was normal on the eleventh.

Fauces, &c., rather red, but not swollen, on the fourth day. The redness disappeared, but they were rather swollen and covered with muco-purulent matter on the fifth day; natural on the eighth; no note taken between fifth and eighth days.

Appetite never bad.

Urine examined daily to the sixteenth day. No albumen at any time in the urine.

A very slight attack. Marked improvement was noticeable, however, with each fall of the temperature.

CASE 6 (see Diagram 3).—Thomas Stephens, *et. 4*. Rash appeared on the second day of the disease, and lasted only one day. Desquamation began on the eleventh day. It had all but ended on the seventeenth day. On the third

day the child greatly improved, and continued to mend till the seventh day, when he became restless and dull. On the eighth day numerous wheals of urticaria appeared on his arms; these continued to appear till the eleventh day, on which day they had all disappeared.

Urine was tested daily. On the ninth day there was a slight cloud of albumen; tenth, ditto; twelfth, there was a very large quantity of albumen; thirteenth, enormous amount of albumen; fourteenth, ditto; fifteenth, the quantity of urine greatly increased, the albumen less; sixteenth, ditto; seventeenth, no albumen in the urine; eighteenth, distinct cloud discovered; nineteenth, slightest trace; twentieth and twenty-first, no albumen.

On the twelfth day the child was drowsy; on the fifteenth he was rather puffy about the face. At no time was there any pitting of the legs nor face on pressure.

Heart always healthy.

CASE 7.—Duchatel, male, *et. 5* years 6 months; was seized on October 12th, between dinner- and tea-time. Rash appeared on the thirteenth; on the third day of the disease the temperature reached 103° ; on the fourth, $102\frac{1}{2}$; on the fifth, $100\frac{1}{2}$; on the sixth, 101 ; on the seventh, 100 ; on the eighth, $98\frac{1}{2}$; on the ninth, 99 ; on the tenth, 98 .

CASE 8.—Welsh, male, *et. 3* years 6 months. On the ninth day of the disease the highest temperature was $102\frac{1}{2}$; on the tenth, $101\frac{1}{2}$; on the eleventh, $103\frac{1}{2}$; on the twelfth, $102\frac{1}{2}$; on the thirteenth, $102\frac{1}{2}$; on the fourteenth, $101\frac{1}{2}$; on the fifteenth it fell throughout the day, and reached $99\frac{1}{2}$, and continued afterwards at about this temperature.

CASE 9.—Eliza Brown, *et. 11* years. Rash appeared on the second day. The urine contained no albumen during her stay in the hospital. On the second day of the disease the temperature rose to 102 ; on the third, 102 ; on the fourth, 99 ; and continued at this last temperature on all the following days.

CASE 10.—Anne Johnson, æt. 7; height, 3 ft. 5½ in.; weight, 16 kilogrammes. She first complained of some sore throat at dinner-time, May 25th, but continued in other respects in her usual health till tea-time, when she lay down and complained of headache and chilliness. During the night she was feverish and vomited.

The rash appeared on the second day of the disease, and disappeared on the sixth. Desquamation began on the sixth day of the disease, and was almost completed on the twenty-seventh. Some milium vesicles were observed on the third day. The conjunctivæ were injected on the second and third days of the disease.

The glands at the angles of the jaw were never enlarged. The glandula concatenatæ were rather enlarged and hard till the twelfth day, when they were found to be normal. In the groins they were enlarged to the size of peas and beans; they became normal between the sixteenth and nineteenth days.

The tongue was very little affected; it was red, with prominent papillæ at the tip and edges, till the seventh day, and was normal on the twelfth.

The throat was vividly red and rather swollen on the second and third days. It became healthy on the eighth. The appetite was bad till the seventh day, when it improved.

Heart was healthy till the fifth day, when a slight systolic basic murmur was heard. This disappeared between the twenty-second and twenty-seventh days.

The child was slightly delirious on the nights of the second and third days.

The urine was examined daily. It never contained any albumen.

General appearance.—The child never appeared at any time at all oppressed by the fever.

Diet.—May 26th, beef tea 1 pint, milk 1 pint, wine 4 oz. 28th, omit wine. 31st, pudding diet, milk 1 pint. June 4th, broth diet. 10th, meal diet.

The child was admitted on the second day of the disease.

On the second day of the disease the temperature rose to 102½; on the third, 102; on the fourth, 100½; on the fifth, 99½; and continued to rise daily to this last temperature till the tenth, when it only reached 98.

Table giving the daily amount of urea, chloride of sodium, and urinary water.

Day of disease.	Urea.	NaCl.	Water.
	Grammes.	Grammes.	
Third	9.112	6.798	225 c.c.
Fourth	9.160	6.960	250
Fifth	6.075	6.911	135
Sixth	8.997	1.992	205
Seventh	6.090	1.450	220
Eighth	6.772	1.827	315
Ninth	Lost		
Tenth	7.680	2.400	480
Eleventh	7.655	2.025	450
Twelfth	7.905	2.325	405
Thirteenth	9.625	4.012	535
Fourteenth	10.875	4.350	580
Fifteenth	7.800	2.640	480
Sixteenth	8.407	2.975	415
Seventeenth	8.797	3.191	345
Eighteenth	8.400	3.990	420
Nineteenth	8.062	3.082	375
Twentieth	10.500	3.250	700
Twenty-first	11.812	3.370	375
Twenty-second	9.560	2.470	260
Twenty-third	11.000	3.212	440
Twenty-fourth	12.235	4.827	645
Twenty-fifth	11.115	3.700	390
Twenty-sixth	11.375	4.225	650
Twenty-seventh	12.670	4.870	650
Twenty-eighth	12.420	3.510	540

CASE 11.—George Gibbs. No note was made of the date of the commencement, but merely of the day on which the rash appeared; this I have assumed to be the second day.

The rash disappeared on the eighth day; desquamation began on the eighth day, and was mostly completed on the twenty-seventh day. Some still remained on the feet and ankles till the thirty-ninth, and on the soles of the feet till the forty-seventh day.

Glands.—At the angles of the jaw they were enlarged; began to diminish on the eighth day, but were still somewhat enlarged on the thirty-first.

Groins, here they were so much enlarged as to be visible; they began to diminish on the ninth day, but were still spotty on the thirty-first.

Urine.—No albumen before and on the eleventh day; slight cloud on the twelfth; increased in quantity on the thirteenth; slight cloud from the fourteenth to the sixteenth inclusive; none on the seventeenth; none till the twentieth, on which day a large quantity was discovered. Still more albumen, urine smoky, on the twenty-first; albumen less, rather smoky, on the twenty-third; much more smoky on the twenty-sixth; very smoky, less albumen, on the twenty-seventh and twenty-eighth; less smoky on the twenty-ninth and thirty-first; not smoky, a slight cloud of albumen, on the thirty-fourth; very slight on the thirty-fifth; smoky, rather dense cloud of albumen, on the thirty-sixth and thirty-seventh; albumen less on the thirty-eighth; not smoky, and only slight cloud of albumen on the thirty-ninth and fortieth, and continued so to the forty-sixth day.

General condition.—Progressed favorably till the night of the eleventh, when he complained greatly of pain in the left ear, which continued during the twelfth day. Was puffy about the face, and pitted over the forehead on the twenty-second. This increased somewhat till the thirty-first day inclusive, and then declined.

Temperature.—On the second day of the disease the temperature reached to $102\frac{1}{2}$; on the third, $103\frac{1}{2}$; on the fourth, $101\frac{1}{2}$; on the fifth, $99\frac{1}{2}$.

It continued to rise daily to this point till the eleventh day, when it rose to 101 . From the twelfth to the seventeenth days inclusive it never rose higher than $99\frac{1}{2}$.

The temperature was not taken on the eighteenth and nineteenth, but on the twentieth day it reached to $105\frac{1}{2}$; on the twenty-first, $99\frac{1}{2}$; on the twenty-second, $101\frac{1}{2}$; on the twenty-third, $99\frac{1}{2}$.

It then continued at about 99 till the twenty-sixth day

when it again rose to $103\frac{1}{2}$. From the twenty-seventh to the fortieth day it remained at about 99 .

CASE 12.—Ann Swinson; height, 3 feet 6 inches; weight, 163 kilogrammes. She was admitted for supposed caries of the crest of the ilium. The scarlet fever developed itself whilst she was in the hospital.

On March 25th she had sore throat, and flagged. On the 27th, a doubtful rash was seen. She had no desquamation, and the reality of the nature of the disease was considered doubtful till the renal affection presented itself.

On the nineteenth day of the disease, between 6 and 7 a.m., seized with vomiting and shiverings; the evening before she was pretty well. At the time of my visit she was oppressed, drowsy, very pale, and puffy. She complained of much frontal pain, and had lost her appetite. Pulse 176, respiration 48. Wounds about the ilium looking as before. Lungs were healthy. During the morning she perspired very profusely, and at 12 a.m. her trunk was covered with minute sudamina.

On the twentieth day was much improved; complained of no pain anywhere. Rather drowsy; appetite very bad; pulse 116, very weak; wounds looking much the same. Last night, about 7 p.m., complained of much pain in right foot; there was no redness nor swelling, but it was very tender. This morning there is a deep-red blush over the malleoli, and this part is very tender. Appetite very bad.

On the twenty-first she was less puffy, complained of no pain; pulse 132, feeble. Redness over the right ankle increased in intensity and area; the wounds about the crest of the ilium were looking well; appetite was better than yesterday; less drowsy.

Twenty-second.—Passed a restless night; still rather puffy; ankle more swollen, very red, and painful; pulse 108, weak; lungs healthy; heart, first sound at the apex murmurish.

Twenty-third.—Matter pointing over the outer malleolus; appetite a little better; still puffy looking.

Twenty-fourth.—Much the same; first sound of the heart at the apex dull.

Twenty-fifth.—Swelling extending up the leg.

Twenty-sixth, 3 a.m.—Active vomiting came on, greenish colour, accompanied with diarrhoea; lips and tongue rather dry; soft systolic apex murmur; ankle opened.

Twenty-eighth.—Much thin discharge from the wound at the ankle; redness very much less; no sickness, no diarrhoea.

Thirty-first.—Face pasty looking; tongue dry; discharge from ankle less. No pitting of any part of body, except right foot; distinct apex systolic murmur.

Thirty-second.—Decidedly better.

Subsequently active tubercular deposition took place in her lungs.

Urine.	Urea.	Quantity.	Albumen.
5-30, March 27th, to 10 a.m., March 28th	4.779	117	
Fifth day, March 28th and 29th	4.720		Shade.
Sixth	3.877	235	Slight shade.
Seventh	5.057	310	Slight shade.
Eighth	6.020	430	Slight shade.
Ninth	7.59	660	Rather more.
Tenth	7.450	745	Shade.
Eleventh	11.325	1175	Shade.
Twelfth	11.325	1175	Shade.
Thirteenth	8.712	1025	Shade.
Fourteenth	9.776	1185	Shade.
Fifteenth	9.159	915	Shade.
Sixteenth	8.085	735	None.
Eighteenth	10.442	585	None.
Nineteenth	7.995	390	None.
Twentieth.—Smoky, red colour			Abundant.
Twenty-first.—Slightly smoky			Very abundant.
Twenty-second	1.875	110	Fills two thirds of the tube.
Twenty-third	0.510	60	Fills two thirds of the tube.
Twenty-fourth.—Albumen much less		85	
Twenty-fifth.—Urine contains casts and uric acid, and also kidney-epithelium.			
Twenty-sixth.—Albumen much more abundant.			
Twenty-seventh.—Albumen much more abundant.			

On the twenty-second and twenty-third days the albumen was removed by shaking up the urine with animal charcoal.

She was readmitted on the seventy-eighth day; the urine still contained albumen.

Temperature.—On the third day the temperature reached 100 $\frac{1}{2}$; on the fourth, 98 $\frac{3}{4}$; on the fifth, 98 $\frac{3}{4}$; on the sixth, 98 $\frac{3}{4}$; on the seventh, 100 $\frac{1}{2}$; on the eighth, 100 $\frac{1}{2}$; on the ninth, 100 $\frac{1}{2}$; on the sixteenth, 99; on the seventeenth, 97; on the eighteenth, 98 $\frac{3}{4}$; on the nineteenth, 105; on the twentieth, 100; on the twenty-first, 101 $\frac{1}{2}$; on the twenty-second, 103 $\frac{1}{2}$; on the twenty-third, 101 $\frac{1}{2}$; on the twenty-fourth, 102; on the twenty-fifth, 100 $\frac{1}{2}$; on the twenty-sixth, 100 $\frac{1}{2}$; on the twenty-seventh, 101 $\frac{1}{2}$; on the twenty-eighth, 102 $\frac{1}{2}$.

CASE 13.—Charles Ebett, *æt.* 3 years 9 months. The commencement of the disease in this case was not noted, but merely the first appearance of rash. This I have assumed to be the second day.

This was a very slight case.

Temperature.—On the second day of the disease the temperature rose to 101 $\frac{1}{2}$; on the third, 99 $\frac{1}{2}$; on the fourth, 99 $\frac{1}{2}$; on the fifth, 97 $\frac{1}{2}$; on the sixth, 97 $\frac{1}{2}$.

CASE 14.—Matilda Browne. The lowest temperature occurred on the fifth day, the temperature falling from 102 $\frac{1}{2}$ to 100.

CASE 15.—Sarah Hewett, *æt.* 7. She was seized on 7th, shortly after breakfast. The rash appeared on the second day.

The temperature on the third day rose to 101; on the fourth, to 100; on the fifth, to 99.

CASE 16 (see Diagram 4).—Ellen Kipping, *æt.* 11. Seized Monday, September 30th, with vomiting, diarrhoea, anorexia, and complained of sore throat. Rash first noticed on October 1st. It was intensely marked. It had nearly

disappeared from the whole body on the seventh. Desquamation began on the ninth, about the forehead and labia. Tongue was rather furred on the second day, thickly coated on the third; after this it was of the strawberry character to the ninth inclusive; after this no notes were made.

Fauces were vividly red, but not swollen, to the fifth day inclusive, and improved on the eighth.

Conjunctivæ were injected till the seventh day inclusive.

Glands at the angles of the jaw were slightly enlarged; more so in groins.

Urine was tested till the ninth day; on the fifth and sixth there was a slight trace of albumen.

The child was rather delirious during the fourth and fifth nights.

Appetite was very bad till the sixteenth, when it improved somewhat.

This was decidedly a sharp attack. The rash was strongly marked. She was very heavy and prostrate till the fifth day, when she very greatly improved, and then continued to mend.

CASE 17 (see Diagram 5).—Maria Hodge, æt. 2 years 2 months. Scarlet fever, urticaria, chicken-pox. The child had an attack of measles three weeks before her admission into hospital. The attack lasted about a week, and then she appeared quite well again. On July 19th was quite well during the day, but was very restless during the night. Next day the rash appeared, and she was feverish and had no appetite. Rash at the time of admission was slight everywhere, but most marked on the legs. It disappeared from the face, chest, and abdomen on the fifth day, and from the legs and arms on the seventh. In this case the rash on the arms and legs was much localized, and caused hard, spotty elevations, some the size of a small pea; it was in these that the rash remained so long.

Desquamation.—Face, none; trunk, none. Legs, appeared on the folds of thighs on the eighth, and disappeared on the ninth. Appeared on the arms slightly on the ninth, dis-

appeared on the tenth; on the sixteenth there was some on the palms of the hands.

On the seventh day of the disease a wheal of urticaria was noticed; a few more appeared on the eighth and ninth. They consisted of elevations about the size of peas, pale, semi-transparent, hard, and surrounded by a ring of redness an inch in diameter.

On the seventh a vesicle of chicken-pox was seen on the chest, followed by a few others till the twelfth day inclusive. These, instead of drying up and healing, ulcerated and became surrounded by much swelling. The ulcers began to heal on the fifteenth day.

Alæ of nose became excoriated with a thin, sanious discharge on the seventh, continuing much the same till the thirteenth, when it improved gradually to the eighteenth day.

Tongue thickly coated, red on the third day, then cleaned; papillæ were prominent on the anterior part still on the fifteenth; was normal on the ninth.

Fauces, &c., very red and swollen, fourth and fifth days; still more so, and covered with muco-purulent-looking matter, on the sixth to eighth inclusive. They then improved, and were almost normal on the eighteenth.

Appetite improved on the fourth day, and continued good after the fifth.

Glands at the angles of the jaw were not enlarged; on the third day began to enlarge; on the fourth they increased in size, till the enlargement was visible on the eighth; they then decreased, and were almost of natural size on the eighteenth. In groin they were first enlarged on the fifth day, and continued slightly enlarged till the ninth, after which they were of normal size.

Urine was examined daily till the twenty-ninth day; no albumen was at any time discovered.

The child appeared to be in a very delicate state and the lips were very dry on the third, cracked and swollen on the eighth and ninth; they then improved somewhat.

On the fifth day a decided improvement, however, was noticed; on the sixth, seventh, and eighth, was not so well

again; vomited. She continued very poorly and heavy during the eleventh and twelfth. After this the child improved.

CASE 18.—Eliza Cunningham, *æt.* 7; assumed weight, 16 kilogrammes. The patient was in the hospital for epilepsy at the time of her seizure. On April 24th, at 11 a.m., she complained of headache and sore throat; soon after, she vomited. After this she appeared to be in her usual health till 4 p.m., when she again vomited, and on being undressed to be put to bed the rash was detected.

Thus the rash appeared on the first day, and disappeared on the eighth day.

Desquamation began on the ninth day and had ceased everywhere on the twenty-eighth day, except on the hands and feet.

Eyes were much injected with some muco-purulent-looking discharge from the second to the sixth day inclusive; it then declined, and ceased on the eleventh day.

Glands.—Angles of jaw were enlarged from the second to eleventh days inclusive, then the swelling declined, and disappeared between the twenty-fourth and twenty-eighth.

The glands of the neck were also enlarged, hard, and spotty.

Groins.—A little hard on the second day, enlarged on the third; still more, so as to be visible, during the fourth and fifth; then the swelling declined, and ceased between the twenty-fourth and twenty-eighth days.

Numerous ecchymoses noticed under the clavicles and in groins on the fourth and fifth days.

Some miliary vesicles appeared on the fourth and fifth days.

Sudamina appeared on the eighth day, and were still more numerous on the ninth.

The child was delirious during the night of the first to the fourth inclusive.

Tongue was thickly coated with a creamy fur, the papillae being prominent on the second and third day. Well-marked

strawberry on the fourth and fifth; it then improved, and was natural on the seventeenth.

Fauces, &c., were very red; on the second day after this the parts improved, and were almost normal on the sixth, continuing, however, a little red and flabby till the tenth.

Heart and lungs healthy throughout.

Pulse was found to be irregular on the twenty-fourth to twenty-eighth days; no other notes made.

General appearance.—Child was heavy and dull on the second day; lips were dry and excoriated on the third; looked better on the fourth day; was decidedly so on the fifth; sat up in bed and played. Then improved till the ninth, when she looked puffy and was rather drowsy. This continued till the twelfth inclusive. After this she improved, though she long continued to be very pale.

There was no albumen in her urine at any time.

Diet.—April 27th, beef tea 1 pint, milk 1 pint, wine 4 oz. May 4th, pudding diet, wine 3 oz. 11th, fish, wine 2 oz.

Temperature.—On the second day of the disease it reached 105; on the third, 105½; fourth, 104½; fifth, 102½; sixth, 102½; seventh, 103½; eighth, 101½; ninth, 101; tenth, 101; eleventh, 100½; twelfth, 99½; thirteenth, 99½; fourteenth, 99½; fifteenth, 98½. It continued at about 98½ till the twenty-third day, after which it was not taken.

Table showing the daily amount of urea and urinary water.

Day of disease.	Urea.	Urinary water.
	Grammes.	
Third	13.500	450 c.c.
Fourth	14.550	485
Fifth	15.240	635
Sixth	8.972	485
Seventh	8.910	810
Eighth	10.290	980
Ninth	5.898	715
Tenth	5.665	515
Eleventh	5.197	585
Twelfth	6.650	550
Thirteenth	4.567	435

Day of disease.	Urea.	Urinary water.
	Grammes.	
Fourteenth.....	6.825	350 c.c.
Fifteenth.....	7.312	325
Sixteenth.....	6.195	590
Seventeenth.....	8.350	450
Eighteenth.....	Lost	
Nineteenth.....	10.135	280
Twentieth.....	10.360	280
Twenty-first.....	9.350	275
Twenty-second.....	10.767	365
Twenty-third.....	9.687	365
Twenty-fourth.....	10.725	330
Twenty-fifth.....	11.700	325
Twenty-sixth.....	12.240	360
Twenty-seventh.....	9.460	220
Twenty-eighth.....	9.800	245
Twenty-ninth.....	11.812	315
Thirtieth.....	6.982	245
Thirty-first.....	7.020	590
Thirty-second.....	12.410	750

CASE 19 (see Diagram 6, *a* and *b*).—Anne Lynch, æt. 3 years 3 months. On April 28th she had rigors. Lost her appetite, and subsequently was very thirsty.

On April 29th the rash appeared. She never complained of sore throat. The rash disappeared on the tenth day.

Desquamation began on the seventh day; it had mostly ceased on the twenty-fourth, but still continued on the heel on the forty-third day of the disease. It also continued on the palms of the hands till the thirty-sixth day.

Numerous miliaria were observed over the whole body, except the hands, arms, and feet, on the fourth and fifth days.

Glands at the angles of jaw were rather enlarged on the third and fourth days, and greatly so from the fifth to the eighteenth days inclusive, those on the right side being the most so. They decreased in size from the twenty-second, though on that day they were as large as a chestnut. They were still enlarged on the fifty-first day.

The glandula concatenatæ on both sides were enlarged.

In the groin they were rather enlarged from the third to

the twenty-ninth day; after this they decreased, and were normal on the thirty-sixth.

Tongue was red, with prominent papillæ on the fourth. It then improved, and on the eighth and ninth it was pale and smooth. After the sixteenth day it was normal.

Fauces, &c., lividly red; tonsils much enlarged, and covered with exudation; much muco-purulent-looking fluid over the posterior fauces from the third to the ninth day inclusive. They were less red, and some exudation was observed on the posterior surface of the uvula on the eleventh day. The tonsils were so swollen as almost to meet on the thirteenth and fourteenth days. A small ulcer was noticed on the palate from the sixteenth to the eighteenth days inclusive. The swelling diminished in size from the twenty-second, and were almost natural on the twenty-fourth.

Appetite was bad till the fourteenth day inclusive. It was excellent from the eighteenth to the twenty-ninth inclusive. On the thirty-third it was bad again, and continued so on the thirty-fourth and thirty-fifth, after which it improved again.

A discharge from the right ear began on the thirty-sixth, and continued till the forty-third day.

Heart was healthy throughout.

Some sonorous rhonchus heard in the lungs on the fifth day.

There was no pitting of the forehead, sacrum, nor legs, on pressure, at any time.

General appearance.—The child was oppressed till the eighth day. After this she improved, and continued pretty well till the thirty-fourth day, when she became very ill again. She was very drowsy from the thirty-third to the thirty-sixth day inclusive.

Urine was examined daily on the twenty-third for the first time; a distinct cloud of albumen was noticed.

Twenty-fourth.—More abundant.

Twenty-sixth.—Still more abundant.

Twenty-seventh.—After boiling and being allowed to stand, the coagulated albumen filled one third of the tube.

To-day, for the first time examined microscopically, a little blood was found, and some uric-acid crystals.

Twenty-eighth.—Albumen fills one half of the tube.

Twenty-ninth.—Albumen same in amount; urine contained blood; casts waxy and granular.

Thirtieth.—Urine very smoky; albumen about the same; contained blood and casts.

Thirty-first.—Of a red colour, evidently due to blood; albumen the same; contained casts, blood, and uric-acid-crystals.

Thirty-second.—Less smoky; albumen a little less; blood, uric acid, casts.

Thirty-third.—Albumen fills one fourth of the tube. Blood appears to be much more abundant; casts and uric acid.

Thirty-fourth.—Albumen same, judging by the colour and the amount of deposit; the blood much more abundant.

Thirty-fifth.—Same.

Thirty-sixth.—Albumen fell one third of tube; urine still redder; casts, with blood, pus, and kidneys; epithelium imbedded in them, also some free from corpuscles.

Thirty-seventh.—Urine very much better, much less red; deposit of blood much less abundant; albumen occupies about one seventh of the tube; cast and pus-cells.

Thirty-eighth and thirty-ninth.—Ditto.

Forty-first.—Albumen less now, merely smoky, and rather red; albumen fell one twelfth of tube.

Forty-second.—Urine better.

Forty-fourth.—An abundant cloud of albumen; urine still pinkish and smoky.

Forty-sixth.—Not smoky. Rather abundant cloud of albumen.

Forty-seventh.—Much more smoky again.

Forty-eighth.—Blood appears to be much more abundant again. Rather thick cloud of albumen. Microscope—blood-casts and plenty of kidney-epithelium.

Forty-ninth.—Still worse, redder, and brown. Very abundant precipitate of blood. Abundance of urine passed.

Fifty-first.—Still very smoky and red.

Fifty-second.—Still very red, and copious deposit of blood. On boiling did not get a trace of albumen.

Fifty-third.—Ditto.

Fifty-fourth.—Ditto but got a decided cloud of albumen, when urine shaken so as to mix the blood with it.

Fifty-fifth.—Same.

Fifty-sixth.—Decidedly better, scarcely smoky. No deposit of blood.

Fifty-seventh.—Urine still less smoky; amount of albumen greatly increased.

Fifty-eighth.—Not smoky; an abundant cloud of albumen.

Sixty-first.—Not smoky; albumen rather less.

Sixty-third.—Urine abundant; distinct cloud of albumen.

Sixty-fifth.—Again red and smoky, with rather abundant cloud of albumen.

Urine always abundant.

CASE 20.—Louisa White; height, 3 feet 5½ inches; weight, 15·4 kilogrammes. She was in the hospital with a slight attack of chorea, and was discharged on account of the prevalence of diphtheria, but was very soon readmitted with scarlet fever.

She was seized with sore throat on March 12th. The rash appeared on the same day, and on the first day the rash was over the whole body except the feet.

Second day it reached the feet, elsewhere declining. Disappeared from the face on the third day; from the forearm on the fourth; from the legs on the fifth; from the shoulders and all the body, except immediately beneath the clavicles and over the pubes, on the sixth, and on the seventh it had universally disappeared.

Desquamation began on the seventh day on the face and neck; on the eighth day it implicated the entire trunk and arms; on the ninth it began on the palms of the hand; on the fourteenth on the thighs; sixteenth, on the fore legs, and at the same time it was completed everywhere else except on the hands.

Twenty-ninth.—Almost completed on the palms.

Thirty-sixth.—Still desquamating on the soles of the feet.

The conjunctivæ were injected on the first, second, and third days.

Glands (angle of jaw).—They were rather enlarged on the first day; they increased in size till the tenth day; between this and the twenty-sixth they became normal.

Glands in the groins were much enlarged, so as to be visible, and on thirty-first day were still enlarged.

Glands of neck were also enlarged.

Tongue red, with patches of fur, on the first day. From the second to eighth inclusive it was strawberry; on ninth and tenth day pale, and rather dry. Became moist on sixteenth, and then was a little red, and continued so to thirty-sixth, when it was still smooth in the centre.

Fauces red and inflamed on the first day; increased in size, so that the tonsils almost met, till the third. On the fourth the parts were covered with a tenacious-looking membrane. On seventh, besides the above membrane, there was much muco-purulent-looking fluid. On tenth, tonsils were less swollen; after this declined till on the fifteenth its parts were only slightly swollen and red.

No delirium on the first night. Delirious during the second, third, and fourth nights.

Appetite very bad till the ninth day, when it improved and continued good afterwards.

Lips, nose, &c.—First day sordes on lips; this continued, and on the fifth, lips were drier and the alæ of the nose became red and excoriated. After the eighth day these improved somewhat, but nose continued red and excoriated and the lips dry till the twenty-fifth day.

Choreæ movements became worse somewhat on the third, fourth, and fifth days. Much better on sixth, and ceased on the seventh; continued so till the twenty-seventh, when they were slight, and then disappeared again, not returning up to the time of her dismissal. Her movements, however, throughout were awkward.

Child was very ill and did not improve till the tenth day, when a slight improvement was noted, and convalescence after this was very slow till the twenty-eighth day, when a decided improvement was noticed.

No albumen in the urine at any time.

Temperature.—On the tenth day the temperature rose to $103\frac{1}{2}$; on the second, to $103\frac{1}{2}$; on the third, to $102\frac{1}{2}$; on the fourth, to $100\frac{1}{2}$; on the fifth, to $100\frac{1}{2}$; on the sixth, to $101\frac{1}{2}$; on the seventh, to $100\frac{1}{2}$; on the eighth, to $101\frac{1}{2}$; on the ninth, to $102\frac{1}{2}$; on the tenth, to $102\frac{1}{2}$; on the eleventh, to $102\frac{1}{2}$; on the twelfth, to $101\frac{1}{2}$; on the thirteenth, to $99\frac{1}{2}$; on the fourteenth the temperature continued about 99 till the fortieth day.

Table showing the daily amount of urea, chloride of sodium, and urinary water.

Day of Disease.	Urea.	NaCl.	Water.
Second	7.370	0.440	220 c.c.
Third	10.650	0.600	300
Fourth	10.608	1.035	345
Fifth	9.800	0.980	280
Sixth	9.500	1.035	345
Seventh	9.500	1.705	620
Eighth	9.347	1.110	370
Ninth	8.200	1.840	400
Tenth	5.880	1.120	280
Eleventh	7.425	1.520	350
Twelfth		Lost.	
Thirteenth	7.245		345
Fourteenth	7.065	2.363	305
Fifteenth	4.140	1.550	180
Sixteenth		Lost.	
Seventeenth	7.567		322
Eighteenth	5.200		200
Nineteenth	7.227		245
Twentieth	5.565		210
Twenty-first	7.055		415
Twenty-second	7.930		305
Twenty-third	7.287		275
Twenty-fourth		Lost.	
Twenty-fifth	11.100		555
Twenty-sixth	5.865		345
Twenty-seventh	6.410		435
Twenty-eighth	7.827		505
Twenty-ninth	9.067		585
Thirtieth	10.065		435
Thirty-first	8.580		520
Thirty-second	8.100		350
Thirty-third	6.745		415
Thirty-fourth	12.610		485
Thirty-fifth	11.760		480
Thirty-sixth	8.500		500

CASE 21.—Anne Neil. Rash appeared on the 27th April. No rash at any time on the face.

Chest.—Numerous reddened, elevated papillæ, from the second to the fifth day inclusive. After this it declined, and had disappeared on the ninth.

Abdomen.—Same as on chest, except that it commenced to decline on the fifth, and some slight rash still continued on the ninth day.

Legs the same as the abdomen.

Desquamation.—No note was made on the ninth and tenth days, but on the eleventh it was slight on the chin and chest.

Glands at both angles of the jaw were enlarged slightly on the second day; those on the left side continued slightly enlarged till the thirteenth, when they were normal; those on the right side, after the second day, increased greatly in size, and were still so on the thirteenth. Glands down the neck were also greatly enlarged. In groin they were so enlarged as to be visible. The enlargement commenced on the second and declined on the fifth.

Tongue thickly furred on the second; this cleared off, and left the strawberry tongue on the fourth; this continued till the eighth inclusive; the papillæ then became less prominent, but on the fourteenth tongue was still beefy.

Fauces, &c., very red and much swollen, from the second to fourth inclusive; this was still more marked from the fifth to seventh; on the latter day the parts were very much more red and swollen. On the eighth the redness and swelling began to decline, and the parts were covered with muco-purulent-looking matter. On the thirteenth day they were still swollen and red.

Abundance of sudamina were observed on the chest and abdomen on the second day.

Appetite was rather bad on the fourth day; after this it slowly improved till the eighth day, when it was again bad, but improved on the thirteenth.

The child throughout was in a very low condition. Lips were dry from the first; alæ of nose became red and exco-

riated on the sixth, and continued so throughout. Lips were greatly swollen, cracked, and bleeding, on the ninth day; on the fifteenth they were better.

Bowels.—From the fifth to the ninth had three or four liquid motions daily; on the fourteenth day the motions were less frequent.

Child died suddenly on the night of the sixteenth.

Post-mortem.—The lymphatic glands generally of the body were loaded with tubercle, and the small intestines extensively ulcerated. There was old cretified tubercle in the lungs; none of recent origin.

Temperature.—On the second day it rose to $103\frac{1}{2}$; on the third, to $104\frac{1}{2}$; on the fourth, to $103\frac{1}{2}$; on the fifth, to $103\frac{1}{2}$; on the sixth, to $102\frac{1}{2}$; on the seventh, to $102\frac{1}{2}$; on the eighth, to $102\frac{1}{2}$; on the ninth, to 103 ; on the tenth, to $103\frac{1}{2}$; on the eleventh, to 104 ; on the twelfth, to $103\frac{1}{2}$; on the thirteenth, $102\frac{1}{2}$; on the fourteenth, $101\frac{1}{2}$; on the fifteenth, $101\frac{1}{2}$; on the sixteenth, $101\frac{1}{2}$.

CASE 22 (see Diagram 7).—Margaret Peacock, æt. 5½. Seized at 1 a.m., September 9th. Rash appeared on the same day. She first complained of sore throat on September 2nd, but remained in her usual health till the 9th. Rash was universal on the first, and disappeared on the fifth day.

Desquamation began on the chest, abdomen, and arms on the twelfth day; began on the legs on the sixteenth, and on the same day was completed on the chest; ceased on the abdomen and legs between the twentieth and twenty-seventh days, and still continued on the hands on the twenty-seventh.

Alæ of nose in this case were excoriated.

Conjunctivæ injected from the first to third day inclusive.

Lips were dry and covered with sordes from the first to twelfth day inclusive.

Glands at the angles of the jaw were enlarged on the first, and continued so to the ninth. On the tenth and twelfth the swelling had increased greatly, extending on the

face over the parotid gland; on the thirteenth it began to diminish, but continued to be enlarged even on the twenty-seventh. Glands in groin were enlarged on the sixteenth day; after this no note of their condition was made.

Tongue, rather furred on the first; fur cleared off, but left the tongue dry and brown, on the third; remained so and was cracked till the eleventh day inclusive; on the thirteenth it was moister; on the sixteenth was clean, continuing so afterwards.

Fauces, &c., red and covered with mucus on the first and third; were still redder on the fourth. Redness was less, but swelling more, on the fifth to ninth inclusive; the parts then improved, and were normal on the sixteenth.

Body covered with sudamina on the ninth. Appetite improved on the ninth day.

Lungs healthy throughout.

Heart was healthy on the ninth. No note was made after this till the eleventh, when a distinct murmur was caught, audible at base and apex, most so at the base. It became much less distinct at the apex on the twelfth; the murmur continued throughout.

Never any albumen in her urine.

Child continued very poorly till the ninth day, when she improved, and continued from this time to mend.

CASE 23.—Sophia Pattenson, *æt.* 5½. On September 12th, after tea, she was seized with vomiting and pain in the head. On September 13th the rash appeared; was universal. According to the mother, it declined on the 15th September (fourth day). It disappeared from the whole body on the seventh.

Desquamation began on the face on the seventh; on the chest and abdomen on the eighth; on the legs between the thirteenth and seventeenth days; on the palms on the tenth; was very active on the chest, abdomen, and the arms, on the seventeenth, and was completed on the twenty-first; still continues on the legs and hands on the twenty-fourth day.

Glands at the angles of the jaw were normal till the ninth day, when they became swollen and very painful on the left

side; they decreased in size from the eleventh, but still remained much enlarged on the seventeenth day. Glands in groin were enlarged on the fifth day, and resumed their normal size between the thirteenth and twenty-first days.

Tongue clean throughout.

Throat normal throughout.

Appetite was middling on the sixth day, and improved after this.

Lungs always healthy.

Heart.—A distinct systolic murmur, audible at base and apex, on the eighth; ceased at the apex but continued audible at the base, and was inaudible at either cartilage from the ninth to the thirteenth inclusive. No murmur was audible on the seventeenth nor twenty-first day. No notes were taken in the intermediate days.

No albumen in the urine at any time.

The child was cheerful on the fifth; on the sixth became heavy, drowsy, and fretful, passed a restless night on the eighth. After the ninth day she improved again rapidly.

Temperature.—On the fifth day it reached 100; on the sixth, 99½; seventh, 99½; eighth, 104½; ninth, 103½; tenth, 102½; eleventh, 102½; twelfth, 102½; thirteenth, 100; fourteenth, 98½; fifteenth, 98½. It remained at about 98 till the twenty-fifth day.

CASE 24.—Mary Hugden, *æt.* 11; weight, 25.5 kilograms. Patient was seized with sore throat on November 23rd; rash appeared on the 24th. Rash was well out and universal till the seventh day inclusive; then declined, and, on the tenth, there was a mere trace.

Desquamation began on the face on the seventh; began on the hands on the ninth; on the twelfth it was universal.

Tongue was dry and glazed on the seventh; moister, eighth and ninth.

Sweated profusely on the night of the eighth and ninth.

Appetite improved on the tenth.

Observations were then discontinued till January 22nd (sixty-first day). During this interval she had much blood

and albumen in the urine. On the sixty-first day she was very pale, scarcely any anasarca.

A small amount of albumen occurred in the urine from the sixty-third to sixty-eighth days inclusive; after this the urine was not examined.

The temperature in this case was not taken after the twelfth day; from the fifth to the twelfth days inclusive the temperature varied between 103° and 101° .

Table giving the daily amount of urea, NaCl, and urinary water.

Day of Disease.	Urea.	NaCl.	Water.
	Grammes.		
Sixth	17.550	3.565	1486 c.c.
Seventh	18.487	4.006	2175
Eighth	15.200	2.968	2375
Ninth	13.456	2.220	1859
Tenth	14.437	1.788	1875
Eleventh	14.265	1.902	1585
Twelfth	15.096	2.294	1480
Thirteenth	10.400	0.357	650
Sixty-third	6.570		595
Sixty-fourth	6.864	3.630	660
Sixty-fifth	9.086	4.543	770
Sixty-sixth	6.420		600
Sixty-seventh	5.050	2.157	370
Sixty-eighth	7.155	3.445	530

CASE 25.—Emma Walsham, height, 4 ft. 1½ in.; weight, 23.6 kilogrammes. Sore throat noticed on April 3rd; rash appeared on the third day. She came under notice on the sixteenth day of the disease. On that day there was copious desquamation over the neck; on the eighteenth day it involved the chest, abdomen, and thighs; on the twenty-first it began on the palms of the hands; on the twenty-eighth it had ceased on the arms; on the thirty-second it was completed on the chest; also on the abdomen on the thirty-sixth. On the thirty-ninth desquamation only

observed on the feet, and here it was still continued on the forty-fourth day.

Tongue was pale, and some of the papillae rather prominent when admitted.

The glands at the angles of the jaw reached their normal size between the twenty-ninth and thirty-ninth days. On the forty-fourth they were still shotty in the groins. *

Heart and lungs healthy throughout.

On the twenty-seventh day the child was noticed to be puffy about the face (she had been up for several days); twenty-eighth, kept in bed; puffy about eye, and continued so.

On the thirty-first day was very feverish, and, during the night, she was delirious.

Urine.—Seventeenth day.—No albumen.

Eighteenth.—A shade.

Nineteenth to twenty-sixth inclusive.—No albumen.

Twenty-seventh.—A shade.

Twenty-eighth.—More distinct; no blood.

Twenty-ninth.—Ditto, ditto.

Thirtieth.—Distinct cloud of albumen. Some blood detected by microscope.

Thirty-first and thirty-second.—No albumen. Some blood detected.

Thirty-fourth.—Smoky, but scarcely a trace of albumen.

Thirty-fifth.—Slightest cloud of albumen. Blood less.

Thirty-sixth.—No albumen. Blood less.

Thirty-seventh.—Slight trace of albumen. Some blood.

Thirty-ninth to forty-first inclusive. No albumen, but blood-discs seen.

From forty-second day to time of dismissal, no blood nor albumen.

Temperature.—The patient was admitted on the sixteenth day of the disease; from this date till the twenty-seventh day inclusive, it remained normal. On the twenty-eighth, it rose to 100° ; on the twenty-ninth, to $104\frac{1}{2}^{\circ}$; on the thirtieth, to $98\frac{1}{2}^{\circ}$; and, on the thirty-first, to $99\frac{1}{2}^{\circ}$. After this it continued between 98° and 99° till the forty-sixth day.

Table giving the daily amount of urea and urinary water.

Day of Disease.	Urea.	Water.
	Grammes.	
Seventeenth	7.875	750 cc.
Eighteenth	15.360	960
Nineteenth	Lost.	---
Twentieth	13.322	730
Twenty-first	12.320	880
Twenty-second	11.000	500
Twenty-third	12.247	710
Twenty-fourth	11.870	625
Twenty-fifth	14.550	430
Twenty-sixth	13.120	410
Twenty-seventh	12.905	445
Twenty-eighth	12.200	610
Twenty-ninth	11.560	680
Thirtieth	11.505	885
Thirty-first	13.037	745
Thirty-second	9.955	610
Thirty-third	11.407	585
Thirty-fourth	12.800	800
Thirty-fifth	10.406	925
Thirty-sixth	14.899	908
Thirty-seventh	13.680	1140
Thirty-eighth	12.325	850
Thirty-ninth	14.700	700
Fortieth	15.505	585
Forty-first	15.502	385
Forty-second	16.065	595
Forty-third	17.381	675
Forty-fourth	16.500	600
Forty-fifth	13.870	730

CASE 26 (see Diagram 8).—Mary Anne Dall, *set.* 2 years 2 months, was first seized at 5 p.m. of Sept. 11th. Rash appeared on the afternoon of Sept. 12th.

At the time of admission there was a slight rash over the whole body, papilliform; this left the chest on the eighth day; abdomen, leg, and arms, on the seventh.

Glands at angles of jaws and groins, were slightly enlarged, till the tenth day inclusive.

The throat was red and swollen on the sixth, after this it improved, and was much better on the tenth.

Tongue was furred on the seventh, clean on the eighth day.

Heart and lungs were always healthy.
No albumen in the urine at any time.

CASE 27.—Mary Wright, urine never contained any albumen. Child died on the fourth day of the disease.

Temperature.—On the second day, it rose to $104\frac{1}{2}$; on the third, $104\frac{1}{2}$; on the fourth, $104\frac{1}{2}$.

CASE 28.—Mary Clarke, *set.* 5 years 3 months. July 9th, in the morning the child was chilly, in the evening she complained of headache.

July 11th, rash first noticed.

Rash was strongly out over the whole body, till the sixth day inclusive, then diminished, and disappeared from the legs and arms on the eighth, and on the same day it was very slight on the rest of the body.

Miliary vesicles appeared on the fourth day, increased in quantity till the sixth inclusive, occurring on neck, chest, abdomen, and thighs. All trace of them was gone on the seventh.

Child vomited several times a grass-green coloured matter on the fourth and fifth, none on the sixth.

There was no albumen in the urine at any time. Was first delirious on the night of the fifth, this continued nightly till time of her death. She had a very putrid smell on the eighth day. Died on the eighth day.

The temperature throughout varied between 103° and 105° .

CASE 29 (see Diagram 9).—James Kelly, *set.* 9 years 4 months, admitted Feb. 2nd; ill first, Jan. 27th.

Feb. 3rd.—Slept excellently; pulse much better, tongue still furred; puffy about face; legs pit slightly, very distinct cloud of albumen in urine removed by two grammes of animal charcoal. To-day he had a muco-purulent discharge from nose.

4th.—Hot air bath $\frac{1}{2}$ hour, perspired freely, albumen in

urine same, uric acid, various shapes, globular, lozenge, some casts.

5th.—Much better; warm bath five minutes.

6th.—Wishes to get up, urine uric, no casts, albumen.

7th.—Not so well, appetite less, legs pit on pressure, very puffy about face, much more albumen in urine; hot air bath 6 p.m., perspired greatly.

8th.—Says he feels better; tongue thickly furred, very puffy about face and eyes, quantity of albumen in urine much increased; hot air bath 6 p.m., *in as usual*, half an hour; perspired freely.

9th.—Decidedly less albumen in urine, more puffy about face.

10th.—Face and legs certainly much more oedematous, nothing so well to-day; drowsy.

12th.—Very sick; 2-30 foamed at mouth, groaned, face distorted into numerous grimaces, passed his feces and urine, and had twitching of arms. Fit lasted quarter of an hour; he never became conscious, fit returned in twenty minutes.

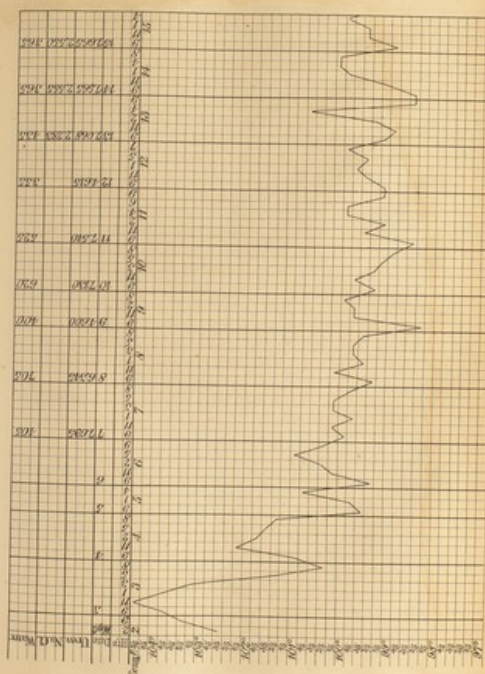
5 p.m. Unconscious; eyes open, foaming at mouth, groans occasionally; hot air bath, and mustard poultice over his loins.

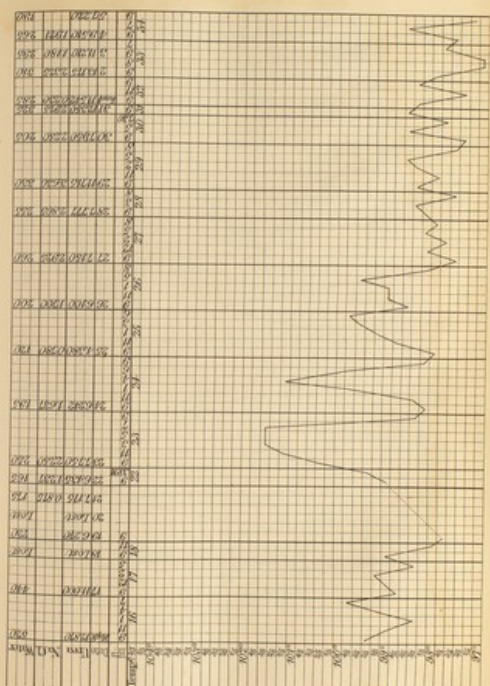
5-20. Another fit; fit continued till 8 p.m.; he died at 12-20 p.m.

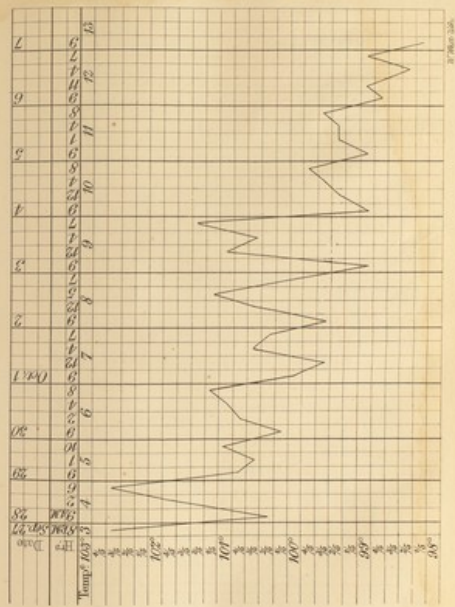
No 14

Case 1. Henry Maylin.

p. 40







Case 6, Thomas Stephens.

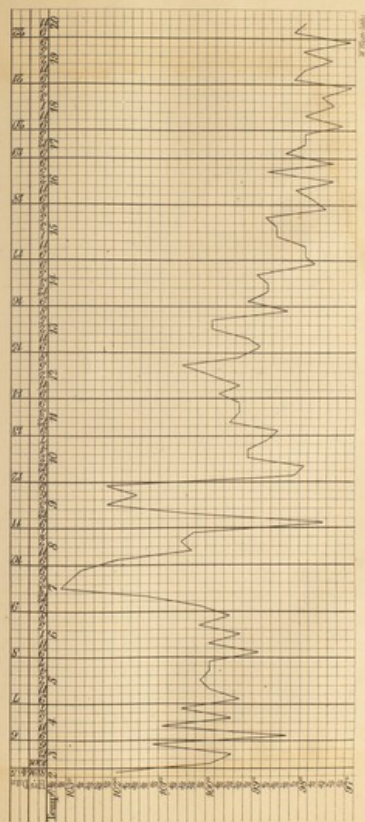
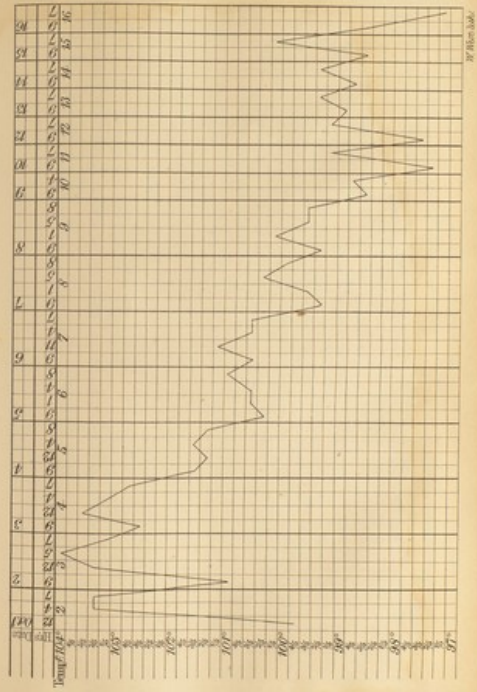


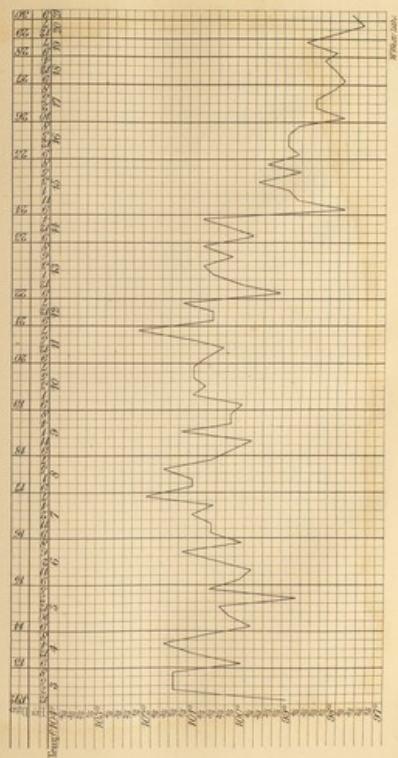
Fig. 4.

Case 46. Ellen Karping.



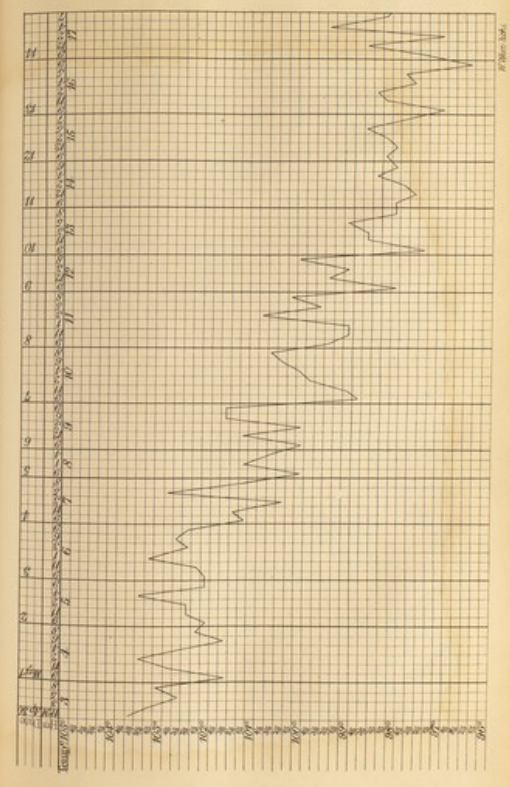
No. 5.

Case 17. Maria Dodge.



N° 6^a

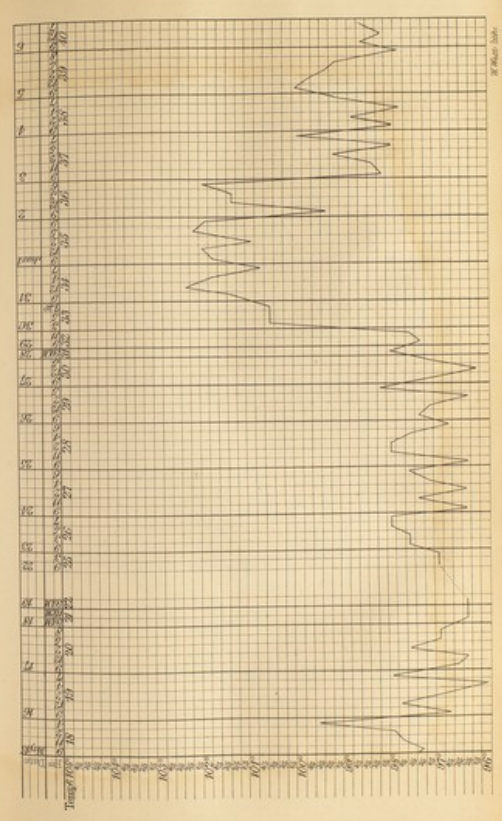
Cas. II. Anne Leardi.



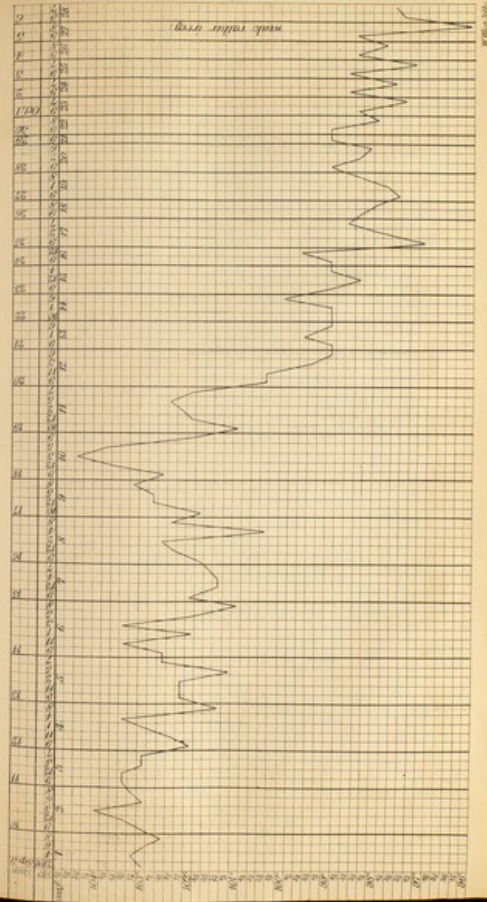
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Anne Leach, Contd.

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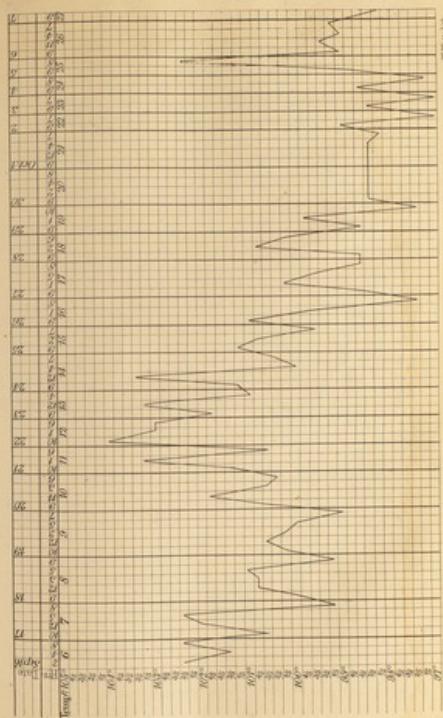


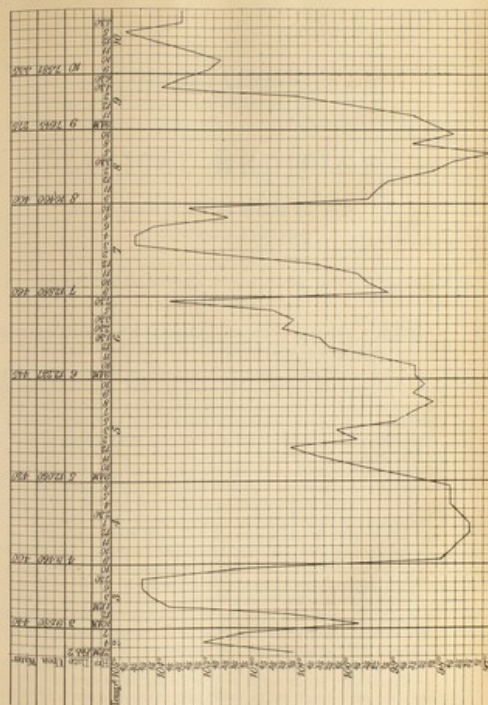
Geo 22. Margaret Peacock. 21.5%



1870

Case 24. Mary Ann Dally





OBSERVATIONS
IN
CLINICAL MEDICINE.

By J. WARBURTON BEGBIE, M.D.,
PHYSICIAN TO THE ROYAL INFIRMARY.

Reprinted from the Edinburgh Medical Journal, October 1862.

*Malignant Disease of the Oesophagus succeeded by sudden Pericarditis,
and ultimately by Pneumo-Pericardium with effusion.*

SYSTEMATIC writers on diseases of the heart have, for the most part, acknowledged three different ways in which an accumulation of air in the inflamed pericardial sac may be determined. 1st, Gas may be the direct product of the irritated membrane itself. It is admitted that occasionally air is produced in the cavities of the pleura and peritoneum when these are the seat of inflammatory action, and if so, there can be no reason why the same formation, or pneumatosis should not occur within the pericardium. Dr Stokes has recorded a case of this nature,¹ to which, in connexion with a brief discussion of the physical signs of pneumo-pericardium, I shall again allude.

2d, Gas may result from the decomposition of fluid within the pericardium. Laënnec² and others have not only pointed out the physical signs which indicate this lesion, but—the former more particularly—have in all probability greatly exaggerated the frequency of its occurrence. The effusion of air and fluid into the pericardium was, in the opinion of Laënnec, a phenomenon likely to occur in the last stage of all diseases, and its existence he was wont to determine both by percussion and auscultation. “L'épanchement liquide et aëroforme à la fois du péricarde peut avoir lieu dans l'agonie de toutes les maladies. Il m'est arrivé quelquefois de l'annoncer à une résonance plus claire du bas du sternum, survenue depuis peu de jours, ou à un bruit de fluctuation déterminé par les

¹ Diseases of the Heart and Aorta, page 21.

² Traité de l'Auscultation Médiate.—Du Pneumo-Péricarde.

battements du cœur et par les inspirations fortes." In a case recorded by M. Bicheteau,—to which reference will be found in Bouillaud's work (*Traité des Maladies du Cœur*), as well as in a note by Andral to his edition of Laënnec's treatise, and which is also noticed by Dr Stokes and Dr Walshe,—the diagnosis of air as well as fluid existing in the pericardium was made during the life of the patient, chiefly from the presence of a peculiar sound with the heart's action, compared by Bicheteau to that produced by a water-wheel (*l'eau agitée par la roue d'un moulin*), while on examination after death the pericardium was found occupied by a purulent fluid of very fetid character, air escaping with a whistling sound when the sac was opened.

3d. Gas may reach the pericardium from a distance through perforation, and this again may be the result of direct injury or of disease. Further, in such circumstances the source of the air may be various. A remarkable case is mentioned by Dr Walshe in which a communication was established between the œsophagus and pericardium, in an attempt to swallow a long blunt instrument, a juggler's knife—the case terminated fatally. The physical signs in this instance, to which I shall refer, were of great interest and clearly established by Dr Walshe.¹ A case of traumatic pneumo-pericardium unattended by inflammation and resulting in complete recovery, is given by Dr Flint, to whom it was related by Dr Knapp of Louisville. The patient was stabbed with a knife which penetrated the pleural cavity and perforated slightly the pericardium.² After the operation of paracentesis pericardii and injection of iodine, physical signs precisely similar to those met with in traumatic cases have been discovered. Such resulted in the memorable instance recorded by M. Aran, under the title, "*Pericardite avec épanchement, traitée avec succès par la ponction et l'injection iodée.*"³ Of communication established between the pericardium and neighbouring organs through the progress of disease, and permitting the entrance of air into the cavity of the former, cases have been already recorded by different writers. Dr Graves, in his *Clinical Medicine*, has furnished a remarkable example of communication by fistulous opening between the stomach and an hepatic abscess on the one hand and the pericardium on the other. Dr McDowell exhibited to the Pathological Society of Dublin, the morbid appearances in a case of communication established between a cavity in the left lung and the pericardium.⁴

When the close anatomical relationship of the œsophagus to the pericardium—the former lying in the posterior mediastinum in contiguity with the posterior portion of the pericardium for nearly

¹ Diseases of the Heart, pages 46 and 271.

² Flint on Diseases of the Heart, page 357.

³ Bulletin de l'Académie de Médecine, Séance du 6 Novembre 1855. See also Trousseau, Clinique Médicale de l'Hôtel Dieu de Paris, vol. I, p. 720.

⁴ For both cases, see also Dr Stokes' work, pages 23, 25.

two inches—is held in remembrance, it will be seen how, in their conditions of disease, likewise, the one is very apt to influence the other. The pressure exerted on the œsophagus by a distended pericardium, may unquestionably determine dysphagia, a symptom of pericarditis, which, though recognised by Testa, has only been duly insisted upon by Dr Stokes, by whom, however, it is regarded as less a mechanical than a vital effect of pericarditis. The case I am now to record is one which illustrates the intimate connexion to which reference has been made,—disease of a cancerous nature, primarily affecting the œsophagus, subsequently involved adjacent organs, in particular giving rise to pericarditis with effusion, and ultimately, by perforation to pneumo-pericardium.

CASE.—Mrs W., æt. 43, mother of seven children, admitted to Ward XIII., 29th July 1862. She had for several months previously been under the care of Dr Hislop of North Berwick, from whom, at the time of her admission, I received the following brief account:—"She had been suffering from increasing difficulty in swallowing, at first considered to arise from spasm in the muscles of the œsophagus,—an opinion which was strengthened by the relief she experienced after passing the probang on several occasions. A month or more ago, in attempting to pass the probang much greater difficulty was experienced, and its use was finally desisted from. She suffered much about the same time from vomiting, and once brought up some blood with mucus." Dr Hislop added, "From the pain she feels in the back, the increasing difficulty in deglutition, and the general features of the case, I fear that the morbid deposit is of a malignant character. I have for some time been doing nothing but supporting the system."

State on Admission.—Patient presents an anxious expression of countenance, is very anæmic, without history of hæmorrhage or renal disease. No albuminuria. As far as can be determined the only cause for her present condition is defective alimentation, on account of dysphagia, which coming on gradually, has existed more or less for nearly two years. She has almost constant vomiting, or rather there occurs immediate rejection of the food before it has reached the stomach. Has little or no pain. On careful examination of the chest, no abnormal indication is furnished either by the lungs or heart. Abdominal organs apparently free from disease.

From the time of admission the opinion gradually gained weight, that the patient laboured under malignant disease of the lower portion of the œsophagus.

22d August.—Under a careful regulation of diet some improvement has resulted. The dysphagia and vomiting have greatly abated. *Vespere.*—Has this evening complaint of headache and pain in the chest.

23d.—After the application of a sinapism the pain in the chest was relieved. On auscultation, a distinct to-and-fro pericardial friction sound is audible over the region of the heart. There is no increase of precordial dulness. In the evening the patient fainted, losing consciousness for a very brief period; but on her recovery from the swoon, remaining cold and collapsed in appearance, with almost imperceptible pulse. Brandy was administered, and warmth applied externally.

24th.—Remained very much sunk during the whole night; the surface of body covered with clammy moisture; at times becoming almost pulseless; when perceptible, the pulsations at wrist numbered 120. Brandy and aromatic spirit of ammonia were given freely. She is now—*Now*—a little stronger, free from pain and without dyspnoea. The friction sound over the heart has lost nothing of its distinctness.

¹ Report subsequent to patient's entering the Hospital, furnished by Dr James Grant.

25th.—Has continued in much the same state. The attrition sound with the heart is not quite so distinct, and now there exists a little increase of dullness on percussion, with appearance of slight fulness in the fourth and fifth left intercostal spaces near the sternum.

26th.—More sunk in appearance. Physical signs have undergone no change.

27th and 28th.—In much the same state.

29th.—On auscultation to-day at visit, a very remarkable character of the heart's sounds was noticed. The friction is replaced by a gurgling noise, a *churning splash*, audible over the whole cardiac region, and rendered more distinct when, for an instant, the patient holds her breath. This sound is not distinguishable at a distance from the chest. The dullness on percussion over the heart has vanished, and now a clear and nearly tympanic note prevails, with increased fulness in precordial region. The patient's extreme weakness forbids any attempt to alter her position in bed; the effect of change of posture on the percussion note cannot therefore be determined.

30th.—Physical signs remain as yesterday.

31st.—Patient died at 9 A.M.

In endeavouring to explain the remarkable physical phenomena connected with the heart, which presented themselves during the closing days of this poor woman's life, I considered it probable that the pericarditis, of which, on the 23d August, the signs were perfectly distinct, was determined by the progress of the cancerous affection of the oesophagus to the posterior wall of the pericardium; and when, on the 29th, the friction sound over the heart was replaced by the gurgling r  le, limited to the cardiac region, and altogether unlike any sound connected with the heart's action previously familiar to me; and when, in addition to the evidence thus afforded, there had occurred an unmistakable alteration in the percussion note over the heart, dullness having yielded to clearness, I concluded that perforation of the oesophagus had taken place, and that, besides the presence of lymph and fluid in the pericardial sac, there was also air. The diagnosis then formed and expressed was as follows:—Cancer, affecting the lower portion of the oesophagus where in contact with the pericardium; pericarditis with effusion from extension of disease in the former; finally, rupture of the oesophagus and passage of gas into the pericardium. The *post-mortem* examination, conducted on 1st September by Dr Haldane, determined the correctness of this opinion in all essential particulars. I subjoin Dr Haldane's report.

"The body was much emaciated: the surface very pale.

"When the chest was opened, the pericardium, marked by the pressure of the ribs, bulged forwards, and on being punctured air escaped. There were no adhesions of the pericardium, but in its cavity were about three ounces of a dark-brown fetid fluid. Both surfaces of the serous membrane were coated with lymph of a yellowish grey colour, of leathery appearance, and evidently of some standing; there was also some softer and more recent lymph, which could be readily scraped off with the nail. When the heart, which was of natural size and structure, was removed, an irregularly circular opening admitting the point of the finger, and communicating with the oesophagus, was found in the posterior wall of the pericardium. On examining the oesophagus, its upper part was found healthy, but the whole of the lower part from about the middle of the thoracic portion was in a cancerous condition; about two inches and a half of its anterior wall was completely gone, and its cavity was

here bounded by the back of the pericardium and by the inner margin of each lung. It was here that the pericardium was perforated, and the pleura covering the lungs in this situation was dull and of a brownish colour, but the lungs were not opened into.

"While the liver was being removed, it was found that the back of its left lobe was adherent to the anterior wall of the stomach in a space about the size of half-a-crown. On separating the adhesions, an opening with sloughy margins was found in the stomach, but the firm connexion with the liver had prevented communication with the peritoneum. The whole of the lower part of the oesophagus, the cardiac extremity of the stomach, and the adjoining portion of its anterior wall were cancerous; the cancer was soft and fungating, and in several situations was in a sloughy condition. The intestines were contracted. There was no other lesion."

I conclude with a very few remarks on the physical signs of Pneumo-hydro-pericarditis. La  nec, who, as already observed, probably exaggerated the frequency of the occurrence of gas in the pericardial sac, speaks of three signs upon which dependence is to be placed in the diagnosis of air and fluid in the pericardium. 1. Unusual resonance over the lower part of the sternum. 2. Fluctuation sound (*bruit de fluctuation*) audible with the action of the heart and on deep inspiration. 3. As specially relating to the diagnosis of pneumo-pericardium, the circumstance of the heart's sounds being heard at a distance from the chest. Upon this sign La  nec placed very considerable reliance. He states, indeed, that his observations respecting it were made some time after those already referred to as *one*, and *two*, and that he had not been able to determine whether it existed in connexion with these. Dr Stokes, whose observations on pneumo-pericarditis are most instructive, noticed the fact of the heart's sounds being heard at a distance in the case which he has recorded. He remarks, however, that this sign was not present in either Dr Graves' or Dr McDowell's cases already noticed. I have mentioned that it did not occur in the instance now recorded, and Dr Walshe has no doubt correctly observed that La  nec's expressed conviction, that in almost all cases (for he uses the expression *presque tous les cas*, and not simply *occasionally*) when the heart's action is heard at a distance from the body, the cause of the phenomenon is a temporary development of gas in the pericardium (often readily absorbed, and whose presence does not give rise to any serious result), cannot at the present day be received.¹ In the remarkable case of pneumo-pericarditis related by Dr Stokes, the following signs were observed. I give them in Dr Stokes' own language. "On examination a series of sounds was observable which I had never before met with. It is difficult or impossible to convey in words any idea of the extraordinary phenomena thus presented. They were not the rasping sounds of indurated lymph or the leather creak of Collin, nor those proceeding from pericarditis with valvular murmurs, but a mixture of the various attrition murmurs with a large crepitating and a gurgling sound, while to all these phenomena was added a

¹ Diseases of the Heart, p. 269.

distinct metallic character. In the whole of my experience I never met so extraordinary a combination of sounds. The stomach was not distended by air, and the lung and pleura were unaffected, but the region of the heart gave a tympanic *bruit de pot fêlé* on percussion, and I could form no conclusion but that the pericardium contained air in addition to an effusion of serum and coagulable lymph.¹ The phenomena on auscultation and percussion thus recorded will receive farther value as indicating the existence of hydro-pneumo-pericarditis, if in addition there be noticed, as was done by Dr Walshe in the "singular case of traumatic communication between the œsophagus and pericardium," referred to in his work on Diseases of the Heart,² a dull or tympanic sound elicited over the precordial region according to the position assumed by the patient. The extreme weakness of the patient in the instance I have recorded alone prevented our determination of the existence of this important sign: from the appearances presented after death, I have little doubt that, had it been in our power to alter the patient's position after the development of the peculiar auscultatory phenomena, we should have had this last indication also to guide us. Without it, however, and in default of a metallic character of the cardiac sounds, as noticed by Dr Stokes, the diagnosis of pneumo-pericarditis with effusion may I think be made, from observing a gurgling or churning splash sound with the heart's action limited to the cardiac region, with which more or less of tympanic precordial resonance is associated. Still more reliable as signs will these phenomena be, if, as in the instance now recorded, the gurgling has succeeded, after its continuance for a few days, a distinct friction sound, and the tympanic replaced a dull percussion note.

¹ Diseases of the Heart, p. 22.

² P. 46.

Reprinted from the *Journal of the Chemical Society*.

Oct 1862

On the Chemistry of Digestion.

[A Discourse delivered to the Fellows of the Chemical Society.]

By W. MARCET, M.D., F.R.S.,

Assistant Physician to the Westminster Hospital, etc.

By the chemical phenomena of digestion, we mean those chemical changes which food undergoes in the stomach and intestines, the object of which is to transform it into certain soluble substances on the one hand, and into emulsions on the other; for under those conditions only, can the food taken be absorbed into the blood-vessels and lacteals, and thereby effect the purpose of nutrition. If from some circumstance connected with the nature of the ingesta, or a vitiated state of the gastric and intestinal secretions, the metamorphosis in question does not take place, what has been eaten will be voided, more or less unaltered,—a fact of importance in a medical point of view.

My attention has been directed for many years to the subject of the present discourse, and I avail myself, with pleasure, of this opportunity of recording the valuable aid of my former assistant, Dr. Frederick Dupré, in a considerable portion of these inquiries.

The stomach, during the period of fasting, contains a small quantity of mucus, exhibiting a slight, though decided, alkaline reaction; this can be easily ascertained by testing the few drops of theropy and frothy fluid obtained from the stomach of a dog having a gastric fistula, or external opening in that organ. As soon as solid food is taken, the stomach secretes in abundance a strongly acid fluid, the gastric juice, and the process of digestion commences. The action of the gastric juice on the contents of the stomach is generally considered as due to its acid constituent, and to a substance called *pepsin*, which it contains in very small quantity. It should be remembered, however, that every one of the constituents of gastric juice must have a share in its action on food; just in the same way as certain therapeutical agents, such as mineral waters, are possessed of medicinal properties, partly on account of a special substance they contain, and partly from the united influence of the whole of their constituents. Owing to this circumstance,

it is important when experimenting on gastric juice, to use, as far as possible, the secretion obtained from an animal, such as the dog, and avoid conducting the inquiry with a digestive fluid prepared artificially by mixing together a dilute acid and pepsin. I invariably used in my researches the gastric juice of ~~some~~ dogs, extracted from each animal through a gastric fistula with which it had been provided. The secretion was excited with hard-boiled cartilage, or soft bones. By this means I obtained, from two dogs, about a gallon of gastric juice; and a much larger amount could have been extracted within the same time, had it been required.

In order to determine the composition of pure gastric juice, it would be necessary to excite its secretion by means of hard substances, perfectly insoluble in that fluid: unfortunately, under those circumstances, as for instance, when dogs are made to swallow pebbles, the quantity of gastric juice secreted is but very small, and barely sufficient for a chemical inquiry. It must be remembered that hard animal tissues, such as cartilage, or even bone, are rapidly acted upon by gastric juice, so much so, that the fluid obtained from the stomach after the ingestion of these substances cannot be regarded as the pure secretion.

The acidity of gastric juice may be considered as one of its characteristic features. It is remarkable that chemists and physiologists do not yet agree as to the nature of this free acid, although, from the investigations of Bidder and Schmidt, Dr. Prout's opinion on the subject now prevails, and the acid substance is believed to be hydrochloric acid; the experiment which Bidder and Schmidt undertook in order to resolve the question, was as follows:—

"About 100 grammes of gastric juice were used, to which nitric acid and nitrate of silver were added; the amount of chloride of silver obtained was determined, which yielded the weight of the whole of the hydrochloric acid present. After the excess of nitrate of silver had been removed from the gastric juice by precipitation with hydrochloric acid, the fluid was evaporated to dryness, carefully incinerated, and the weight of the bases determined; the quantity of hydrochloric acid necessary to neutralize these bases was then calculated." By so doing, Messrs. Bidder and Schmidt obtained, on the one hand, the weight of the whole of the hydrochloric present in gastric juice, and on the other, the weight of the hydrochloric acid necessary to neutralize the bases; and as they found that the total amount of hydrochloric acid present was

greater than was necessary to neutralize the bases, they concluded that gastric juice contained some free hydrochloric acid. In order to decide whether there was any free acid in gastric juice besides hydrochloric acid, these enquirers determined the acidity of gastric juice with an alkaline normal solution, and they found the amount of the base necessary for the neutralization of this free acid to be exactly, or nearly equivalent to the quantity of free hydrochloric present, which had been ascertained in the first part of the experiment; they therefore concluded that the whole of the free acid of gastric juice is hydrochloric acid.

Notwithstanding the apparent correctness of this method of investigation, the gastric juice of a dog on which I have experimented, has appeared to me, for the following reason, to contain more or less of another acid besides hydrochloric acid. I happened to place some gastric juice in Graham's dialyser, which was made to float on distilled water. After a time, the secretion contained in the dialyser had lost certain of its immediate principles, which had passed into the distilled water. At first the acid gastric juice gave an abundant precipitate with nitrate of silver, but the bulk of the precipitate gradually diminished, the hydrochloric acid passing into the distilled water; and, finally, after coagulating the albumin by heat, and filtering, the addition of the reagent failed to give even a trace of a precipitate, thereby showing that the hydrochloric acid had entirely found its way through the membrane. On testing the fluid in the dialyser at that period of the experiment, it exhibited a distinctly acid reaction; consequently the gastric juice was still acid, although it did not contain a trace of hydrochloric acid, showing that another acid was present besides the hydrochloric acid.

The acidity of gastric juice, according to Bidder and Schmidt, ranges from 0.1708 to 0.3353 % of hydrochloric acid. I have had frequent opportunities of testing the degree of acidity of this secretion, and have found it to correspond to between 0.085 and 0.303 % of hydrochloric acid; these numbers, being rather lower than those of Bidder and Schmidt. While enquiring into the acidity of gastric juice, I observed that the degree of this re-action varied according to the period after the ingestion of food, at which the fluid was collected. The first quantity removed from the stomach was the most abundant, but the least acid; gradually less and less of the secretion could be obtained, but it became more and more acid; while the last quantities

of the gastric juice which were extracted exhibited the maximum degree of acidity. The following is a table showing the quantities of gastric juice extracted from a dog bearing a gastric fistula, at different periods after the animal was fed, and also the degree of acidity of the fluid, expressed as potash, necessary to neutralize 100 CC. of the secretion. In four experiments the acidity invariably increased, so much so that the degree of acidity was considerably greater towards the close of each experiment than it was at the beginning.

Acidity of Gastric Juice.

The time is counted just after the dog took his meal, consisting of boiled wind-pipe. The acidity is expressed in grammes of potash necessary to neutralize 100 CC. of gastric juice. The experiments were made in middle of March, 1860.

No. I.			
Time after food taken.	Quantity.	Acidity.	
30 minutes	2oz.		0.065
40 ^m later	1oz.		0.113
No. II.			
30 minutes	lost.		
30 ^m later	2oz. 2dr.		0.130
10 ^m later	7dr.		0.156
10 ^m later	6dr.		0.169
10 ^m later	5dr.		0.195
No. III.			
30 minutes	7dr.		0.208
30 ^m later	1oz. 6dr.		0.227
30 ^m later	1oz.		0.273
30 ^m later	3dr.		0.377
30 ^m later	1oz. 5dr.		0.390
No. IV.			
39 minutes	2oz. 6dr.		0.180
10 ^m later	1oz. 3dr.		0.190
17 ^m later	2oz. $\frac{1}{2}$ dr.		0.235
15 ^m later	1oz. 4dr.		0.250
8 ^m later	5dr.		not determined.

This gradual increase of the acidity of gastric juice must be due, partly to the first quantities which are secreted being diluted and neutralized by the slightly alkaline mucus contained in the fasting stomach; but more especially, I believe, because the fluid secreted towards the end of digestion, is really more acid than that secreted at the beginning. This last hypothesis appears probable, if it be remembered that the mucus of the stomach, during the period of fasting, is but slightly alkaline, and exists therein but in small quantity. Whatever be the cause of the fact, the gradually increasing acidity of gastric juice, after food has been taken, appears established, and must serve some important purpose in connection with the process of digestion, one of these objects being obviously to add to the power of the stomach of digesting those pieces of food which have not been properly masticated. The outside of each morsel of animal food, which has been masticated, will not require for its digestion the action of strong gastric juice; but the inside of the morsel, especially in the case of animals, which is not so well triturated with the teeth, will require for its digestion the action of a more powerful or more acid gastric juice.

If we consider the average acidity of gastric juice to be due to the presence of 0.253 % of hydrochloric acid; and, if it be admitted, with Bidder and Schmidt, that an adult man, weighing about ten stone, secretes in twenty-four hours about 6400 grammes of gastric juice, then no less than about sixteen grammes (247 grns.) of free hydrochloric acid will be extracted from a human adult's blood, and returned into it in twenty-four hours. This acid must result from the decomposition of chloride of sodium, as was suggested by Prout* in 1834; but he adds "What becomes of the soda from which the muriatic acid has been disunited?" He proceeds to state, as his belief, that the largest part of this soda is probably directed to the liver, and passes off in the bile. We are now aware that such is not the case, and Dr. Bence Jones, by his researches on the acidity of the urine during the period of digestion and fasting, has solved this interesting question.† Blood is always alkaline; it must be so for the maintenance of health.‡ There is, however, in the blood a constant tendency to the forma-

* Chemistry, &c., of Digestion, 1834, p. 500.

† Phil. Trans. 1849.

‡ Dr. Pavy has observed that, after phosphoric acid has been injected into the circulation, the urinary secretion contains sugar.

tion of acid; but, before the blood can turn acid, the excess of this substance is eliminated by the secretions; consequently acids are constantly being secreted from the blood, sometimes in the perspiration, sometimes in the urine, sometimes by the stomach. When the secretion of acid in the urine is active, that of the stomach is quiescent, and *vice versa*; during the secretion of gastric juice, the acidity of the urine is greatly diminished. At the time when digestion is at its height, the acidity of the urine is at its minimum. These are the interesting results of Dr. Benec Jones' inquiries. We may therefore surmise that, during digestion, the chloride of sodium of the blood yields hydrochloric acid to the gastric juice, while the soda is immediately taken up by the acid, which, during fasting would have been excreted with the urine, but which remains in the circulation while digestion is going on.

If chloride of sodium be the source of the principal constituent of the gastric juice—*hydrochloric acid*—this salt must be one of the most important immediate principles of the blood; and we can understand, why, when animals are for a time comparatively deprived of chloride of sodium in their food, they eliminate none of this salt in their excrementitious secretions; and, consequently, in such cases, the same hydrochloric acid serves over and over again for the gastric secretion, while all the other constituents of the body are undergoing the normal process of waste. This is one of those admirable provisions of nature which have fitted animal life to be preserved under so many different circumstances.

The main object of the gastric digestion is the transformation of albuminous food into a substance which Lehmann has called *peptone*, and the conversion of neutral fats into fatty acids. Albumin, fibrin, casein, gelatin, and chondrin are modified in the stomach, and assume new characters, obviously in great measure with a view to their more ready absorption into the blood. Under the influence of the gastric juice, albuminous or protein-compounds lose their coagulability by heat and mineral acids, and likewise their property of forming insoluble combinations with most metallic salts. Lehmann has been unable to discover any difference between the quantities of nitrogen, carbon, oxygen, and sulphur contained in the peptone and in the substance from which it was derived. As to cartilage and gelatinous bodies, they are converted in the stomach into substances which, according to Lehmann, correspond perfectly in their physical, and in most of their chemical properties to the peptones of the protein-bodies. We

gather from the observations of this chemist, that, in his opinion, the various peptones derived from protein and gelatin compounds are sufficiently analogous to be considered as one and the same body. Peptones must be looked upon as modifications of albumin, fibrin, casein, gelatin, and chondrin, approximating to albumin of blood; indeed they are partly assimilated compounds.

Although I have no reason whatever to disbelieve Lehmann's views regarding the analogy existing between the chemical characters of peptones, still they do not appear to agree altogether in their physical properties; and this point is one of much interest. Some peptones, or perhaps only one kind of peptone, acts on polarised light, rotating the plane of polarisation to the left, while others appear to exhibit no such property.

I observed, in the spring of 1860, that gastric juice acts on polarised light; and for the following twelve months I diligently inquired into the various circumstances connected with this phenomenon. Shortly after my results had been communicated to this Society, I found in Henle and Meissner's "Bericht über die Vortschritte der Anatomie und Physiologie," for 1859, that Hoppe had already observed this same optical property of the gastric fluid; and I willingly give up to him the priority of the discovery; at the same time it should be understood that Hoppe merely detected the bare fact, and only expressed an opinion on its cause, based on one experiment. He believed this phenomenon to be due to a substance secreted in the gastric juice, while I have shown that it is owing to a product of the digestion of animal food, or a *peptone*.

There are two different animal tissues, which, when submitted to the action of gastric juice, in or out of the body, yield polarising peptone—these are cartilage and the mucous tissue of boiled intestines, which can be easily separated from the muscular tissue. I could obtain none, or no appreciable quantity of this substance, from pure coagulated albumin or coagulated casein, when digested in gastric juice out of the body. My experiments on the digestion of meat out of the body, have failed to give me any clear results, although dogs fed exclusively on cooked beef-steaks for a week, invariably yielded a gastric fluid which acted on polarised light. We can be sure that the constituent of gastric juice which thus rotates the plane of polarisation of light, is a *peptone* and not a *substance secreted*; for if, after keeping a dog fasting for thirty

hours or longer, and then washing out his stomach thoroughly with water, the animal be made to swallow silicious pebbles, the small quantity of gastric juice secreted has no power of rotating the polarised ray. The result of this experiment is at variance with that obtained by Hoppe, who, after allowing an animal to fast, excited the gastric secretion with a glass rod, without however, taking the precaution of washing the stomach, and found that the fluid acted on polarised light. After exciting the secretion with fragments of bone, I obtained a gastric fluid which exerted but a very slight influence on polarised light, showing only seven divisions to the left in Soleil's saccharometer, while when the secretion was excited by cartilage, the rotation was from thirty to forty divisions, obviously from the digestion of the cartilage; and moreover, gastric juice, mixed with cartilage or with the internal tissue of boiled tripe, and exposed in a water-bath to a temperature equal to that of the body, acquires, after a short time, a power of deviating the plane of polarisation, which may be twice as great as that it possessed before the commencement of the artificial digestion.

After many experiments as to the best method of extracting from the gastric fluid the peptone under consideration, I succeeded in preparing it in a comparatively pure state, and then found it to be quite similar in its chemical properties to Lehmann's peptone. Four experiments were made with the dry substance in order to determine its rotatory power in Soleil's saccharometer. Solutions of the peptone in twenty-five cubic centimetres of water yielded for 1°, or division of the instrument, 0.020, 0.023, 0.027, 0.029 grammes: average 0.024 grm. Therefore, when gastric juice is examined in the saccharometer, for every degree of rotation to the left, the fluid contains 0.024 grm. of polarising peptone, or a proportion closely approximating to that, in twenty-five cubic centimetres.

Besides its office of transforming meat and allied articles of food into soluble substances, the stomach is possessed of another power no less important for the purposes of nutrition. I have discovered that it has the property of converting neutral fats, containing stearin, margarin, and butyrin, into their corresponding fatty acids; this fact, simple as it appears, is the key to the explanation of the digestion of fats. The fats of food are constantly neutral, they are not acidified by the process of cooking,

although heated even for a length of time in contact with the acid juice of flesh, and as fatty acids are found in the stomach, it must be owing to a phenomenon of gastric digestion. If a dog bearing a gastric fistula be made to eat butter, the fluid extracted through the fistula a quarter of an hour or twenty minutes later, or even less, will smell strongly of butyric acid. The fat of beef or mutton, after having remained for a certain time in the stomach, yields to alcohol, although not to water, an acid reaction. When the food taken contains but little fat, the whole of it, perhaps, is acidified during the gastric digestion; in those cases where much fat is ingested, I believe a part of it escapes from the stomach, without having undergone any change. With respect to the olein of food, I had concluded in a paper I communicated to the Royal Society, in 1858, that this substance is not acidified in the stomach, but I do not wish to be too positive regarding this statement.

The importance of the transformation of neutral fats into fatty acids in the stomach will be readily understood, if we take into consideration the action of bile upon these substances, the characteristic physiological property of that secretion being its power of converting fatty acids into an emulsion, while, with regard to neutral fats, it exhibits no such influence: consequently fats undergo in the stomach a certain transformation, by which means the bile becomes capable of acting upon them, and transforming them into an emulsion.

I must now beg to make a few observations on the phenomenon of fatty emulsions.

When neutral fats or fatty acids in a fused condition are agitated with water, no emulsion is formed, but large globules are seen to pervade the fluid; on standing they immediately run into each other, and rise to the surface, the aqueous fluid remaining perfectly clear. When fused *neutral fats* are agitated with a solution of neutral tribasic phosphate of soda, I have observed the same phenomenon to take place as with pure water, but when fused *fatty acids* are treated with a solution of phosphate of soda, an emulsion is formed, or in other words, the globules of fat lose the property of running into each other; they remain separate, the fluid assuming a milky appearance: consequently phosphate of soda does not act equally on neutral and on fatty acids. This subject appearing to me of great interest, I have given it much attention,

and I have found that the emulsion produced by acting on fatty acids with phosphate of soda is invariably accompanied by the formation of a certain quantity of soap; and a fact worthy of note is that the formation of soap persists, although, from the neutral phosphate becoming transformed into an acid phosphate, the emulsion is suspended in an acid fluid.

Emulsions of fat are also produced when bile and fused fatty acids are agitated together, or when pancreatic juice is shaken with both neutral or acid fats. This state of emulsion or of minute division of fats is indispensable for their absorption in the blood; an inquiry into the real nature of an emulsion would be therefore an object of much importance. Is it not something more than a mere mechanical division of fats into very small globules? I believe there are strong grounds for the assumption that there can be no emulsion without a chemical action, which may be conceived to take place in the following way.—When fused fatty acids are shaken in a test-tube with phosphate of soda or bile, the fat is instantly divided by a mechanical process into very minute globules, just as would happen if the fat was agitated with water; but at that moment each of these very small fatty particles becomes surrounded with a layer of soap, from the surface of the globule being saponified by the phosphate of soda or bile. From this circumstance the globules of fat lose their property of running into each other, and acquire a slight increase of specific gravity, which however is not sufficient to prevent them from rising slowly in the mother-liquor, and finally occupying the upper part of the liquid. Thus it is that the fluid is milky, and that the emulsion takes a much longer time to rise to the surface than pure oil would. The chemical analysis of the emulsion bears out this view, for I have found it to consist invariably of free fatty acid and soap.

This theory may have been already proposed without my knowledge; its adoption will assist in explaining the absorption of fats: for in this case, when in the form of an emulsion, they can no longer be in direct contact with the intestines, a layer of soap intervening between the fat and the membrane. Soaps may be conceived to pass through a membrane, whilst fat is known to be incapable of so doing; consequently we may imagine fats to be absorbed under the form of complex globules, the inside of which is free fat, and the outside soap.

Let us now return to the action of bile upon fats. After I had observed the property of phosphate of soda of emulsifying fatty acid, and not neutral fats, it struck me that bile might be possessed of a similar power, which I found to be the case; and, after giving this important subject all the attention it deserved, the results of my enquiries were communicated to the Royal Society in 1858. Shortly afterwards I was informed that bile had already been observed by Lenz, of Dorpat, to possess the above-mentioned property; and, by referring to his valuable Dissertation "On the Digestion of Fats," I found that he was the first who discovered, so far back as 1850, that bile emulsified fatty acids, but not neutral fats; he also noticed that the emulsion was attended with the formation of soap, and I readily avail myself of this opportunity of acknowledging the fact.

I cannot find that Lenz conducted his experiments with other fats than oil. I employed the solid fats of meat, which are those usually taken into the stomach; my attention was especially directed to the quantitative analysis of the emulsion, in order to determine the proportions of soap and fatty acids it contained; and finally I discovered this remarkable and interesting fact, that if the mother-liquor in which the emulsion is suspended be filtered from the emulsified fat, it is capable, notwithstanding its strongly acid reaction, of forming more soap and emulsifying more fatty acid, although before filtration the saponification had been entirely arrested; the filtrate from this second operation is likewise capable of effecting a further saponification and emulsion, but the action is less and less powerful; in one series of experiments, the proportions of fatty acids saponified were 20.5 %, 12.7 %, and 3.8 % of those experimented on.

The importance of the change which neutral fats undergo in the stomach will now be readily understood; for it is indispensable to their subsequent emulsion in the intestines by the bile, under which form they are absorbed. I do not wish, however, to attribute to bile exclusively the power of digesting fats: for I fully believe that a portion of the fats taken into the stomach is not acidified, and, consequently, must escape the action of the bile; in this case the pancreatic juice effects the required emulsion. It is remarkable that in both cases the fats are absorbed in great measure under the form of emulsified fatty acids; for it is now generally admitted that the pancreatic juice has the power of acidifying neutral fats, as was discovered by Prof. Bernard.

This absorption of fatty acids explains a fact I observed, with Mr. Verdeil, while at work in Messrs. Wurtz and Verdeil's laboratory at Paris, in 1851—namely, that blood contains free fatty acids.

The alimentary canal is so perfectly adapted to the functions of digestion, that notwithstanding the great variety of food taken, and the amount of nourishment ingested, which is frequently much greater than that strictly required for the maintenance of the body, only a very small proportion of food passes off in the evacuations in the healthy state; they may be said to contain no element of food soluble in water, for they merely yield to water a small quantity of an albuminous substance possessed of the characters of the albumin of the pancreatic juice, which in all probability it is. Under the microscope, we find in the alvine dejections, a few fragments of muscular tissue, from the meat taken, and the hairs of wheat ingested with bread; a few cells and fibres are all that remains of vegetable food. None of the fats of food are found therein in the free state, but the evacuations contain small quantities of phosphate of soda, soda soaps, and a larger proportion of earthy phosphates and earthy soaps.

The principal object of the alvine evacuation is obviously to rid the body of certain parts of the intestinal secretions, which, after having served their purpose in effecting the digestion of food, are not fit to return into the blood. It is very difficult to trace the connection of these excreted substances with the various intestinal secretions; in this respect, we must at present rest satisfied with probabilities. Among these various substances there is one of great interest, I have called *excretin* [$C_{78}H_{78}O_8S$]. This substance, which has been fully described elsewhere, crystallises from its alcoholic or ethereal solution, exhibiting beautiful silky crystals, which are acicular four-sided prisms. In its properties it closely resembles *cholesterin*, being soluble in hot alcohol and ether, sparingly soluble in cold alcohol, insoluble in water, and not saponifiable; like *cholesterin*, it dissolves in small quantity in the bile. It has also some physiological connection with *cholesterin*, as I have found the excreta of very young children to contain no *excretin*, but *cholesterin*, whilst it is the reverse in the case of young people and adults. On this account, I believe *excretin* to be a constituent of the bile.

PRACTICAL REMARKS ON LARYNGEAL
DISEASE AS ILLUSTRATED BY
THE LARYNGOSCOPE.

I TRUST, sir, that I do not lay myself open to the charge of presumptuousness in venturing to offer to you and the members of the Harveian Society some remarks on laryngeal disease, as illustrated by the laryngoscope. When, at the last meeting, you stated that there was a lacuna in the papers for this evening, I, perhaps somewhat rashly, volunteered to make some remarks on the subject; and I tender you my sincere apologies for the desultory manner in which I am compelled to treat it. But I admit that I was urged to come forward as I have done by observing, in the course of numerous conversations with my professional brethren, that the advantage offered by this new method of investigating an important class of diseases was not yet appreciated as I believe it merits.

A very limited number of medical men have among ourselves familiarised themselves with its use, and the public press has brought but little evidence that it has yet been turned to much account. I think that the laryngoscope is just one of those aids to accurate medicine which ought, like the stethoscope, to be in the hands of every practitioner. It is so manifestly calculated to throw much light upon a class of diseases that have hitherto been but ill understood, that it ought to become common property; and it is with a view to bringing forward a few facts that may indicate its value and convince you of the necessity of rescuing it from unmerited neglect, that I venture to bring before you the results of a comparatively limited experience.*

* The paper was illustrated by diagrammatic drawings, which it has not been thought necessary to reproduce here.

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Having, a good many years ago, felt the desirability of bringing the larynx within the range of vision, I myself sought to obtain that object by the introduction of a mirror into the pharynx. I must, however, admit my complete failure, from the want of that ingenuity and perseverance which have enabled others to succeed. Although, however, what I have this evening to say is based upon the knowledge acquired through the well-directed efforts of my friend Professor Czernak of Prague, it is but just to the memory of one who was probably well known to and esteemed by many of us, the late Mr. Avery, to state that he invented an excellent laryngoscope, which, but for his premature death, would certainly have been made better known to the profession. His instrument shows that he thoroughly understood all the objects to be gained, and had overcome the main difficulties in achieving them. It combines all the essentials requisite for obtaining a view of the interior of the larynx, and possesses merits which have not yet been realised by more modern laryngoscopes. The apparatus of Mr. Avery, however, laboured under certain defects which prevented its being very generally employed, especially unless its value were demonstrated by the manipulative skill of its inventor. These defects would doubtless have been remedied, had he survived. If, in dealing with the history of laryngoscopy, the name of the English surgeon demands a prominent position, we must also bear in mind that England gave birth to the first essay in laryngoscopy, though it proceeded from the pen of a foreign artist. I allude to the very interesting researches of M. Garcia, which, under the title of "Observations on the Human Voice", were published in the seventh volume of the Proceedings of the Royal Society, where this gentleman, the well-known brother of the famous Madame Malibran, made known a series of excellently conceived and carefully executed observations on the movements of his vocal cords in vocalisation.

Whatever merit is due to the two gentlemen just named, the greatest praise is due to Dr. Czernak and other continental philosophers for having fairly rendered laryngoscopy a means for the detection and treatment of disease. Dr. Czernak especially has popularised it by showing its practical utility, and introducing his mirrors in England. He has simplified it so that comparatively slight practice will enable any medical man to look into the larynx and trachea; and he has thus removed the affections of these parts from the domain of dreamland,

to which they have hitherto belonged. His work, ably translated by Dr. Gibb for the new Sydenham Society, is in the hands of most of you; and I would specially recommend it to your attentive perusal, if you have not already studied it. But I fear that, without the stimulus which Dr. Czernak gave to laryngoscopy by his personal visit to London during the past summer, the subject would scarcely have attracted even as much attention as it has. While he was here, he gave a series of demonstrations, and astonished many physicians and surgeons, not ill versed in their profession, by the remarkable display of his own vocal organs which great practice in auto-laryngoscopy enabled him to make. At the same time, some of us took advantage of the instruction he kindly imparted to overcome the *à s c* of laryngoscopic manipulation. There is but little, indeed, to show in this respect; but, as we all know, "*c'est le premier pas qui coûte*". We are all of us liable to the false shame of disliking the first step in an unexplored region, and are very grateful to any one who will carry us over the boundary, even though the barriers seem no barriers at all after they have been once passed.

And let me say at once, that while there is every inducement why we should learn the use of the speculum laryngis, the difficulties are in reality extremely trifling. A tolerably steady hand, a moderator lamp, and the necessary mirror, will enable every one of you to look into the larynx after a very few trials, provided you have suitable subjects to make your first essays upon; and the practice which private or public patients will readily permit of your acquiring will soon make you masters of the whole art.

My own experience further leads me to state that I think there must be a difference in the character of German and English patients; for I cannot otherwise account for the complaints which German authors (and there are many who have written on the subject) make with regard to the difficulties that often present themselves in the intolerance of the instrument on the part of the examinee. Possibly the great susceptibility often spoken of, the irritability of the fauces, the tendency to retch, which preclude all laryngoscopic examination for the time, may result from the examiner being self-instructed, and having to find out for himself those practical rules which a few explanations from one already versed in the use of the instrument would have enabled him to lay hold of at once. Still I cannot

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avoid the conclusion that we have in this respect fewer difficulties to deal with than our German *cofrères*; for every now and then I have met with cases in which that training of the fauces which they insist upon appears necessary, owing to the reflex action excited by contact of the mirror with the soft parts of the throat. In these cases, however, it is not even always, as it appears to me, contact which is necessary to produce the unpleasant effects; the mere effect of the imagination will, I am confident, operate so antagonistically to the operator as to preclude a fair field of inquiry. Strange to say, medical men themselves are by no means the best subjects, either in health or in disease, to deal with. Whether they are naturally more nervous, or, as I rather think, from their anxiety to assist the speculator, less obedient to his directions, their larynges certainly disclose their secrets less readily than the vocal apparatus of men and women who are unfettered by any anatomical knowledge or diagnostic considerations.

Allow me now briefly to state the main directions which may be given for the guidance of an incipient laryngoscopist.

Place the examinee on a chair of such elevation that, when you sit before him, his head and your own may occupy the same elevation. Place a good moderator or other lamp a little to one side of the patient's occiput, and let him approach as near the table upon which the lamp stands as possible. The patient's head should then be thrown back so as to straighten and stretch the passage to be explored as much as possible, and so that the axis of the introitus laryngis and of the trachea may coincide. As much depends upon the aid afforded by the patient, it is well, as a preliminary to the introduction of an instrument, to calm all apprehension by explaining that no pain can result; and that, if any trifling inconvenience should happen to arise, it will be a signal for the removal of the mirror. Before, however, this is used, it is important to learn how to throw a steady light from the reflector, which is fastened to the forehead by an elastic band, or may be carried between the teeth on a stem. Some beginners find in this proceeding the chief difficulty. Czermak's reflector has, as you see, a central circular spot which is not silvered, and through which the operator is supposed to look. Those who enjoy good eyesight will find the adjustment of their eye to this hole a trouble which they may altogether dispense with, by being content to see with the uncovered

eye alone. Having satisfied yourself that you have secured such a position for the lamp, the patient's head, and the reflector, that you obtain the best possible illumination of the patient's mouth, the next step is to prepare the examinee still further for the introduction of the faucal mirror. Let him put out his tongue, and either steady it with the tip of his right forefinger by pressing it slightly against the lower incisors, or take hold of it with the forefinger and thumb of the same hand. The object of this measure is to enlarge the cavity of the mouth, and more particularly to draw forward and flatten the base of the tongue, which otherwise proves an obstruction, and prevents the due passage of the light. The unvoluntariness of the tongue, the obstinate arising of its base, proves a serious difficulty in a few instances, but may be generally overcome by perseverance. Next let the patient be directed to breathe in and out as freely as possible. The very act of a full voluntary inspiration not only expands the fauces, but diminishes the reflex excitability of the parts.

Having made these preparatory arrangements, warm the mirror over the lamp; and, having tested its temperature against your own cheek, to insure its not being unpleasant to the patient, introduce it into his mouth, carefully avoiding all contact with the lips, teeth, or tongue. Push it steadily backwards against the uvula; and, while you rest the third and fourth finger of the hand that holds the mirror against the patient's chin, incline the mirror at such an angle to the horizon (about 75°) that the pencil of light which you throw upon it from the reflector may be conveyed into the respiratory tube. The parts that are illuminated will become visible in the mirror; and a little dexterity will soon enable you successively to examine the base of the tongue, the epiglottis and its attachments, the arytenoid cartilages, corpusculum Santorini and the thyro-arytenoid ligaments, the ventricles of the larynx, the vocal cords, and the trachea.

It is scarcely necessary to say that the hand that holds the mirror is to be kept on one side of the patient's mouth, so that it may not come into the line of vision. The stem of the mirror for correct observation will generally come to lie near the bicuspid tooth of the right or left side, according as we employ the right or the left hand. After getting over the first difficulties, it is always well to learn to introduce the mirror with either hand, not only for the purpose of satisfying ourselves

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that we have seen all accessible parts, but also because the introduction of pencils or probangs into the larynx is facilitated if we can manipulate well with the left hand.

If, after we feel confident in our own ability to use the mirror, there appear to be an undue irritability in the patient's throat, I have found the employment of an astringent gargle of essential advantage in reducing that irritability and allowing a more satisfactory exploration. Mathien's irrigator has served the same purpose, and may be recommended as an adjunct in the diagnosis as well as in the therapeutics of laryngeal affections.

The appearance of the living larynx and its appendages conveys a different impression from what most of us would probably anticipate who have only seen it in the dead body, and then only laid open and otherwise altered in its anatomical relations. The epiglottis is occasionally visible without the laryngoscope, if the tongue be very forcibly depressed; but this is so rare, that whatever colour it may present at such times cannot be a guide to its normal appearance. When unaffected with any inflammatory congestion, its surface presents a pale yellowish red colour, verging rather to straw-colour than pink. In irritative conditions, we find it in every stage of congestive redness: while on the other hand, in anæmic states, its blanched, bloodless, cream-coloured hue offers a striking contrast with the pink mucous membranes covering the adjacent parts. As a matter of course, its thickness varies in proportion to the congestive condition of its mucous investment, the serous effusion that may have taken place, and the character of the inflammation. We see one part more swollen than another, so as to destroy its symmetry, and causing it to appear what vulgarly would be termed *lopsided*. The epiglottis generally serves as an indication of the pathological condition of adjacent parts; and on this account it is a decided advantage to have obtained a view of this part alone, even if we fail to achieve a deeper view into the laryngeal cavity.

Even if we obtain a good view of the larynx during the first attempts at employing the speculum, it is not quite easy at first sight to recognise the true relative position of the parts. We must bear in mind that we do not see them directly, but through the medium of a mirror; their position is, therefore, reversed. The posterior surface of the epiglottis, that which is turned towards the intritus laryngis, presents itself to the

observer as if he were looking through a hole in the cervical part of the vertebral column; the anterior insertion of vocal cords appears to be behind, and *vice versa*. The right side of the patient's vocal appendages appears on our left; and similarly the anterior surface of the trachea looks as if it were the dorsal aspect. The uvula, again, gives us a little trouble sometimes, by pertinaciously appearing in front of the mirror and obstructing our view. This and other difficulties that we have to combat are not of any magnitude, and will be overcome by a moderate amount of perseverance, and by bearing in mind the anatomical relation of the parts and the simplest laws of optics.

After inspecting the epiglottis, the parts that next attract attention are the prominent parts of the arytenoid cartilages, and more particularly the *corpora Santorini*. Here we see, in health, a pale red smooth mucous membrane; while, in the various laryngeal affections that call for the use of the laryngoscope, we meet with great variations in the hue and state of turgescence, generally or unilaterally, of the parts. They are just such as might be anticipated *à priori* from what we see in other mucous membranes accessible to the eye; with this proviso, that we have hitherto had no means of localising the morbid conditions, and have been in the habit of setting down a certain class of symptoms as equally indicating affections of the whole laryngeal apparatus. The more I have seen of laryngeal disease, however, by the aid of the laryngoscope, the more I am satisfied that we have been greatly in error in our interpretation of symptoms. The upper thyro-arytenoid ligaments, their colour, their mode of action, and the space which intervenes between them, next call for attention. The amount of mucus investing the different parts is important; and it is well to be on our guard against the deceptive appearances which it presents. An ulcer may be easily simulated by the presence of a patch of viscid mucus; and I have seen a string of mucus stretched across the laryngeal passage in such a way as closely to resemble a true vocal cord. The explosive effect of a cough will generally suffice to put things in their true light; or it may be necessary to use a camel-hair brush to dislodge a seeming ulcer and expose a healthy or unshaded mucous surface.

The mucous membrane of the upper part of the larynx, and more particularly the ventricles which form the space between the vocal cords and the upper thyro-

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arytenoid ligaments, present numerous variations, which a little experience in laryngoscopy readily displays. I am inclined to think that these parts play a much more important function in the production of the voice than is ordinarily attributed to them; and that, in the great majority of cases of laryngeal disease accompanied by some modification or impairment of the voice, the morbid condition upon which the hoarseness or aphonia depends does not imply a direct lesion of the cordæ vocales themselves. I have certainly examined numerous cases of such derangement, in which I could discover no other lesion than that of more or less tumefaction and redness of the mucous membrane in one or all the parts above the vocal cords.

It is possible that, in some of the instances that have fallen under my notice, there may have been a direct impairment of the vibratile power of the vocal cords, or of the controlling influence of the thyro-arytenoid muscles, though not sufficient to be visible by the speculum; but, even without this, we may find an explanation of the fact by analysing the various elements which are conducive to the production of the human voice.

Any boy who has made a whistle out of a fresh willow or horse-chestnut branch knows how much the sound depends upon the form and size of the air-chamber which he fashions with his pocket-knife behind the slit (or glottis) of his instrument. In the same way, a swelling of the mucous membrane above the vocal cords alters the capacity of the air-chamber in which the laryngeal sound is produced; and this alteration will be effected by a tumefaction of the lateral parts, as also by swelling of the upper and more distant tracts of mucous membrane. I hold it pathologically demonstrated that, although the ventricles and the upper thyro-arytenoid ligaments do not by themselves permit the production of sound, they must both be in a state of integrity to enable the vocal cords to perform their normal functions.

If you look at the diagram representing a section of the vocal apparatus, you will particularly notice the important relation of the ventricles in regard to the air-chamber above the vocal cords. You will observe that, if a swelling of the mucous and submucous tissues occur at this point, the cavity lying immediately above the cordæ vocales must be more or less obliterated. The vocal cords may appear perfectly healthy, and seem to respond readily to the will of the patient; but, if there is not sufficient air immediately above them to take up

their vibrations, the effect will be that of a damper in a pianoforte. The case of a gentleman suffering from chronic laryngitis of catarrhal origin, which I have recently examined, and which was accompanied by complete aphonia, afforded me a striking corroboration of the correctness of this view. He was unable to produce a sound in the ordinary way—viz., during expiration; which, you will readily convince yourselves, is the ordinary mode of phonation, to use the pet term of laryngoscopists. But he was able, by an inspiratory effort, to produce a laryngeal sound. The mucous membrane of the trachea below the vocal cords being unaffected, the vibrations downwards were effected with sufficient readiness, and met with a sufficient body of air in the tracheal air-chambers, to cause a sound; but, when the air was forced upwards from the lungs, the obliterated ventricles checked the necessary undulations, and no sound resulted.

This I believe to be the explanation of the majority of cases in which the voice is altered, especially in those evanescent forms of hoarseness or aphonia accompanying the ordinary catarrhal affections of the larynx. Need I remind you, in further illustration of the influence upon the voice exerted by parts not in immediate relation with the vocal cords, how even tumefaction of the Schneiderian membrane modifies the voice?

But, if we remember how close the connexion is of the mucous membrane with the various muscles that contribute to phonation, we find a further mode of accounting for modifications of the voice, apparently due to affections of the membrane at a distance from the vocal cords, in the impairment of muscular contractile power. This, in its turn, may be propagated through the nerves or the vessels. This readily accounts for arrest of vocalisation where we see nothing but tumefaction in the vicinity of the Santorinian corpuscles; the large bundle of muscular fibres, denominated the arytenoid muscle, being necessarily more or less involved in any morbid condition of the superimposed mucous membrane, and thus causing a less perfect adaptation and closure of the vocal cords.

Nor must we lose sight of minuter changes in the character of the surface, either by injection of blood-vessels, altered epithelium, or vitiated secretion, as influencing the pitch and timbre of the voice. These may not necessarily be visible even with the light of the spec-

culum; but it will be scarcely necessary to adduce corroborative illustrations, though the fact may often be brought vividly before the practitioner in the rapid changes which local treatment is able to secure.

Allow me to pass on to the review of some of the circumstances revealed by the laryngoscope in regard to the vocal cords themselves.

I have already stated one negative fact, which I would repeat for the special benefit of the incipient laryngoscopist: that in many cases of laryngeal disease, where the voice appears even very seriously and permanently to have suffered, the cords themselves may present their normal appearance. I apprehend that this normal appearance will take every person by surprise who sees these membranous expansions for the first time in the living subject. The epiglottis is commonly paler and more glistening than the surrounding parts; but the contrast offered by the vocal cords with the adjacent mucous membrane is still more striking. They are almost white, of a pale cream colour, with a pearly sheen, quite different from the velvety character of ordinary mucous membrane. This peculiar colour enables us readily to recognise them, provided the light is properly thrown into the larynx, and minute changes are readily recognised in and about them. This part of the organs of phonation is one of the most interesting subjects for examination, both physiologically and pathologically; and the study of their movements alone is amply sufficient to repay you for any little trouble, and it really is but a little trouble, that the acquisition of the necessary dexterity may involve. To those of you who would wish to go more fully into the physiological part of the question, I would strongly recommend a perusal of M. Bataille's recent work on *Phonation*, written by a gentleman who, though by profession a teacher of singing, was educated to medicine, and distinguished himself in the earlier part of his career as an anatomist.

Although, as I have said, the vocal cords do not always exhibit pathological changes when, *a priori*, we might have anticipated them, they nevertheless present various lesions which could never have been discriminated without the speculum. We see in them (and I speak here, as throughout, from actual observation) the different degrees of inflammation, from the faintest injection to diffused redness, ulceration and loss of sub-

stance of greater or less extent, new growths, and evidences of paralysis, for none of which we have hitherto possessed any satisfactory and certain indication in the symptoms accompanying these conditions. Even my limited knowledge of the laryngoscope has satisfied me that we have been in the habit of confounding conditions widely distinct and necessarily demanding totally different treatment. I have seen warty excrescences simulate a nervous affection; I have seen paralysis with the symptoms of inflammation; I have seen urgent symptoms of acute laryngitis without a trace of inflammation. Imagine a case of the last kind coming to you suffering under extreme dyspnoea and aphonia, and telling you that, on a former occasion, the inflammation had been so severe as to necessitate tracheotomy; contemplate the effects that your venesection and leeches, calomel and tartar emetic, would produce; and say whether it is not our duty to familiarise ourselves with the use of an instrument which tells us that our diagnosis, and consequently our treatment, was utterly wrong?

Almost the first case in which I employed the laryngoscope was one which very forcibly impressed upon me the practical advantages derivable from the instrument. A man was under my care with cough, pain under the right clavicle, circumscribed dulness at that spot, a blowing systolic murmur, *Idées*, a pulse feebler in the right than in the left radial, and, as it were to corroborate the diagnosis compelled by the foregoing symptoms, aphonia. Who, under such circumstances, would have failed to diagnose a subclavian aneurism, causing, by its pressure on the recurrent, a paralytic condition of the thyroarytenoid muscles? What did the laryngoscope demonstrate? Two large epithelial growths proceeding from the anterior junction of the vocal cords. This did not necessarily vitiate the previous diagnosis, though it presented an unexpected feature, compelling a revision of the entire case, and a very evident indication for treatment at least in one direction not previously manifest.

The literature of laryngoscopy already furnishes us with numerous instances of growths occupying the vocal cords and their vicinity; and, with an improved diagnosis, we already see an advance in the more precise therapeutics of laryngeal affections. I have long satisfied myself of the perfect possibility of applying remedial agents to the interior of the larynx with certainty, and

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with benefit to the patient, by the means of sponges; but, before the laryngoscope came into use, we were necessarily in the dark both as to the nature and locality of the morbid condition we had to deal with. Our applications were made at a venture. Sometimes they succeeded, but they as frequently failed; because a general mopping out of the laryngeal cavity in a disease affecting only a given point was as likely to do harm as good, and because it was the merest guesswork as to what remedy was requisite.

The speculum laryngis enables us first to see definitely the characters of the malady calling for our remedial measures; and, secondly, to direct the application of sponges, camel-hair brushes, wire loops, caustics, inhalations, or irrigations, with something like an approach to the certainty of the issue which we can calculate upon in other maladies amenable to physical methods of diagnosis.

Few of us, who have watched the effects of galvanism, can fail to arrive at the conclusion that it is a very valuable agent in the treatment of disease. Its influence on the nervous, and I think even more upon the muscular, system, in cases suitable to its application, is undeniable. It is one among the various agents upon which we have been in the habit of relying the changes in laryngeal disease. But of what good can it be in a case dependent upon syphilitic ulcer of the vocal cords, upon catarrhal inflammation of the laryngeal mucous membrane; or in epithelial tumour growing from some part of the organs of phonation?

A simple uncomplicated case of paralysis of the vocal cords may be now diagnosed with as much certainty as a paralytic condition of any external part. We can see with the speculum that there is a want of symmetry of the movements of the two sides of the larynx, or that the movements are generally feeble or arrested; and we then apply our moist sponges, and direct the galvanic current with the precision, and with the same reasonable hope of success, with which we are able to direct it through the muscle of an extremity paralysed by rheumatism or by lead-poison, or irregularly excited by choreic spasm. Other modes of treatment are also of great value in such cases, and will readily suggest themselves. I place before you an illustrative diagram of a paralytic affection of the right vocal cord, which had caused aphonia for seven months. The patient was under

treatment for a fortnight without benefit, until, the laryngoscope having revealed the true condition of the parts, I caused her to use Matthieu's irrigator charged with a solution of tannic acid; and, after two sittings, she had recovered her voice, and the parallelism of the vocal cords was shown by the laryngoscope to have been restored.

If you refer to the literature of the subject, you will find that there is also evidence of the utility of the laryngoscope in exploring the condition of the inferior surface of the vocal cords. This, of course, can only be done in rare cases of chronic disease necessitating tracheotomy. The tracheal opening is used for the purpose of introducing a mirror, which, being turned upwards and receiving the light of the reflector, gives an image of the under side of the vocal cords with the same precision as the more ordinary mode of employing the laryngoscope affords of the upper parts. Of this I know nothing from direct observation; but I mention it because it may afford a clue to those cases in which we can detect no organic changes in the vocal apparatus, or in which only the tracheal mucous membrane appears to be the seat of any lesion.

The affections of the trachea are to a certain extent well brought out by the laryngoscope. If the larynx is fully open, there is no difficulty in throwing a good light on the anterior surface of this channel; the rings and the intervening spaces are readily recognisable under these circumstances; and their changes in colour or organisation cannot fail to be discovered. Variations of redness are most frequently met with. But, in illustration of the remark made just now of the influence exerted upon vocalisation by the parts below the vocal cords, I may state that, in one case of long standing aphonia connected or complicated with incipient pulmonary tubercle, I saw the trachea studded with small round yellowish spots, which I regarded as milium granules or ulcers. No other abnormality of the vocal apparatus could be discovered; and the vocal cords themselves appeared to act uniformly and symmetrically.

You will find it stated that the laryngoscope enables you to see down to the bifurcation of the trachea. I am confident that I have in one case succeeded in seeing the orifices of the bronchi; but this is almost an accidental circumstance, which cannot be reckoned upon. Whether,

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in the case of foreign bodies being introduced into the trachea, we can discover them, must depend mainly upon the greater or less readiness with which the patient bears the examination. I have not met with a case in books or in practice where I was able to test the laryngoscope under such circumstances. I should, however, deem it perfectly feasible, seeing how little reflex irritability is often found to exist after the foreign body has passed the chink of the glottis.

As to the question of the treatment of laryngeal affections, I do not propose now to go into it more fully. I have merely adverted to it in so far as it served to illustrate the use of the laryngoscope. For my own part, I am assured that both for laryngeal disease, and negatively or indirectly for the affections of other organs, this method of exploration is of extreme value. It is only necessary to make a few laryngoscopic examinations of suspected laryngeal disease to satisfy ourselves that without it our diagnoses not only want precision, but are destitute of any reliable basis. But this morning (Nov. 19th) a patient was brought to me with hoarseness of three years duration, depending upon a pathological condition which it would have been simply impossible to have surmised, but which the laryngoscope made as plain as if the parts had been displayed by dissection. The epiglottis was contracted so as to form a narrow triangular opening. The mucous membrane investing it and the vicinity was reddened and congested. The contraction, from reasons which it is unnecessary to enter into here, was probably congenital; and the hoarseness was attributable to a comparatively trifling affection of the mucous membrane, though obstinate on account partly of the patient's mode of living, partly, and yet more, owing to the peculiar malformation alluded to. I need scarcely point out how essentially the correctness of our diagnosis and prognosis is advanced in such cases by this new method of examination.

It is a field in which there are as yet but few labourers, but it is one which I would earnestly invite you to cultivate. I may repeat, that the pleasure you will derive from acquiring the necessary facility of manipulation far outweighs the labour you may have to bestow. Physiologically and pathologically, and, *à fortiori*, therapeutically, the laryngoscope is a great acquisition, which we should all utilise. As one man uses the stethoscope with greater facility and precision than another, so one

practitioner may become a more ready laryngoscopist than another; but, in the same sense as the former instrument has become the common property of the profession, so ought the latter to be understood by all. This, indeed, is the gist of my observations; and, although I am fully aware of the imperfect character of these jottings, I should consider that they were not fruitless if I had succeeded in convincing those of you who have not yet familiarised themselves with the laryngoscope, that it is an acquisition of considerable value in scientific medicine, deserving of your attention.

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C. H. Parker

ON THE USE OF NICOTINE

IN

TETANUS,

AND CASES OF

POISONING BY STRYCHNIA.

BY

REV. SAMUEL HAUGHTON, M.A., F.T.C.D.

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ON THE
USE OF NICOTINE IN TETANUS,
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*ἡ δὲ ἀντιρροία δι' ὅσους περιβάλλει τὸν ἀκρωτήρα καὶ κεφαλὴν, ἐκ
ἐκείνου ὅπου τὸν ζῶον τῆς κατὰ πέρας τελευτήσῃ. —GALEN.*

On the 29th of November, 1856, I laid before the Royal Irish Academy the results of some experiments made by me as to the physiological action of Nicotine and Strychnine on frogs. These experiments are published in the Proceedings* of the Academy, and appear to show that these two alkaloids exert effects counteractive of each other, at least on frogs. In the *Medical Times and Gazette* of the 12th of January, 1858, a case is published by Dr. Thomas O'Reilly, of St. Louis, Missouri, in which a serious case of poisoning by strychnine was successfully treated by the use of infusion of one ounce and two drachms of dry tobacco leaves, which was administered at intervals of five minutes, and with the result of completely resolving the muscular spasms, when half the above-named quantity had been given, and a complete cure was effected by the cautious continuance of the infusion. In this case six grains of strychnine had been taken.

It occurred to me, on reading this case, that nicotine might be usefully employed in cases of tetanus, as well as in cases of strychnine poisoning. I had an opportunity of testing the truth of this conjecture in two cases that occurred in this city in 1860 and 1861: the one, a case of traumatic tetanus; and the other, a case of idiopathic tetanus. I consider that both cases were satisfactory, in proving the utility of nicotine in counteracting tetanic spasms; although, in the first case, the man was moribund when the nicotine was given, and the original injury was so serious and extensive that no human power could have saved his life.

* Vol. vi., p. 420. 29th November, 1856.

I am indebted for the opportunity of experimenting on the first case to the kindness of Mr. S. A. Cusack and Dr. Croker, of Dr. Stevens' Hospital, who were present, and can testify to the remarkable effects of the nicotine; and I owe the opportunity of using nicotine, in the second case, to Dr. Alfred Hudson, who was physician to the Adelaide Hospital at the time the case occurred in that institution. Mr. Alexander Preston recorded the case in Dr. Stevens' Hospital; and the excellent notes of the case in the Adelaide Hospital were written for me by Mr. Anthony H. Corley, who was a pupil of that hospital.

CASE I.—*Traumatic Tetanus, treated by Nicotine.*—Patrick M'Garry, aged 34, a farmer, was admitted into No. 1 Ward, Dr. Stevens' Hospital, on the 29th of January, 1860, suffering from a severe and extensive burn. On Sunday morning, the 29th, at three a.m., on his way from Blanchardstown, his native place, to Dublin, he lay down on the wall surrounding a limekiln to sleep, which he did for two hours, but at five o'clock, by some mishap (according to his own statement, uninfluenced by drink), his lower extremities and hands came over the pit of the kiln, and were dreadfully burned. A stranger coming to the place at the time, found him almost in a state of suffocation; he immediately took him to his own house, and sent for Dr. Maguire, medical officer of the Castleknock district, who had him conveyed to Dr. Stevens' Hospital, where he arrived a little before noon, at which time Dr. Hamilton saw him, and ordered the parts injured to be thickly spread over with dry flour, and cotton to be laid over this, so as to exclude the air as much as possible.

On examination, the injury was found to occupy a very large extent of surface. Both legs, both feet, and both hands, with the penis and scrotum, were found to be implicated; the muscles, fascia, and soft parts of the feet and legs in particular, were in a very bad condition; the left limbs were more severely burned than the right, a fact which is easily accounted for by the way in which he lay at the kiln. The constitutional symptoms in his case were:—Collapse, drowsiness, thirst, delirium, and repeated and violent rigors; the surface was pale, and extremities cold; pulse feeble, 88 to 90; heart-burn was experienced; and at a later period continual twitching of the muscles, with violent spasm of same, followed finally by well-marked tetanus. He had no vomiting, and after the 3rd of February did not complain of pain.

3rd February.—He complains of severe stinging pains, running from the pit of the stomach and pelvis to the toes; and this day showed the first tendency to delirium, and rigors, which last from 30 to 40 minutes; pulse 88, and feeble. He is annoyed by disagreeable heart-burns. Took castor oil this morning, which had good effect.

6th February.—Last night the slough began to separate, and this morning he was seized with tetanus, almost closing his mouth, and rendering it

impossible for him to open it wider than would admit a spoon: it has to be kept open by a plug of wood. In other respects he has been much the same since the 3rd.

8th February.—He is in a very dangerous state to-day: at intervals of about an hour each he has a very violent spasm.

At 8h. 40m. a.m. he was visited by Professor Haughton, at which time his pulse was 130. Mr. Haughton gave him one drop of nicotine (0.6 gr.). This had the effect of lowering the pulse to 105 at 9h. 5m. a.m., and of rendering the respiration fuller and less frequent; and at 9h. 50m. a.m. the nicotine had lowered the pulse to 92-88.

At 10h. 55m. a.m. he got one drop of nicotine. Before this the pulse had been 112; shortly after the dose it fell to 92.

At 12h. 20m. p.m. had a very violent spasm; and at 12h. 55m. p.m. got one drop of nicotine. The pulse had been 116, and was lowered, as before, by the nicotine.

At 1h. 40m. p.m. had a very violent spasm, pulling his bedclothes with all his might.

At 1h. 50m. p.m. pulse very quick, and intermitted, ranging from 112 to 150.

At 2 p.m. pulse 112. For the last 20 minutes his breathing has been very rapid, and greatly impeded, owing to mucous deposited in the bronchial tubes, for the absorption of which he has iodine rubbed over the upper part of the chest.

At 2h. 10m. p.m. had a very violent spasm, just before which he was conscious, answering indistinctly when spoken to. In addition to the tetanic spasms, he has had continual twitching of the muscles yesterday and to-day.

At 2h. 40m. p.m. had a very violent spasm.

At 2h. 45m. p.m. expired.

The effects of the nicotine observed in this case were the following:—

1. The immediate relaxation of the spasm of the muscles of expression, of respiration, and of deglutition.
2. The cessation of delirium, and feeling of relief from agonising pain.
3. The lowering of the pulse from 130 to 88 per minute.

These effects succeeded the administration of the nicotine up to the last moment, and by the use of this alkaloid the unfortunate sufferer was spared a most painful death, and allowed to die quietly of double surgical pneumonia, consequent on the extensive surface injury, instead of dying in torture by tetanus.

CASE II.—*Idiopathic Subacute Tetanus, caused by exposure to cold; treated by Nicotine.*—Arthur Kershaw, aged 40, Gardener, was admitted into the Adelaide Hospital, January 2nd, 1861, under care of Dr. Hudson. He stated that the nature of his occupation exposed him to all kinds of weather, and consequently he received several severe wettings during the

past winter. The last of these occurred a short time before Christmas. He did not experience any immediate ill effects from that wetting; but early on Christmas Day, whilst standing beside the canal looking at the skaters, he was suddenly seized with cramps and pains in his lower extremities. He described these sensations as commencing in the loins and passing downwards, both in front and behind, as low as his knees. This attack was not very severe, lasted about an hour, and when it was over he was perfectly free from pain. The same night he had another attack precisely similar to the first but rather more severe. From that time the spasms increased both in frequency and intensity, but did not occur with any degree of regularity. On the day of his admission he was able to walk from his residence to the Dispensary, a distance of about half-a-mile. He was late, however, for admission, and on going home he was again seized with the spasms and obliged to take a car. That evening he became so much worse that it was necessary to have him conveyed to Hospital. It was subsequently ascertained that the different members of the patient's family, as well as himself, had been remarkable for possessing less than ordinary mental capacity; one of his brothers having been epileptic, and subject to attacks of furious mania. In one of these paroxysms, which occurred about sixteen years ago, he struck our patient on the head with a spade, knocking him senseless, and inflicting a severe wound. There was no evidence of fracture, and the wound soon healed. From that time, however, he became subject to a feeling of weakness in his head, never amounting to absolute pain, and never causing any inconvenience except preventing him from bearing any weight on his head; such, for instance, as a heavy hat, and obliging him to keep his head cool. This sensation was not increased by drink. On the succeeding morning, when seen by Dr. Hudson, his condition was as follows:—He was lying on his face in bed, his legs and thighs forcibly extended and fixed. The erector spinae muscles, the glutei, and those of his thighs were spasmodically contracted, hard, and rigid. The adductors and tendons of the hamstrings in particular, stood out like tense cords, and the muscles themselves could be seen quivering under the skin; this condition was next manifest in his left thigh, and he said it had been so from the commencement of the attack. His legs, however, were not affected; his abdomen was tense and hard, and he complained of pain about the ensiform cartilage, caused, apparently, by the spasms in the recti muscles. Slight relaxations occasionally took place, but, during the time he was being examined there was no complete remission; pressure over the spines of the superior lumbar vertebra appeared to cause slight pain, but did not increase the spasms. The agony he suffered during the occurrence of these appeared to be excruciating. In the partial intermissions that took place, he was able to describe his sensations, and it was thought, from the manner in which he said he walked, that his left leg

was paralyzed. He stated that his appetite had been good up to the previous day; that his bowels had been regularly moved; and that he had passed his urine without any inconvenience.

He was ordered to be cupped on either side of the tender part of the spine, to have hot stupes applied immediately afterwards, and to take the following every hour—a powder containing one twelfth of a grain of calomel, and a grain of James' powder.

At one o'clock the same day, after he had been cupped, his pulse was 84, full and labouring; the spasms were as severe as in the morning, but not so frequent. When they occurred he obtained most relief by being placed on his feet and allowed to sink gradually on his knees. It then became apparent that the inability to move his left leg which he had spoken of in the morning arose, not from paralysis, but from rigidity of the entire limb. At seven o'clock in the evening he seemed quieter; the spasms were more frequent but not so severe.

January 3rd. The spasms and pains continued without diminution; they occurred so frequently and with such violence during that night, that he was unable to obtain any sleep. They had extended this morning to his legs, the muscles of which had become as hard and rigid as those of his thighs and abdomen. He was observed to hiccup once, and he stated that he had done so two or three times during the night. His pulse was 96, but very irregular, both in rhythm and fullness. The powders were ordered to be omitted and the following pills to be substituted—calomel and James' powder, twelve grains; extract of belladonna, four grains; extract of hyoscyamus, a scruple. To be divided into six pills, and one to be taken every third hour. A fetid and terebinthinate enema to be administered immediately, and a long narrow blister to be applied on either side of the spine. At seven o'clock he was asleep, and it was not judged expedient to awaken him.

January 4th.—There was no appreciable alteration in the character or situation of the spasms. The patient appeared weaker, but said he suffered as much agony as ever. There was a permanent flush on his cheeks and forehead. His pulse was 120, but regular. He said he had hiccupped several times during the night, and he was frequently observed to gulp, as if flatus were coming off his stomach. The enema had produced several copious evacuations. The pills to be repeated, with the extract of belladonna increased to one grain in each. The blistered surface at the spine to be dressed with an ointment composed of 4 drachms of muriate of morphia to 4 drachms of lard; 1 drachm of mercurial ointment to be rubbed in at the inner side of each thigh. Subsequently Dr. Duncan saw him, in conjunction with Dr. Hudson, and the patient's tongue being found brown, dry, and fissured, he was ordered the cardiac mixture, 2 ounces to be taken occasionally; brandy to be administered, if necessary, and a large, hot, bran poultice to be applied to the spine.

January 5th.—During the night the patient became suddenly delirious, and afterwards passed into a state of coma, from which he was aroused by the administration of stimulants. Sinapisms were subsequently applied to the soles of his feet. In the morning he appeared quiet, and did not complain of much pain, but the spasms continued, with very slight occasional relaxations. Although not delirious, he was not quite rational, and sometimes spoke rather unconnectedly. His tongue was not so brown or dry as on the preceding day. His pulse 100, regular, but weak, and his pupils slightly dilated. His pills were stopped, and others substituted, containing calomel, camphor, antimonial powder, and extract of hyoscyamus, of each one grain, of which he was to take one each second hour.

January 6th (Sunday).—He appeared somewhat better, but spoke so incoherently, that it was impossible to ascertain how he felt. The condition of his limbs, however, was unchanged. His pulse 96. He had been very unmanageable during the night; got out of bed, and attempted to stand, but fell, striking his back against the rail of the adjoining bed.

January 7th.—The delirium was increased, pulse 84, tongue moist, but rough and white in the centre. The rigidity of his limbs was not in the slightest degree diminished. He had been very violent during the night; got out of bed, and insisted on lying on the floor. A severe purging had come on, and there was a mercurial fetor from his breath. The urine which he had passed on Saturday, having been kept in a glass vessel, was examined, and it presented a very striking appearance. There was a copious sediment deposited, consisting of two distinct strata; the lower and larger of the two, whitish in colour; the upper a very thin purplish layer. The supernatant fluid was high coloured; alkaline in reaction, and it exhaled a very strong ammoniacal odour. A drop of this, the sediment having been previously stirred up, was examined under the microscope, and crystals of the ammoniaco-magnesian phosphate, in large quantities, were observed.

January 8th.—The preceding night it was found necessary to remove him into a ward by himself, in consequence of the violence of his delirium. During his removal, the muscles of the upper part of his back and those of his neck showed a tendency to participate in the spasmodic action. This was manifested in a slight attack of opisthotonos, which lasted for a couple of minutes. An enema of morphia in decoction of starch was administered, and a blister was applied to the nape of his neck. After these measures, he became quiet, but there was no relaxation in the spasms of his limbs. When seen in the morning, he was quite delirious; speaking loudly and continuously, and not ceasing even when spoken to. He continued in that state from five o'clock in the morning until noon, when he gradually became quiet. His pulse was 84.

At the suggestion of the Rev. Professor Haughton, Dr. Hudson determined to try the effect of nicotine. That medicine was, therefore, administered four times during the day, according to the former gentleman's directions, and with the following results:—At two o'clock, the patient, having sunk into an uneasy sleep, was awakened. Half a drop of nicotine, which is equivalent to three-tenths of a grain, was administered in 2 drachms of wine and water. His pulse was 80, but rose within ten minutes up to 88. At half-past three, when he was next visited, his pulse was 78, and he was bathed in a profuse but clammy perspiration. At half-past five, his pulse being still 78, the nicotine was repeated in the same dose. The pulse shortly afterwards rose to 100, and it was beating at that rate at six, seven, and eight o'clock. Immediately after the administration of the first dose, the abdominal muscles became manifestly relieved, and the abdomen rose and fell to a nearly natural extent in breathing. The muscles of the limbs, however, were still rigid. Towards the evening he became quite rational, and when visited at half-past eight, he was sleeping calmly. When awakened, he was in perfect possession of his senses; his pulse 78; his tongue white, but moist, and his pupils, which had been much contracted during the day, were dilated to a natural extent. His urine was tested and found neutral, but when allowed to stand for some time, phosphates in abundance were deposited. At nine o'clock he got the third dose, but this did not affect his pulse, which remained at 78, until midnight. He then got the fourth dose, and shortly afterwards his pulse rose to 80, and did not alter during the night.

January 9th.—This morning he appeared improved in every respect. He had slept well, and there had been no return of the delirium. Although he still complained of pain during the presence of the spasms, he said it was not nearly so severe as before, and the intervals between the spasms were growing much longer. He seemed unwilling to move his arms, or stir himself in any way, as he said that the least motion sometimes brought on an attack. His abdomen was soft, and nearly natural, but there was no appreciable change in the state of his limbs. The profuse sweating still continued. At 11 o'clock he got the fifth dose, his pulse being then 86. The sixth was administered at five o'clock, the seventh at eight, the eighth at midnight; his pulse, during the day and night, ranging from 78 to 82. From the time that he became rational, his strength was kept up by beef-tea and small quantities of brandy, administered at intervals during the day.

January 10th.—This morning the improvement in his condition was remarkable. He could move his arms freely, without inducing the spasms. These seldom attacked him, and even when they did, the pain, in comparison to what he had suffered, was trivial. He was able to move his legs and bend his knees a little for the first time. His spirits rose, and he began to feel hopeful. During the intermissions the muscles of his legs were

perfectly relaxed, and those of his thighs were not quite so tense as they had been. The sweating continued, and he said his appetite was returning. The dose of nicotine was increased to three-quarters of a drop, or nearly half a grain; and the first of these altered doses was given at 11 o'clock a.m., his pulse being 90. In half an hour it was 96. At two and at five, when he got the second and third doses, it was still 96. At eight o'clock he was not so well as in the morning. The spasms had become a little more frequent and painful, but yet not anything like so severe as they had been; and during the intervals the muscles, even those of his thighs, were perceptibly softer and less rigid. He seemed somewhat depressed and anxious, and complained greatly of thirst: his pulse had risen to 100. The medicine was administered every three hours during the night. Immediately after each dose the pulse rose to 100, but in a short time fell to 92.

January 11th.—There was further improvement this morning. The muscles of the abdomen and legs were quite soft and flaccid, and those of the thighs becoming gradually relaxed. There had been no return of the spasms from 12 o'clock the preceding night. The sweating still continued, and exhaled the peculiar snuffy odour characteristic of the nicotine. The power of moving his limbs was increasing. As he complained of being tired of the beef tea, it was ordered to be omitted, three eggs daily to be substituted for it; and stirabout, for which he expressed a desire, was ordered. His brandy was discontinued, and the nicotine ordered to be given in plain water. The next dose, the ninth of the increased quantity, was given at half-past four; and at half-past seven it was repeated, his pulse being 98. Up to that time there had been no return of the spasms. His bowels not having been moved for some days, a laxative enema was ordered.

January 12th.—He was not so well this morning. The sweating had ceased, and his pulse was 120. He had had only one or two spasms, and they were not severe, but he appeared nervous and anxious. The angles of his mouth were drawn slightly backwards, giving his face a peculiar expression. The fact of the cessation of the sweating being coincident with the discontinuance of the stimulants, suggested the idea that the nicotine required to be combined with these, in order to exert its full constitutional effects. The brandy was, therefore, again ordered, and the nicotine to be given in sherry in the following proportions:—Twelve drops were mixed with two ounces of wine, of which one drachm was to be taken every three hours; the dose being, therefore, thirteen-sixteenths, or, in round numbers, three-quarters of a drop. At half-past six in the evening, the medicine having been regularly administered, with the stimulants, he was again improving; his pulse down to 96, and his skin moist and soft.

January 13th (Sunday).—His pulse was 100, but he was in every other respect still improving.

January 14th.—He felt himself much better, was able to move his legs and bend his knees to some extent. In the evening the muscles of his thighs and abdomen became a little more tense than they had been for some days, but he did not complain of pain. His pulse during the day ranged from 92 to 108, and at half-past seven it was 102. He was then quiet, and inclined to sleep. The dose of nicotine to be reduced one-half.

January 15th.—His face had regained its ordinary expression, and he could move his limbs more, but he did not like to do so, as it gave him some pain, and appeared to require great exertion. His pulse varied from 88 to 100.

January 16th.—This morning he had still more power in his limbs, and said he felt much better. There was, however, an anxious expression in his face, and he did not speak naturally, or with as much distinctness, as he had done previously.

January 17th.—The nicotine to be administered only three times a-day.

January 18th.—The general improvement was becoming more and more evident, and he was making inquiries as to when he would be allowed to get up. His pulse maintained the same character and variations.

January 19th.—He was able to sit up in bed. He got one dose of nicotine, and then the medicine was discontinued.

January 20th.—He was allowed to get up for the first time. He expressed himself as feeling very well; was able to stand without assistance, and could walk a step or two by holding on to the rail of his bed. A glass and a-half of brandy to be taken during the day.

January 21st.—He was up and moving about for some time.

January 22nd.—Feeling rather fatigued from the preceding day's exertions he remained in bed this day.

January 23rd.—No perceptible change; but he said he was getting gradually better. He had not been attacked by the spasms for several days, but still the muscles of his thighs were by no means completely relaxed, the adductors and hamstrings particularly.

January 24th.—Two strap-shaped blisters were ordered to be applied, one on either side of the lumbar spines; and he was desired to take five grains of iodide of potassium in a wine glassful of compound decoction of sarsaparilla, three times a day.

No circumstance worthy of note occurred during the remainder of the week. On Saturday the 26th, he was up and walking about the room, but was obliged to use a stick to support himself. The muscles of his thigh were still stiff, but there was an entire absence of pain. The discontinuance of the nicotine did not appear to produce any material change on his pulse, which ranged from 90 to 100 during the week. The sweating continued, but not anything like as copiously as during the administration of the nicotine. During the succeeding week there was gradual but decided improvement. On Saturday the 2nd of February,

he was able to walk remarkably well, and all he complained of was a slight stiffness about his knee joints. All his muscles, even the adductors and hamstrings, were perfectly relaxed, soft and flaccid.

He was discharged from hospital on the 6th of February, and the only inconvenience which he complained of was the stiffness in his knees, which was then rapidly diminishing. In all other respects he declared himself perfectly recovered.

The quantity of nicotine given to Kershaw, from the 8th to the 19th January (11 days), amounted to 44 drops, or 26·4 grs.

The following statements as to the physiological effect of this alkaloid, when combined with minute doses of alcohol, appear to be borne out by the details of the case:—

1. It produced immediate relaxation of the muscles of the abdomen, back, and diaphragm.
2. It caused cessation of delirium.
3. There was a slight tendency to cause increased circulation, to the extent of 10 beats per minute.
4. It caused profuse sweating, which exhaled an intolerable odour of snuff, not of tobacco.
5. It had a tendency to produce deep sleep.
6. It failed to control quickly the adductor muscles supplied by the obturator nerve; and even when the hamstring muscles gave way, the adductors refused.

I became aware, after I had used nicotine in the two preceding cases, that tobacco enemata had been employed successfully by the late Dr. O'Beirne,* of this city, in the treatment of traumatic tetanus. Dr. O'Beirne was induced to employ tobacco, from its known power in overcoming constipation and destroying worms, as well as from its control over the nervous system.

Hippocrates distinguished between traumatic and idiopathic tetanus, pronouncing the former fatal (*taurikos*), and stating that sufferers from the latter would recover, if they survived the first attack for four days. I believe that most practitioners would substitute 20 days for four days, at least, in our latitudes. He appears also to have distinguished between tetanus accompanied by opisthotonos, and by emprosthotonos, using venesection in the former case; and hot stupes with wine internally, in the latter.

Aretaeus invented the terms, Opisthotonos and Emprosthotonos, and considered the causes of tetanus to be either a wound (including abortion in a woman), or exposure to extreme cold; and used the same remedies as Hippocrates, superadding cupping at each side of the occiput, and purgative enemata—his method of stuping is one that might, possibly,

* Dublin Hospital Reports, Vol. iii., 1822, p. 343.

with advantage, be revived among us; viz., by means of warm oil applied in bladders of animals, half filled, so as to lie flat on the surface of the limbs, and take their shape.

Galen, in his book on Rigors, chap. viii. states, that he considers spasms in the muscles, such as occur in tetanus, to be the result, either of too great humidity or too great dryness, acting upon the nerves that influence the muscles. "Consider, therefore," he says, "that the nerves and tendons in the bodies of animals may either be distended by superfluous moisture, or dried up by causes promoting exsiccation, so that they reach that degree of tension at which voluntary motion takes place in those that are healthy."

"Thou knowest, indeed," he adds, "that it is allowed by all that there are three kinds of this affection—the first, which is and is called Emprosthotonos, the second, Opisthotonos, the third, Tetanus. . . . The spasms which arise from dryness (*xerotes*), thou shalt never heal, for those thus affected, die presently, not even allowing time for devising a remedy; but the spasms that arise from repletion or inflammation (*πάρεσις ἢ φλεγμονή*), these thou can'st heal, by evacuating the repletion, and by curing the inflammation by its appropriate remedies."

The distinction here drawn by Galen is perfectly just, although not expressed in modern language, and is, perhaps, as intelligible as the "explosions of the animal spirits" to which tetanus was ascribed two hundred years ago, or as the contraction by polarity, by which some moderns think to explain this mystery, *obscurem solvitur per obscurius*.

Of the remedies proposed for tetanus, the venesection of Hippocrates has no chance of trial, until time shall have exploded our prejudice against blood-letting; and if we believe, with Galen, that it is an acute disease of the nervous system, our choice of remedies must lie among the vegetable alkaloids. Of these, certainly, Nicotine and Belladonna are the most likely to succeed; and nicotine, if used, should be employed as such, and not in infusion of tobacco leaves, in which its properties are masked by the action of two or more vegetable oils, the operation of which on the nervous system is unknown.

CASE III.—*Attempted Suicide with Strychnine; Treatment with Nicotine (Tobacco); Recovery.*—Within the last few days a serious case of attempted suicide with strychnine, has been treated successfully in the Meath Hospital by Dr. P. C. Smyly, with tobacco, after the manner so well applied by Dr. Byrne in the celebrated case at Cincinnati, U. S. A. I shall give the narrative in Mr. Smyly's words, adding little by way of comment:—

Thomas Cuddy, aged 15, was apprenticed to a druggist about a year ago. He did not like the business, and ran away. His father brought him back, and set him to work again. He has always borne an excellent

character. He on several occasions told his parents that he did not like his trade. As they did not attend to his wishes, he made up his mind, on the morning of Tuesday, 11th March, to make some change for himself in his circumstances. At about half-past eight of the same evening, he took down the strychnine bottle from the shelf, shook out on his hand about as much as would cover a shilling (his own account), swallowed the whole of it, and then ate a quantity of raisins, to take away the bitter taste from his mouth. One of his fellow-shopmen having seen him, ran and told his master, who was at his tea, what had occurred. The boy denied having taken anything, and went on with his work as usual. In about from 20 to 40 minutes he felt his legs getting stiff, walked to the end of the counter, and fell backwards with a jerk into his master's arms, crying out, "I did take it; oh! turn me on my back." He had eaten a full meal of potatoes and meat at half-past four o'clock. An apothecary was at once sent for. His assistant came and administered about three grains of tartar emetic, without exciting vomiting. The apothecary then arrived, and put the boy under the influence of chloroform, "to facilitate his removal into the hospital, the spasms were so violent."

The resident pupil, Mr. J. Foot, gave him some sulphate of zinc. This having no effect, was followed by a mixture, consisting of an ounce and half of mustard, vin. ipecac. 3 drachms, and a pint of warm water. Only a few mouthfuls of this were ejected!!*

When I arrived, at 10 15 P.M., he was lying on his back, his head thrown back, chest raised and fixed, limbs rigid, hands clenched, eyelids spasmodically closed, and corneae turned upwards. The priest told me, that during the administration of the last rites of his church, the boy was seized with a sudden spasm, which threw a tin tray with some violence into the air; the tray was resting on his abdomen. I had an infusion of tobacco made, by pouring a pint of boiling water on about an ounce of cut Cavendish, heated over the fire, and strained; cold water was then added until the liquid was tepid. I made him drink two-thirds of this. Furious vomiting followed instantly. He swallowed the rest, with same result. The stomach appeared to be completely empty. He lay quietly on his back for about five minutes, when he was seized with a violent spasm, and quivering of the whole body, then complete opisthotonos and clenching of the hands. At about the middle he gave two short screams, as if he was in intense pain. I gave him another pint of the infusion in three doses, all followed immediately by vomiting. Another pint was prepared from the same ounce of tobacco; about a teacupful of this was retained in the stomach for about five minutes; a second was retained somewhat longer. Profuse sweating now commenced, and he slept for

* The stomach pump was tried, but the pipe was stopped by the portions of potato he had eaten for his dinner.

a short time. I left him for about half an hour. On my return I found him lying quietly on his back; all his muscles, except those of the legs, relaxed, breathing less rapid, pulse slower, soft, and rather weak. I turned him on his side (which he was afraid to do himself). He drew up his knees, put his hands under his head, and went to sleep. The night nurse says he only turned once during the whole night to ask for a drink.

Wednesday morning.—Appears weary, and rather sulky. Hamstrings* still hard and stiff. Pulse 120.

Thursday.—All his muscles relaxed; complains of some tenderness along the spine; appetite returning.

It appears to me clear, from this case, that the tobacco produced two important effects:—

1st. It produced vomiting, when all other means had failed, and so saved the patient from the yet future effects of the strychnia remaining undissolved in his stomach.

2nd. The physiological effect of the nicotine absorbed by the system, counteracted that of the strychnia already absorbed, as in the Cincinnati case, and as in the case of my own experiments on frogs.

From the statements of the boy to myself, checked by comparison with a known weight of strychnia, I estimate the quantity of the poison taken by him at four grains. The case itself furnishes a powerful argument in favour of the introduction of Nicotine into the Pharmacopoeia, as the dose of this alkaloid might be regulated with any required degree of precision; whereas the strength of a decoction, or infusion of tobacco, is almost entirely unknown. Too much credit can scarcely be given to Mr. Smyly for his promptitude and courage in administering the only known remedy; but, at the same time, it cannot be denied that dangerous consequences will probably arise from the use of tobacco in such cases, as the strength of the remedy employed is unknown, and its own effects upon the system are only second in violence to those of the strychnia it is employed to counteract.

In concluding this paper, I have thought it useful to bring together, in one table, several of the best observed cases of strychnia poisoning, so as to show the dose, the interval till the first spasm, and the interval till death, when fatal. In forming this table, I have largely availed myself of Dr. Alfred S. Taylor's work *On Poisoning by Strychnia*. 8vo. London, 1856: p. 78.

* Note added by Professor Huxley.—This obstinacy of the hamstring and adductor muscles appears remarkable, when considered in connexion with the similar tension of these muscles, which was observed in the case of idiopathic tetanus already given. It would almost appear as if the controlling power of the nicotine were limited to a certain tract of the spinal cord, and did not extend below it.

Physiological Phenomena accompanying Cases of Poisoning by Strychnia.

Case	Authority for Case	Age and Sex	Dose	Interval 1st dose	Interval 2nd dose
1	Booth and Bartley— <i>Medical Times and Gazette</i> , July, 1856.	46 M.	Grains	Minutes	Minutes
2	Bumhardt— <i>Lancet</i> , 7th January, 1858.	17 M.	1.50	—	165
3	Bennett— <i>Lancet</i> , 21st August, 1859.	13 F.	4.99	15	90
4	Watson— <i>Edinburgh Monthly Journal</i> , December, 1845.	12 F.	1.50	60	150
5	Cornwall's <i>Monthly Journal</i> , February, 1846.	12 F.	0.75	20	65
6	Thimhardt— <i>Casper's Weekender</i> , 7th February, 1846.	28 M.	2.00	20	105
7	Warner— <i>Br. Am. Journal</i> , August, 1847.	1 M.	20.00	15-20	30
8	<i>Pharm. Journal</i> , 1848, vol. II, p. 298.	12 F.	1.00	5-10	75
9	<i>Med. Times and Gazette</i> , 26th April, 1855.	12 F.	1.00	—	90
10	Lonsdale— <i>Ed. Med. Journal</i> , 1855.	20 M.	1.50	20	60
11	Goodenough— <i>Dub. Med. Press</i> , 25th June, 1855.	28 M.	5.00	15	25
12	J. B. Wilson— <i>Med. Times and Gazette</i> , 8th March, 1862.	18 F.	—	15	35
13	P. C. Novly— <i>Dub. Med. Press</i> , 17th March, 1862.	15 M.	4-5	20-40	—
14	J. P. Cook—Murdered by Dr. Palmer, 20th November, 1855.	28 M.	—	75	75
15	Lawrie and Cowan, 1855— <i>Taylor's Med. Jurisp.</i> , 7th edit., p. 208.	—	—	90	—
16	Anderson, 1845.	Do.	Do.	120	—

POSTSCRIPTUM.

CASE IV.—*Tetanus—Recovery*.—Since reading the foregoing paper, I had an opportunity, through the kindness of Mr. Tufnell, of trying the effect of nicotine in a case of traumatic tetanus (John Dobbins) in Baggot-street Hospital.

Mr. Tufnell writes, 16th March:—"The tetanus case has been fairly tried under chloroform, and will, I think, not do well under it. The nicotine will, therefore, if successful, be entitled to full credit."

The case was one of compound fracture of the right radius, and will be published in full by Mr. Tufnell; I, therefore, only give the following facts respecting the effects of the nicotine:—

The first dose of nicotine (one drop) was given on Sunday evening, 16th March, and produced rapid relief of the spasm of the muscles of the neck, chest, and abdomen, followed by profuse sweating; and the patient expressed himself as being much relieved.

From the evening of the 16th till the morning of the 18th nicotine was given, in doses of 1, 2, and 2½ drops, in sherry and water, according to the urgency of the tetanic spasms. After the administration of the dose, in three minutes, the spasm was gone, and the muscles relaxed, and profuse sweating, accompanied by a smell of snuff, set in.

On the 19th the nicotine was not given, but was resumed on the 20th and 21st, with the same beneficial results as before. From the latter date to the present time (4th April) it was not found necessary to give nicotine, and the patient is now gradually recovering.

During the four days on which he was treated with nicotine, he received, by the mouth and rectum, altogether 54 drops=32½ grains; and the physiological effects appeared to be the same, whichever way the alkaloid was exhibited.

CASE V.—*Tetanus (Idiopathic) in a Horse treated by Nicotine*.—Dr. Barton

* Sulphate of Strychnia.

has favoured me with the following account of the use of nicotine, injected into the cellular tissue of a horse's neck. Although the case terminated fatally, it is instructive, as showing the physiological action of the nicotine in this animal. There appears to have been the same effect in relaxing spasm, and relieving respiration, that is observed in man:—

A grey horse, 11 years of age, was driven 40 miles in a car upon the 1st of Jan., 1860, and driven back the same distance upon the following day. Upon the morning of his return, at starting, it was noticed that he was stiff—"going wide in hind legs." Next morning he could not turn in the stall, and was quite stiff. Was not attended to, and got worse. Sent in on the 6th to Mr. Martin, veterinary surgeon, in Peter-street, who found him in the following state:—Hair standing all over his body; muscles of neck and trunk rigid; belly retracted; legs stiff, and separated; nose protruded; unable to open his mouth more than about an inch; frothing at mouth; bowels moved scantily. Mr. Martin gave him a ball, which had moved the bowels. When I saw him I determined to try the nicotine solution. We employed it in the following way:—

Jan. 11th.—Gave him 20 drops of the solution of nicotine in an injection, with croton oil.

Jan. 12th.—Gave him 10 drops twice in his bran mash, but he only took some of it.

Upon the next day I injected beneath the skin of the neck about the same quantity—20 drops. During this treatment his muscles became decidedly softer, and he moved his legs better. His respiration became hurried, and he died upon the night after the injection. After death the muscles were not rigid.

Wie hat der Staat
der
Prostitution gegenüber

sich zu verhalten?

Eine zeitgemässe Frage

in Bezug auf die gesetzlichen Bestimmungen Sachsen und das dormalige
Verhältnis der gewerbmässigen Unzucht und der gewerbmässigen Beförderung
derselben in Leipzig, sowie in Bezug auf das neuerdings eingeführte
Regulativ gewisser polizeilicher Massregeln

erörtert und besprochen

von

Dr. med. Carl Wilhelm Streubel

Professor an der Universität Leipzig und Polizeiarzt.

Leipzig

Verlag von Otto Wigand.

1862.

Die drei Artikel des königlich sächsischen Strafgesetzbuchs, die gewerbmäßige Unzucht, die Beförderung der Unzucht und die Rupperei betreffend.

Die Handhabung dieser Artikel und die Prostitutionsverhältnisse Leipzigs.

Stellung des Staates zur Prostitution.

Das neue Regulativ über die polizeilichen Maaßregeln der Stadt Leipzig zur Beschränkung des Prostitutionswesens und Verhütung der Lustseuche.

Im Jahre 1833 brachte die sächsische Staatsregierung den Entwurf eines neuen Gesetzes „über die Bestrafung der fleischlichen Verbrechen und einiger hiermit in Verbindung stehenden Vergehen“ an die Stände. Die Regierung hatte sich überzeugt, daß die bezüglich der gedachten Verbrechen und Vergehen erlassenen gesetzlichen Bestimmungen im allgemeinen viel zu streng wären und mit den Gesetzen der benachbarten Staaten hier und da in Widerspruch ständen, auch hatte sie mit Mißfallen bemerkt, daß der Gerichtsgebrauch sich überall Milderung und sogar Umgehung der in den bestehenden Gesetzen verhängten Strafen erlaubt hatte, ohne dadurch doch, abgesehen von der nicht zu billigenden Gewalt gegen die Gesetze, deren Mängel wirklich zu verbessern. Die Regierung hielt es daher unter solchen Umständen für gerechtfertigt, ein neues umfassendes Gesetz über die Fleischverbrechen im weitesten Sinne zu beantragen, ohne erst die Ausarbeitung eines neuen, vollständigen Strafgesetzbuchs abzuwarten. Die Stände stimmten den Ansichten der Regierung bei und der Entwurf des neuen Gesetzes wurde den Kammern zur Diskussion vorgelegt. So lebhaft, interessant und ausführlich aber auch die Kammerverhandlungen namentlich über die Nothzucht, die Entführung, den Ehebruch und den Incest waren, so fanden doch gerade diejenigen Paragraphen, welche die Unzucht als Gewerbe und Kuppelei betrafen, keine besondere und tiefer eingehende Erörterung, obgleich diese Vergehen gerade diejenigen bilden, die in den größeren Städten tagtäglich begangen werden. Wie daher diese Paragraphen in dem Gesetzentwurf standen, wurden

sie fast wörtlich angenommen und gelangten auch so in das Strafgesetzbuch des Jahres 1838.

Fragen wir nach den Gründen, welche wohl die Stände bewegen haben mochten, die näheren Auseinandersetzungen über Gewerburerei und Kupperei zu vermeiden, so lassen sich in der That mehrere denken. Zuerst und hauptsächlich fühlten wohl die meisten Mitglieder der Kammern, daß ihnen eine speziellere Kenntniß über den Zustand des Hurenwesens und der Kupperei im Lande abgehe; sie scheuten sich ferner einen Gegenstand öffentlich abzuhandeln, bei dessen ausführlicher Besprechung notwendigerweise schmutzige Details zur Sprache kommen mußten. Die Unsicherheit und das Schwanken in dem Gerichtsverfahren von den ältesten Zeiten her legte ohne Zweifel die Nearbe, es sei besser, bei den gedachten Vergehen nicht zu sehr ins Einzelne zu gehen. Endlich traute sich Niemand gegen die so verbreitete Ansicht, „die Unzucht als Gewerbe und die Kupperei seien Vergehen, die, um der Moral zu genügen, unter allen Umständen bestraft werden müßten“, etwas einzuwenden und die in dem Gesetzentwurfe getroffenen Bestimmungen erschienen beim ersten Anblicke so ausreichend, entsprechend und gelind, daß man sie ohne Weiteres recipirte und sich war, über ein fatales Capitel rasch hinweggekommen zu sein.

Auch in dem neuesten sächsischen Strafgesetzbuche vom Jahre 1855 sind die Artikel über die gewerbmäßige Unzucht, über die Beförderung der Unzucht und über die Kupperei mit geringen, wenig erheblichen Abänderungen und Umstellungen ziemlich dieselben geblieben und sind in dem 17. Capitel, in welchem von den Verletzungen der Sittlichkeit die Rede ist, aufgeführt.

Es ist nicht in Abrede zu stellen und muß rühmend anerkannt werden, daß bei Abfassung des Gesetzes vom 8. Februar 1834 die Humanität vorzüglich berücksichtigt wurde. Wir machen darauf aufmerksam, daß alle hierher gehörigen Verbrechen und Vergehen fast insgesammt mit viel geringeren Strafen als früher belegt wurden, daß bei einigen Vergehen nicht mehr von Amtswegen, sondern nur auf Antrag der dabei in ihren Rechten verletzten Personen eingeschritten werden durfte, und daß selbst mehrere früher gestrafte Vergehen, wie der einfache außereheliche Beischlaf, der Concubinat, das Quasi-Mul-

terium und der Quasi-Incest, aus der Reihe der strafbaren Handlungen ausgeschlossen wurden. Bezüglich der Gewerburerei und Kupperei stellte sich indessen die Humanität der Gesetzgeber weniger heraus. Die anscheinend milde gesetzliche Bestimmung verlor diesen Charakter bei der praktischen Ausübung. Wenn die früheren gesetzlichen Vorschriften die in Rede stehenden Vergehen zwar äußerst hart abmilderten, so wurde doch die Härte in der Praxis fast allgemein verkannt und die richterliche Gewalt erlaubte sich nach ihrem Ermessen und Gutdünken Auslegungen des Gesetzes, die zur Milderung der Strafe, ja selbst zur Freisprechung führten, oder man ignorirte die Vergehen und machte sich möglichst wenig mit ihnen zu schaffen. Es kam früher nur auf den Grad der Freisinnigkeit des Richters an, ob die einzelnen Fälle von Gewerburerei und Kupperei gelind oder gar nicht bestraft wurden. Das Gesetz von 1834 und auch das neueste Strafgesetzbuch von 1855, haben gegen Unzucht als Gewerbe, gegen Beförderung der Unzucht und Kupperei unter allen Umständen Strafe verhängt, und sie lassen nur in so fern eine Milderung zu, als auf das von den festgesetzten Strafen niedrigste Strafmaaß erkannt werden kann, verbieten das Ignoriren der Vergehen und bestimmen das Einschreiten ex officio.

Wir wollen nun versuchen die betreffenden Artikel des Strafgesetzbuchs vom Jahre 1855 zu analysiren, ihre Mängel darzuthun und zu zeigen, daß man bei ihrer Abfassung von irrigen Grundsätzen ausgegangen sei, und daß hieraus in der Praxis ein ziemlich beklagenswerther, unbilliger und fast rechtloser Zustand sich entwickelt habe.

Artikel 354 des Strafgesetzbuchs trägt die Ueberschrift: „Gewerbmäßige Unzucht“, und lautet folgendermaßen: Weibspersonen, welche die Unzucht als Gewerbe betreiben, sind mit Gefängniß von 3 Wochen bis zu 2 Monaten zu bestrafen.

So einfach und verständlich die Fassung des Artikels erscheinen mag, so unendlich und ungenau zeigt sich derselbe, wenn wir bezüglich der praktischen Ausübung nach dem Sinne desselben fragen. Da wir in den Landtagsoverhandlungen und in den Gesetzesinterpretationen nähere Erklärungen nicht gefunden haben, so werden wir zu der Ansicht gedrängt, als habe man die in dem Artikel enthaltenen Bestimmungen über allen Zweifel erhaben gehalten. Die Ueberschrift und

die Fassung des Artikels scheint gewissermaßen voraussetzen, es müsse Jeder den Sinn der Worte sofort begreifen. Ehe wir eine richtige Vorstellung davon haben können, was unter Unzucht als Gewerbe zu verstehen sei, muß und notwendig klar sein, was das Wort „Gewerbe“ und der Ausdruck „ein Gewerbe betreiben“ bedeute. Gleich hier stoßen wir auf eine kaum zu beseitigende Schwierigkeit. Obgleich in allen Gesetzsammlungen der verschiedenen Länder die einzelnen Gewerbe in Bezug auf Berechtigung, Besteuerung u. s. f. möglichst genau aufgeführt, auch gegen gewisse Gewerbe Beschränkungen und Verbote erlassen sind, so hat man doch nirgends eine Definition des Begriffs Gewerbe zu geben sich getraut und wir hegen auch die Ueberzeugung, daß eine solche Begriffsbestimmung zur Zeit schwerlich genau sich festlegen lasse. Im weitesten Sinne des Wortes kann man unter Gewerbe alle Beschäftigungen begreifen, die zum Erwerb dienen. Der Zweck des Gewerbes ist Unterhalt, Verdienst, Erwerb, Gewinn, und um diesen Zweck zu erreichen, unterzieht sich der Gewerbetreibende gewissen Handlungen, Beschäftigungen, Arbeiten, Anstrengungen. In diesem Umfange genommen sind die meisten Menschen als Gewerbetreibende zu betrachten, und es fallen nur diejenigen weg, die von ihrem Gelde leben und diejenigen, die noch nicht, oder nicht mehr, oder überhaupt gar nicht lohnende Beschäftigungen zu treiben im Stande sind. Es sind ferner die Worte „Gewerbe treiben“ nicht bloß auf eine einzige, d. h. einmal des Erwerbs halber unternommene Handlung zu beziehen, sondern der Begriff schließt eine fortgesetzte Reihe von Handlungen in sich. Dieser letztere Begriff, der als wesentlich zu betrachten ist, darf auch nicht verloren gehen, wenn von unerlaubten, tadelhaften und entehrenden behufs des Erwerbs getriebenen Handlungen die Rede ist, und die Strafgesetzbücher der verschiedenen Staaten haben denselben nicht nur angenommen, sondern sie finden darin selbst mit Recht einen Grund zur Schärfung und Erhöhung der Strafen. Das sächsische Strafgesetzbuch belegt demzufolge z. B. den gewerbmäßigen Vertrieb gestohlener Sachen, den gewerbmäßigen betriebenen Wucher u. s. f. mit erhöhter Strafe, und da schon für die einfache Parthiererei und Wucherei niedrigere Strafen bestimmt sind, so ist es wohl nicht zu verkennen, daß der gewerbmäßige Betrieb dieser straf-

baren Handlungen nur auf eine Mehrheit, auf eine Reihe solcher Handlungen bezogen werden kann, die dann natürlich dem einzelnen Falle gegenüber eine Straferhöhung erfahren muß.

Als Weibspersonen, welche die Unzucht als Gewerbe betreiben, können folglich auch nur diejenigen gelten, die erwiesenermaßen für Lohn mehrfach Männern sich preisgegeben haben, d. h. es müssen, um in den vorkommenden Fällen Gewerbschurerei anzunehmen, mehrere constatirte Beispiele vorliegen, welche erweisen, daß der Weischlaf für Lohn von den betreffenden Frauenzimmern vollzogen wurde. Der Artikel will also den einzelnen Fall von Lohnschurerei nicht bestrafen wissen, will nur die gewerbmäßig betriebene Lohnschurerei zur Strafe ziehen. Nun läßt sich aber durchaus nicht begreifen, wie eine tadelnswerthe und unmoralische, aber keineswegs criminalrechtlich strafbare Handlung, wie die Lohnschurerei nach dem Artikel, auf einmal zu einer strafbaren Handlung wird, wenn sie mehrmals ausgeübt worden ist. Ueber diese Inconsequenz ist nicht hinwegzukommen und selbst wenn wir von derselben absehen, so müßten wir wiederum fragen, wie viele Male ein Frauenzimmer dem Weischlafe sich unterzogen haben müßte, ehe man den gewerbmäßigen Betrieb der Unzucht bei ihm annehmen könnte. Hierbei würden wir eine bestimmte Zahl wissen müssen, da dieselbe aber weder in dem betreffenden Artikel, noch anderswärts angegeben ist, so bleibt die Zahlenbestimmung der Beurtheilung des Richters anheimgestellt, der nach Gutdünken eine hohe oder niedrige Zahl wählen und nach Belieben damit wechseln kann.

Mehrere Criminalisten finden, daß ein preussisches Rescript vom 10. Mai 1814 die Frage, „was die Unzucht als Gewerbe betreiben heiße“, richtig so erklärt habe: — daß es dabei nicht darauf ankomme, daß in einzelnen Fällen für Vollziehung des Weischlafs Bezahlung gefordert oder genommen worden, sondern wesentlich sei, daß die Weibsperson sich Jedem ohne Unterschied zur fleischlichen Vermischung gegen Bezahlung hingiebt, was nach den concurrenden Umständen zu beurtheilen, und wobei es in Bezug auf die Existenz des Gewerbes überhaupt gleichgültig ist, wie oft die Schurerei wirklich betrieben worden. — Wir können in dieser gepriesenen Erklärung nur Widersprüche finden. Es soll für den Begriff der Gewerbschurerei wesentlich sein, daß sie sich Jedem

ohne Unterschied für Bezahlung hingiebt und doch soll es in Bezug auf Gewerbestimmung gleichgültig sein, wie oft die Hure wirklich betrieben worden. Können wir wohl im voraus annehmen, daß eine Weibsperson, die einmal oder ein paarmal für Bezahlung sich hat brauchen lassen, deswegen ohne Unterschied einem Zeten, der zahlt, sich preisgeben würde? Wenn wir aber das Zeten ohne Unterschied festhalten wollen, so dürfen wir den größten Theil der Frauenzimmer, die anerkanntermaßen von der Lohnhurei leben, nicht zu den Gewerdbuhren rechnen, weil sie, ohne wählbar zu sein, doch nicht Zeten ohne Unterschied zulassen und immer noch bezüglich des Alters und der körperlichen Eigenschaften eine Wahl unter den Mannspersonen zu treffen pflegen. Will man endlich anführen, daß es Beispiele von Lohnhurei genug gebe, in welchen schon aus dem einzelnen Falle mit ziemlicher Sicherheit auf die getriebene Gewerdbuherei geschlossen werden könne, oder wo die Absicht, die Lohnhurei gewerbmäßig betreiben zu wollen, sich nicht in Abrede stellen lasse, so wollen wir dies gern zugeben, müssen aber bemerken, daß im ersten Falle die concurrirenden Umstände, die den Schluß rechtfertigen sollen, gehörig zu begründen sind, im zweiten Falle können wir aber in keiner Weise beipflichten, da der Wille etwas auszuführen, der Versuch und die wirkliche Vollbringung stets zu sondern sind.

Die sächsischen Richter scheinen aber bei der Unzucht als Gewerbe von dem Begriffe des gewerbmäßigen Betriebs fast ganz ab und wir haben uns durch die Einsicht in die Acten überzeugt, daß sehr häufig nur der Einzelfall von Lohnhurei als Gewerdbuherei geahndet wird. Wir wollen keineswegs den Richtern Inhumanität, ja nicht einmal zu große Strenge vorwerfen, wir geben sogar zu, daß den meisten der verurtheilten Frauenpersonen insofern kein Unrecht geschehen ist, weil dieselben, obgleich nur ein Fall von Lohnhurei constatirt war, doch insgesammt wirkliche Gewerdbuhren waren, allein wir müssen tadeln, daß man eben jene Umstände, welche in den betreffenden Fällen die Gränzen der Gewerdbuherei in klarem Licht gestellt hätten, gar nicht weiter verfolgt hat. Wir finden in den Acten bemerkt, daß die der Lohnhurei in einem Falle überwiesene Frauenperson in verdächtigen Wirtschaften aus- und eingegangen sei, daß sie in einem verdächtigen

Hause gewohnt, mit anderen verdächtigen Frauenzimmern verkehrt habe und zusammengekommen sei u. s. w.; allein alle diese Angaben, die bei weiterer Untersuchung dazu geführt haben würden, daß man außer dem Beweise für den Betrieb der Unzucht als Gewerbe, auch den Beweis für die Gränzen wirklicher Bortelle und für den Betrieb der gewerbmäßigen Unzucht bei einer ganzen Menge von Frauenpersonen gefunden hätte, verfolgen die Richter nicht gern weiter und hüten sich möglichst den vor Augen liegenden Schlamm aufzurühren. Wir müssen auch noch zu bedenken geben, daß das gebräuchliche richterliche Verfahren auch wohl einmal zu ganz auffallender Ungerechtigkeit Veranlassung geben kann, wenn man eben einen in der That unzweifelhaft einzigen Fall von Lohnhurei als Gewerdbuherei betrachtet.

In dem Strafgesetzbuche von 1838 war dem Artikel über die Gewerdbuherei als zweiter Theil die Bestimmung beigefügt: ist eine solche Weibsperson zur Zeit des Beischlafs wesentlich mit der Lustfeude behaftet gewesen, so findet sechsmonatliche bis einjährige Arbeitsstrafe statt. In dem Strafgesetzbuche von 1855 hat man die wesentliche Ansetzung in einem besondern Artikel abgehandelt und ausgebreitete Bestimmungen getroffen. Der Artikel 359 hat die Ueberschrift „Ansetzung“ und lautet: Ist bei Ausübung des Beischlafs oder widernatürlicher Unzucht der eine Theil wesentlich mit der Lustfeude behaftet gewesen, und dadurch eine Ansetzung herbeigeführt worden, so wird er mit Gefängnis von 2 bis 4 Monaten oder Arbeitsstrafe bis zu 1 Jahre bestraft.

Das Gesetz macht also bei der Gewerdbuherei noch zwei Fälle namhaft, in welchen Erhöhung der Strafe eintritt: 1) bei Rückfälligkeit, und 2) wenn die Weibsperson bei Ausübung des Beischlafs wesentlich mit der Lustfeude behaftet war.

Ueber die Bestrafung der Rückfälligkeit (cf. Art. 85) läßt sich nichts weiter sagen und wir verweisen nur auf die bereits erhobenen Bedenken.

Anderes steht es mit der zweiten Bestimmung. Das Strafgesetzbuch von 1855 ist insofern gerechter, als es nicht bloß die Gewerdbuherei, sondern Zeten straft, der wesentlich mit Lustfeude behaftet den

Beischlaf vollzieht und ansteckt, allein die größere Gerechtigkeit hat hier, wie weitere Erörterungen darthun werden, keinen Nutzen.

Ghe wir weiter gehen, können wir nicht unterlassen einige scharfsinnige Interpretationen sächsischer Criminalisten anzuführen, von welchen wir hoffen, daß sie nie Anklang finden werden. Sie sagen: „Da in der Beischlafsgeltung von Seiten der Gewerbhure, die wesentlich mit der Lustseuche behaftet ist, nicht nur ein großer Leichtsinn, sondern selbst Niederträchtigkeit liegt, so muß das Gesetz strenger strafen, und die erhöhte Strafe noch dann vollziehen, wenn selbst der Leichtsinn und die Niederträchtigkeit keinen Schaden getan haben, d. h. wenn die Mannsperlen durch den nur einen Beischlaf nicht angesteckt wurde. Als ein Ausnahmefall ist zu betrachten, wenn ein Freudenmädchen, die mit einem Manne ein wirkliches Liebesverhältnis unterhält, obgleich wesentlich mit Lustseuche behaftet ohne Lohn zum Beischlase sich hingiebt; im Falle der Nichtansteckung würde sie dann als ganz straflos, im Falle der Ansteckung nur nach den Bestimmungen wegen Körper- und Gesundheitsverletzungen für strafbar zu erachten sein.“ — Würde es nicht im ersten Falle sehr hart, ja ungerecht sein, eine Ansteckung zu strafen, die gar nicht bewirkt worden ist und welche nur möglich war? Würde aber nicht im zweiten Falle Straflosigkeit oder eine mildere Strafe als das Gesetz vorschreibt ganz widerständig sein, wenn wir bedenken, daß die wesentlich mit der Lustseuche behaftete Person sogar die Gesundheit des Mannes auf's Spiel setzt, der ihrem Herzen am nächsten stand?

Doch es sei genug mit diesen Proben des Scharfsinns. Wir können überhaupt keinen Grund finden, warum die wesentliche Ansteckung durch Lustseuche anders beurtheilt werden soll, als andere Verletzungen der Gesundheit, warum sie schärfer gestraft werden soll. Nur bei Ansteckung aus Bosheit und Rache könnte billigerweise von Straferhöhung die Rede sein. Was würde man uns antworten, wenn wir folgende zwei Fälle zur Entscheidung vorlegen: Kann eine Person gestraft werden, wenn sie den Concubenten von ihrer Krankheit vorher in Kenntniß gesetzt hat und dieser den Beischlaf dennoch vollzieht und sich inficirt? Kann ein Mann oder ein Weib, wenn es mit der Lustseuche wesentlich behaftet ist, gestraft werden, wenn der Beischlaf

mittels des Condoms vollzogen wurde, der Condom aber in Folge von Zerreißung oder sonst schlechter Beschaffenheit nicht schützte und Ansteckung eintrat?

Die Gerechtigkeit, die Art. 359 insofern übt, als er Jedem, der wesentlich Ansteckung vermittelt, straft, verliert an Bedeutung, wenn wir sehen, daß sie nur nach einer Seite hin gerichtet ist. Wenn ein Mann von einer Gewerbhure angesteckt Anzeige macht, und es stellt sich heraus, daß dieselbe beim Beischlaf wesentlich mit Lustseuche behaftet war, so wird die Gewerbhure nach dem Artikel gestraft; wenn aber umgekehrt die Hure von einem wesentlich mit Lustseuche behafteten Manne angesteckt wurde, so wird der Mann zwar ebenfalls gestraft, allein die Hure geht nicht straflos aus, sondern wird, nachdem sie hergestellt worden, wegen Lohn- und Gewerbdunngucht gestraft. Der Artikel 354 hindert, daß der Artikel 359 gerecht sein könne, denn der Gewerbhure wird es nimmer einfallen, sich Gerechtigkeit holen zu wollen.

Die Krankheit, um welche es sich in Art. 359 handelt, scheint von den Gesetzgebern wenig gekannt zu sein, sie scheinen sich auch über dieselbe von Sachverständigen irgend welchen Rath nicht erholt zu haben, sonst würden sie den Artikel schwerlich in der vorliegenden Weise gefaßt haben. Der Name Lustseuche, der nicht einmal mehr bei dem Volke gebräuchlich ist, muß deswegen als falsch und unpassend verworfen werden, weil die in Rede stehende Krankheit seit der großen Epidemie zu Ende des 15. Jahrhunderts, der sie ihr Entstehen verdanken soll, den Charakter einer Seuche fast ganz eingebüßt hat. Jetzt wird die Krankheit allgemein mit dem Namen Syphilis, Venerie bezeichnet. Obgleich es nicht in unserer Absicht liegt, die Syphilis hier ausführlicher zu besprechen, so müssen wir doch, um unsre Behauptung zu erweisen, auf eine Charakteristik derselben, die sie in ihrem Wesen schildert, kürzlich eingehen.

Das syphilitische, venerische Gift erzeugt an den Stellen, wo es eingimpft wird oder sich einimpft (gewöhnlich an den Geschlechtstheilen durch Beischlaf) Bläschen, Pusteln, die in Geschwüre von mehr oder weniger charakteristischer Form übergehen. Diese Geschwüre werden mit dem Namen der primären syphilitischen Geschwüre, der sogen-

nannten Schanfergeschwüre bezeichnet, sie sondern Eiter (den Contagium-träger) ab, vergrößern sich bei sonst gutem Verlaufe eine Zeit lang, bleiben dann eine Weile stationär und fangen endlich an zu vernarben, nachdem sie 1 bis 2 Monate bestanden haben und die Krankheit ist beendet.

In vielen Fällen sind nun diese primären Geschwüre leider nicht die einzigen Erscheinungen der Syphilis; durch schlechtes Verhalten des Kranken, durch Schwächlichkeit und Kränklichkeit desselben, verzögert sich der Verlauf, das Gist des Geschwürs dringt durch Resorption in's Blut, es entsteht eine Blutvergiftung und nun brechen nach längerer oder kürzerer Zeit, oft nach anscheinender Heilung, eisernt von der früheren Infektionsstelle verschiedene Ausschläge, Geschwülste und Geschwüre hervor, die sich zuweilen über den ganzen Körper verbreiten, zuletzt selbst die Knochen befallen. Sowie die Zeichen der Blutvergiftung hervortreten, so ist die primäre locale Syphilis in die secundäre constitutionelle Syphilis übergegangen. Die Syphilis verliert dadurch, daß sie in das Blut dringt, constitutionell wird, beträchtlich an Ansteckungsfähigkeit, so daß secundäre syphilitische Ulcerationen in der Regel nicht impfbar sind und nur sehr selten einmal Ansteckung veranlassen. Die warzenähnlichen Wucherungen an den Geschlechtstheilen, die sogenannten Condylome, scheinen manchmal mit Syphilis zusammenzuhängen, selbst syphilitischer Natur zu sein, in vielen Fällen sind sie aber von Syphilis ganz unabhängig und ihre Ansteckungsfähigkeit ist stets sehr beschränkt. Die älteren Aerzte und selbst einige Neuere rechnen zu der Syphilis fälschlicherweise noch jene satarethaischen, blennorrhoeischen Schleimhautentzündungen an den Geschlechtstheilen, welche spontan entstehen können und Ausflüsse bedingen, die allerdings, namentlich beim Weichlaß anstecken, und die sogenannten Tripper, Gonorrhoeen veranlassen. Diese Ausflüsse, die mit syphilitischen Geschwüren und Affectionen zusammen vorkommen können, inficiren nie das Blut, bleiben stets auf die Schleimhaut beschränkt. Es muß noch hervorgehoben werden, daß die syphilitischen primären und secundären Affectionen oft unter so verschiedenen Formen, mit so geringen und räuschernden Erscheinungen auftreten, daß selbst ein sehr erfahrener Arzt längere Zeit braucht, um sich von der Existenz der Syphilis zu überzeugen.

Keihen wir nach diesem kurzen und gedrängten Abrisse zu Artikel 359 zurück. Da in allen gerichtlichen Fällen, wo Syphilis in's Spiel kommt, dieselbe erst durch ärztliche Untersuchung constatirt werden muß, so kann der Gerichtsarzt, da das Gesetz nicht anzeigt, was unter Lustseuche zu verstehen sei, sich jede beliebige Erklärung von derselben bilden und wenn diese auch noch so obscur sein sollte. Der Gerichtsarzt z. B., der den älteren Ansichten anhängend, Tripperentzündungen, Ausflüsse und Erosionen mit zur Lustseuche rechnet, kann durch sein Urtheil jedem Mädchen, die sich einmal preisgegeben hat und gestehen muß, daß sie schon seit Jahren am weissen Fluße gelitten habe, zu harter und ungerechter Strafe verhelfen. Da es ferner Thatsache ist, daß nur primäre Syphilis, secundäre aber fast gar nicht ansteckt, so könnte eigentlich nur dann Strafe stattfinden, wenn Jemand gewußt hat, daß er primäre syphilitische Geschwüre bei der Weichlaßvollziehung gehabt habe. Wie kann man aber verlangen, daß Jeder, wenn er etwas an seinen Geschlechtstheilen, wenn er Geschwüre merkt, wissen solle, ob dieselben primär syphilitisch, ansteckend, oder secundär nicht ansteckend seien, ob die Geschwüre überhaupt zur Syphilis gehören, da doch den Gesetzgebern bei Fassung des Artikels 359 solche Reminiscenzen abgegangen ist?

Es ließe sich zwar bezüglich der Lohnhuren einwenden, wenn einer solchen Dirne schon von mehreren Männern der Vorwurf der Ansteckung gemacht worden ist, könne die Beschuldigung sich wesentlich syphilitisch preisgegeben zu haben, nicht weggelängnet werden, allein jeder Gerichts- und Polizeiarzt weiß, wie oft Dirnen als syphilitisch krank angezeigt werden, die bei der genauesten Untersuchung völlig gesund erscheinen; etwas leucorrhoeische Euren theilen den concubirenden Männern leicht Tripper mit, sind aber ganz frei von Syphilis; endlich sind primäre syphilitische Geschwüre bei Frauenzimmern oft so klein, liegen so versteckt und verursachen der Kranken gar keine Beschwerden, so daß sie erst nach mehrfachen Untersuchungen von Aerzten wahrgenommen werden. Die damit behafteten Weibspersonen, die ihre Krankheit nicht ahnen, schenken daher den Vorwürfen der Mannspersonen, die von ihnen angesteckt zu sein angeben, gar keinen Glauben.

Wir gehen nun zur Betrachtung des Artikel 355 über. Der Artikel hat die Ueberschrift „Beförderung der Unzucht“, er lautet: Wer dergleichen Personen (nämlich Weibspersonen, welche die Unzucht als Gewerbe treiben) anderen zuführt, oder ihnen das unzuchtige Gewerbe in seiner Wohnung gestattet, hat Gefängnißstrafe bis zu 2 Monaten verwirkt. Arbeitshausstrafe bis zu 1 Jahre tritt ein, wenn die Weibsperson mit der Lustkeuse behaftet gewesen ist, oder die Beförderung der Unzucht gewerbmäßig betrieben wird.

Dieser Artikel ist fast ebenso gefaßt wie im Strafgesetzbuche vom Jahre 1838, nur daß er eine andere Ueberschrift bekommen hat, denn im Strafgesetzbuche von 1838 ist der Artikel mit dem Worte „Kuppelei“ überschrieben. Das Gesetzbuch von 1855 unterscheidet Beförderung der Unzucht und Kuppelei und bezieht die Beförderung der Unzucht nur auf Weibspersonen, welche die Unzucht als Gewerbe betreiben, während bei der Kuppelei Verleitung unbescholtener Frauenspersonen, Kinder, Verwandter u. s. w. zur Unzucht vorausgesetzt wird.

Im weitesten Sinne des Wortes können wir unter Kuppelei alle nicht gewaltsame Theilnahme, Hilfsleistung, Beförderung und Begünstigung, die Jemand an der Unzucht eines Dritten nimmt, verstehen. Die Kuppelei besteht im Gelegenheitsmachen zur Unzucht und es ist für den Begriff und Thatbestand der Kuppelei einerlei, ob dieses Gelegenheitsmachen um des Gewinnstes willen, oder aus irgend welchen Gründen geschah, ob es in positiven Handlungen, oder in einem negativen Verhalten (Zusehen, Gesehenlassen) bestand. Können wir die Kuppelei in diesem weitesten Sinne auf, so ist es ersichtlich, daß sie in unzähligen gradweisen Abstufungen vorkommen kann und daß sie zwischen den Grenzpunkten eines leichten Vergehens und eines schweblichen Verbrechens alle Zwischenstufen zu durchlaufen vermag.

Das Gesetz würde hart und ungerecht sein, wenn es alle Fälle von Kuppelei im weitesten Sinne bestrafen wollte, es darf aber auch gewisse Fälle von Kuppelei durchaus nicht straflos dahingehen lassen und so kommt es denn, daß die Gesetzgeber der verschiedenen Länder nur bestimmte Fälle von Kuppelei hervorheben, in welchen sie die Bedingungen strafbarer Handlungen vorzufinden glauben. Hierbei

kommen nun Fälle vor, in welchen die Kuppelei nicht nach den allgemeinen Grundsätzen über Theilnahme an Verbrechen bestraft wird, sondern wo die Begünstiger, Helfer, Urheber u. besonders gestraft werden.

Das Strafgesetzbuch von 1855 trennt die Beförderung der Unzucht von der Kuppelei; das Verbrechen der Beförderung der Unzucht kann nur in Bezug auf Gewerbschuren verübt werden, denn bei solchen kann nicht von Kuppelei die Rede sein, weil sie weder unbescholtene Personen sind, noch bei ihnen ein Verleiten zur Unzucht stattfinden wird. Als ausgezeichnete Fälle, die mit erhöhter Strafe belegt werden sollen, hebt der Artikel hervor: 1) wenn die Weibsperson mit Lustkeuse behaftet war und 2) wenn die Beförderung der Unzucht gewerbmäßig betrieben wurde.

Der Unterschied, den das Strafgesetzbuch zwischen Beförderung der Unzucht und Kuppelei macht, will uns nicht recht einleuchten. Die Beförderung der Unzucht wie sie Art. 355 beschreibt, kommt viel häufiger vor, als die Kuppelei, die der Art. 356 angiebt, ja wir können leider sagen, daß der gewerbmäßige Betrieb der Beförderung der Unzucht in zahlreichen Etablissements tagtäglich und schwunghaft vor unseren Augen betrieben wird, während nur seltener einmal ein Fall von Kuppelei nach Art. 356 zu unseren Ohren dringt. Wir sind seit Alters her gewohnt, die Beförderung der Unzucht durch einen Dritten als Kuppelei zu betrachten, mag das verkuppelte Frauenzimmer ein unbescholtene oder übelberühmte sein, und beziehen daher mit Recht das alte, passende und gebräuchliche Wort Kuppelei auf die am gewöhnlichsten vorkommende Art der Beförderung der Unzucht. Wir können uns auch nicht entschließen, jene Kuppeler und Kuppelerinnen, die wir so oft sehen müssen, künftighin nach Art. 355 nur als Beförderer und Beförderinnen der Unzucht zu betrachten, zumal da wir in diesen Persönlichkeiten Prototypen von Kuppelern und Kuppelerinnen gefunden zu haben uns überzeugt halten.

Wie sehr dem Art. 355 daran liegt, die Beförderung der Unzucht zu verpönen, geht daraus hervor, daß schon die geringste Beförderung, die oft nur aus Leichtsinne begangen ist, als strafbar geahndet wird, daß Personen der Beförderung der Unzucht bezüchtigt und bestraft

werden, die auch nicht die entfernteste Idee hatten, daß sie ein criminal, rechtlich strafbares Vergehen begangen haben.

Obgleich Jedem einleuchten muß, daß das Zuführen einer Gewerbhure und Weischlafgeßatten in der Wohnung auf die verschiedenste Art und Weise und unter den verschiedensten Umständen erfolgen kann, so daß dabei der Begriff der Beförderung der Unzucht manchmal fast ganz verwischt wird, so nimmt doch der Art. 355 hierauf keine Rücksicht und bringt alle Fälle unter das Strafmaaß von 3 Wochen bis zu 2 Monaten Gefängniß. Wenn Jemand einem Anderen eine Gewerbhure zugeführt, oder ihm den Weischlaf in seiner Wohnung gestattet hat, so fragt es sich, wie und in welcher Absicht dies geschah. In den meisten Fällen wird Gewinnsucht zu Grunde gelegen haben, die kuppelnde Person läßt sich das Zuführen von der Mannsperſon oder auch von der Hure bezahlen, oder sie überläßt gleichfalls für Geld ihre Wohnung der Hetäre und deren Galan zur Vollziehung der Unzucht. Daß solche Handlungen, bei welchen der Aneculpat aus dem Stillkeitsvergehen Anderer Nutzen zieht und diese des Gewinnes halber befördert, zuläßt und zu Stande bringt, mit Recht als schmutzige bezeichnet und mit Strafe belegt werden, scheint gewiß ganz in der Ordnung. Aber nicht selten führt Jemand weder aus Gewinnsucht, noch des Vortheils halber, noch aus irgend einer böswilligen Absicht einem Anderen eine Lohnhure zu und gestattet den Weischlaf in seiner Wohnung, und das Gesetz verfällt dadurch, daß es diese Fälle von den ersten genannten nicht sondert, in Härte. Wenn ein Herr seinem Diener befehlt, er solle ihm diese oder jene Gewerbhure holen, oder wenn zwei befreundete Männer, die beide oft der papistischen Göttern opfern, sich gegenseitig Dinen zuführen, oder einander ihre Zimmer leihen, so würde nach Artikel 355 Beförderung der Unzucht in solchen Fällen vorliegen. Beispiele der Art könnten wir zu Dugenten aufstellen, und wenn sie in der Praxis glücklicherweise nur selten einmal zur Bestrafung kommen, so ist es schlimm genug, daß sie überhaupt im Bereiche der Strafe liegen. Daß derartige Straffälle indessen vorgekommen sind, ist uns aus zwei Beispielen bekannt; in dem einen Beispiele hatte ein Lohnbedienter auf Verlangen einen Fremden in ein Bordell geführt; in dem andern Beispiele hatte ein Bürger aus

unüberlegter Gefälligkeit einem Auswärtigen eine Lustbude auf Verlangen zugeschildet.

Es muß wohl darauf gesehen werden, ob die der Beförderung der Unzucht beschuldigte Person für einen bestimmten ausbedungenen Lohn, oder in Erwartung einer Belohnung, eines Vortheils, oder ohne irgend einen Gewinn oder Vortheil erlangen zu wollen, nur aus Gefälligkeit ihre Dienste beim Zuführen u. s. w. geleistet habe. Es ist ferner zu beachten, in welchen Verhältnissen die bei dem Vergehen theilhaftigen Personen zu einander standen und ob die Beförderungsdienste angeboten oder aufgefordert geleistet wurden. Diener und Dienerrinnen, überhaupt abhängige Personen, die von ihrer Herrschaft, ihren Principalen aufgefordert worden sind, Gewerbsunzucht durch Zuführen, Zimmerüberlassen zu befördern, sind gewiß nicht nach Artikel 355 zu beurtheilen, ebensowenig Freunde und Bekannte, die sich nach dieser Richtung hin unterstügen und sich gegenseitig ausbilden, während Vertraute der Gewerbhuren, welche die Zuführer machen, oder Zimmer und Lager bereit halten, oder Personen, welche Fremden ihre Hilfsleistungen anbieten und gewähren, die Schuld der Beförderung sicherlich auf sich laden.

Als ausgezeichneten Fall, in welchem Arbeitshausstrafe bis zu 1 Jahre eintritt, hebt der Art. 355 den hervor, wenn die Weischlafperson mit der Lustseuche behaftet gewesen ist.

Hier stoßen wir schon wieder auf das unglückliche Wort Lustseuche, dessen vagen Begriff wir oben schon satzsam gerügt haben. Da kein Unterschied gemacht wird, ob die Beförderer der Unzucht, ehe sie eine Gewerbhure zuführten, von der Krankheit derselben Kenntniß hatten oder nicht, ob sie also wissentlich oder unwissentlich eine syphilitische Hure verdingten, so ersieht man, daß gegen die Beförderer eine besondere Strenge geltend gemacht werden soll. Bei den händischen Verhandlungen 1834 über die vorliegende gesetzliche Bestimmung, fühlte zwar die Deputation die Härte derselben und schlug vor, die erhöhte Strafe nur auf die Fälle zu beschränken, wo nachweislich das Kranksein der Gewerbhure dem Unzuchtsbeförderer bekannt gewesen sei, allein dieser Vorschlag wurde verworfen, indem hervorgehoben wurde, daß deswegen kein Unterschied gemacht werden dürfe, weil in solchen

Fällen die kuppelnden Personen durch ihre Sorglosigkeit auch noch die Gesundheit Anderer gefährdeten. Da die Syphilis weder auf der Hand liegt, noch, erquisite einzelne Fälle ausgenommen, an der Nase angesehen werden kann, so müßten die die Verwerbunzsucht befördernden Subjecte jedenfalls, ehe sie eine Werdbhure zuführen, dieselbe untersuchen, oder untersuchen lassen; im ersten Falle werden ärztliche Kenntnisse vorausgesetzt, im zweiten Falle müßten sich die Beförderer auf das Urtheil des Untersuchenden verlassen. Da nun schon oben angedeutet worden ist, daß syphilitische Geschwüre bei Frauenpersonen häufig sehr versteckt liegen, daß sie selbst von den geschicktesten Ärzten leicht übersehen, oder erst nach mehrfachen Untersuchungen gefunden werden, so können die befördernden Subjecte trotz der größten Sorgfalt und trotz ärztlicher Beihülfe doch nicht vermeiden, einmal eine syphilitisch kranke Werdbhure zuzuführen, und man sieht, daß ihnen rein etwas Unmögliches zugemuthet wird. Die gesetzliche Bestimmung ist aber nicht einmal bloß gegen die für Lohn, Vortheil oder aus böswilliger Absicht geleisteten Beförderungsdienste gerichtet, sondern sie bezieht sich auf alle Fälle, welche nach dem Artikel liegen welche Beförderung der Unzucht erkennen lassen: Personen, welche aufgefordert, aus Gefälligkeit, ohne Nutzen oder Aussicht auf Gewinn eine Werdbhure zuführen, oder den Beischlaf mit derselben in ihrer Wohnung gestatten, können, wenn die Weibsperson unglücklichweise syphilitisch war, Arbeitshausstrafe erhalten. —

Als zweiten ausgezeichneten Fall von Beförderung der Unzucht markirt Art. 355 den gewerbmäßigen Betrieb der Beförderung und hier trifft die erhöhte Strafe, sobald sie bei Personen, welche Absteigerquartiere oder gar Bordelle halten, in Anwendung gebracht wird, in der Regel hinlänglich strafwürdige und nichtsnutzige Subjecte. —

Artikel 356 mit der Ueberschrift „Kuppelerei“ ist folgendermaßen gefaßt: Wer unbescholtene Frauenszimmer zur Unzucht mit Anderen verleitet, wird mit Gefängniß von 3 Monaten bis zu 1 Jahre bestraft. Ist dieses Verbrechen an Kindern unter vierzehn, jedoch über zwölf Jahren, der eignen Ehefrau, Verwandten oder Verschwägerten in absteigender Linie, Pflege- oder Wahlkindern, Geschwistern, dem eignen Mündel, oder zur Erziehung anvertrauten Personen, oder von

gewerbmäßigen Beförderern der Unzucht begangen worden, so findet Arbeitshaus bis zu 4 Jahren statt.

Wir werden diesen Artikel nur einer kurzen Kritik unterwerfen, da eine ausführliche Besprechung desselben einmal unsre Kräfte übersteigen, andererseits weniger zum Zwecke unsrer Arbeit gehören würde, doch wollen wir kurzlich darthun, daß auch die Fassung dieses Artikels sehr viel zu wünschen übrig läßt, daß die mangelhafte Fassung in praxi ebenso leicht zu Härte und Ungerechtigkeiten, als zu unverantwortlicher Nachsicht Veranlassung geben kann.

Fragen wir zuerst, was der Artikel unter unbescholtenen Frauenzimmern versteht, so müssen wir, da alle Erläuterungen mangeln, den Sprachgebrauch zu Rathe ziehen. Im Allgemeinen kann man Jeden, der sich zur Zeit nichts, oder nur wenig Erhebliches hat zu Schulden kommen lassen, unbescholten nennen. Das Unbescholtensein würde dann dem Ausdruck „in gutem Rufe stehen“ sich nähern. Leider wird uns eine derartige Definition hier wenig befriedigen, da das Unbescholtensein im Gegensatz zur Unzucht gebraucht ist. Eine unbescholtene Frauensperson würde daher mit einem keuschen oder als keusch geltenden Frauenzimmer identisch sein. In früheren Zeiten brauchte man das Wort unverleumdete, und eine Frauensperson, von welcher es bekannt war, daß sie sich außerehelich einem Manne hingegeben habe, wurde nie als unverleumdete betrachtet. Warum hat der Artikel nicht keusche oder unverleumdete Frauenspersonen verlangt? Es bleibt unter solchen Umständen die Beurtheilung der Unbescholtetheit in den einzelnen Fällen den Richtern überlassen.

Nicht besser steht es damit, was es heißen solle: „zur Unzucht mit Anderen verleiten.“ Gewalt ist beim Verleiten jedenfalls ausgeschlossen, ob dieses indessen auch von Drohungen gelte, dürfte sich noch bezweifeln lassen. Das Verleiten wird wohl meist durch Zureden, durch Versprechungen, Anerbietungen, Geschenke, Geld u. s. w. zu Stande gebracht, es wird dadurch die sogenannte unbescholtene Frauensperson mehr oder weniger leicht bewogen, sich zur Unzucht, die sie vorher nicht zu begehren beabsichtigte, dennoch zu verfehen. Ob nun die der Kuppelerei beschuldigte Person aus gewinnstüchtiger oder böswilliger Absicht, ohne Lohn, aus Gefälligkeit gegen einen Dritten die Verlei-

tung unternommen habe, ob die Verleitung nach Aufforderung oder unaufgefordert aus eigenem Antriebe geschehen sei, darauf nimmt der Artikel keine Rücksicht, sondern hält nur das Verleiten der unbefohlenen Frauenperson fest.

Wollten wir Beispiele anführen, in welchen das stattgehabte Verleiten nur als ein sehr geringes Vergehen angesehen werden könnte, so würden wir eine ganze Reihe vorbringen können, wir bemerken nur, daß das Verleiten, wenn es etwas Leichtes war, wenn die unbefohlene Frauenperson nach geringem Zureden, nach kleinen Geschenken u. s. w., also ohne daß besondere Künste nothwendig waren, zur Unzucht sich bewegen ließ, wenn das Zureden weder aus böswilliger Absicht, noch für Lohn geschah, häufig eine Gefängnißstrafe von 3 Monaten mindestens wohl kaum als gerecht anerkannt werden kann.

Dadurch aber, daß der Artikel ein Verleiten zur Unzucht fordert, also ein actives Verfahren und außerdem noch unbefohlene Frauenpersonen voraussetzt, geräth er mit früheren Bestimmungen und mit den Ansichten anderer Strafgesetzbücher in Widerspruch. Durch Zusehen, Nichtsehenwollen, kurz durch ein passives Verhalten kann also eine Kuppelerei selbst bei unbefohlenen Frauenzimmern nicht begangen werden; die Eltern können ruhig zusehen, wie ihre Töchter von Männern zur Unzucht verlockt werden, sich preisgeben, es kann ihnen keine Kuppelerei zur Last gelegt werden, selbst wenn sie Vortheil von der gestifteten Unzucht der Tochter haben, nur dürfen sie nicht geradezu activ eingreifen. Der Ehemann kann es geschehen lassen, daß seine Frau zur Unzucht verleitet wird, er kann ohne strafbar zu werden selbst Schweigegeld annehmen. Da selbst thätiger Eingriff bei der Unzucht, ein Verleiten kann straflos verübt werden, wenn die Frauenperson nicht unbefohlen ist, nur darf sie keine Gewerbhure sein. Man kann Frauenpersonen, die gehändig sind, Unzucht getrieben zu haben, die aber weder Lohn noch Gewerbhuren sind, zuführen, ihnen mit einem Manne in der eignen Wohnung den Weischoß gestatten, kann Vortheil davon haben und begeht weder das Verbrechen der Kuppelerei, noch das Vergehen der Beförderung der Unzucht.

Wir sehen, daß es Fälle giebt, die nicht nur nach dem Sprachgebrauche des Volkes, sondern auch in der Wirklichkeit zu der schmach-

vollsten und niederträchtigsten Kuppelerei gehören und die dennoch der Artikel 356 nicht so betrachtet, die, da sie auch anderwärts in dem Strafgesetzbuche keine Erwähnung finden, demnach als straflos gelten müssen, obgleich sie aller Eitte Hohn sprechen.

Im 2. Sage des 356. Artikels sind die Fälle von Kuppelerei namhaft gemacht, wo das Verbrechen an Kindern unter vierzehn Jahren, an der eignen Ehefrau, Verwandten u. s. f. begangen worden ist und wo eine Arbeitshausstrafe bis zu 4 Jahren stattfinden soll.

Auch bei diesem zweiten Sage könnten wir Vieles von dem bereits Gesagten wiederholen, wir könnten auch neue Bedenken und Mißbilligungen erheben; wir unterlassen es auch, den Artikel 358, die Vollendung der fleischlichen Verbrechen betreffend, zu besprechen, und richten nun unsere Aufmerksamkeit auf die Handhabung der Artikel 354 und 355, d. h. auf die Betrachtung der Art und Weise, wie diese Artikel zur Geltung gebracht werden, wobei wir namentlich Leipzig in's Auge fassen.

Es ist bekannt und Thatsache, daß in allen größeren Städten Frauenzimmer sich vorfinden, die sich für Lohn preisgeben, die aus der Unzucht ein Gewerbe machen. Trotz der größten Zwangsmassregeln kann keine größere Stadt sich solcher Frauenzimmer entledigen. Die Entwicklung der Gründe, warum dies so ist und sein wird, gehört nicht hierher. Leipzig hat als eine sehr bevölkerte Stadt natürlicherweise auch ihren bestimmten Theil von Lohn- und Gewerbhuren, ja es treten bei dieser Stadt Umstände hinzu, welche den Betrieb der Lohn- und Gewerbhurerei härter machen, als bei anderen ebenso bevölkerten Städten. Diese begünstigenden Umstände werden hauptsächlich durch die Stellung Leipzigs als Handels- und Meßstadt hervorgerufen. Dreimal jährlich strömen Tausende von Verkäufern nach Leipzig, die eine noch viel größere Anzahl von Käufern nach sich ziehen. Die Verkäufer wie die Einkäufer bleiben oft mehrere Wochen lang in Leipzig, und da sie bei ihren Geschäften, namentlich die Letzteren, freie Zeit übrig haben, so verwenden sie dieselbe, sich auf verschiedene Weise zu vergnügen, wozu es denn auch gehört, daß sie feile Dirnen zum Geschlechtsgenusse aufsuchen. Rechnen wir nun noch dazu, daß viele

Männer lediglich um sich zu belustigen nach Leipzig zur Messe kommen, daß außer den Messen durch Universität, Garnison, durch Handels- und Handwerkerstand, endlich durch Fabrikarbeiter ein großes Contingent geschlechtsreifer Männer gegeben wird, von welchem ein ziemlicher Theil kein moralisches Bedenken trägt sich für Geld Geschlechtsgegnen zu verschaffen, so begreifen wir ebensowohl, daß die Nachfrage nach käuflichen Dirnen überhaupt nicht unbeträchtlich ist, als daß diese in den Messen sich noch steigert.

Wir haben in Leipzig fast alle Klassen von Huhldirnen, wie sie in London, Paris, Berlin und anderen größeren Städten existiren, und das Geschäft der Lohnhure wird in allen Abstufungen von der größten Offenlichkeit bis zur größten Geheimhaltung betrieben.

Wir besitzen zuerst galante Frauen (*Entretenues*), die von einem oder einigen vermögenden Herren unterhalten werden; sie besitzen gut eingerichtete Wohnungen, halten sich selbst Dienstmädchen, haben das Aeußere einer feineren Dame und wissen sich manchmal ganz gut zu benehmen. Diese Frauenszimmer, deren Anzahl gering ist, denn wir sind in der Cultur doch noch nicht so weit fortgeschritten wie in Paris, besuchen nur das Theater und einige bessere Vergnügungsorte, sie halten sich ziemlich zurückgezogen, allein sie sind doch nichts anderes als Lohnhuren, da sie ihren ganzen Lebensunterhalt der Wollust verdanken; sie sind ihren Unterhaltern, die da meinen etwas allein zu besitzen, wohl nur selten ganz treu, sie sind aber nicht leicht zu haben, ergeben sich erst nach längerer Bekanntschaft und setzen einen ziemlichen Preis auf ihre Gunft.

Die größte Klasse der Lohnhuren bilden diejenigen Frauenzimmer, die als Näherinnen, Putzmacherinnen, Fabrikarbeiterinnen, Tänzerinnen, Dienstmädchen, noch eine wirkliche Beschäftigung treiben, aber den Abend oder die sonst freie Zeit dazu benutzen durch Preisgebung Geld zu verdienen. Viele von diesen machen die Lohnhuren nur unter der Hand und bei vorkommenden Gelegenheiten, andere beschäftigen sich ernstlicher damit. Diese Frauenspersonen tragen bald die Kleidung der Diensthörsen, bald die Kleidung der besseren Stände; sie geben sich nicht gleich Jedem hin, sondern man muß erst ihre Bekanntschaft gemacht haben, was übrigens nicht schwer hält. Sie setzen selten einen

bestimmten Preis auf ihre Gunft, lassen sich durch Anerbietungen gewinnen, verlangen zuweilen nur Geschenke oder erwarten sie. Um die Unzucht zu begehen, benützen sie selten ihre Wohnung, sondern sie besuchen entweder die Herren in ihren Wohnungen oder sie bedienen sich der Absteigequartiere.

Eine dritte Klasse von Lohnhuren oder richtiger Gewerbhuren sind diejenigen, welche mehr oder weniger gut eingerichtete Wohnungen für sich haben. Die vornehmeren Dirnen dieser Klasse haben eine stehende Kundschaft, treiben sich nur zum Vergnügen aber nicht um Männer zu gewinnen herum, sie nehmen meist nur auf Empfehlung neue Besucher an. Die weniger vornehmen Dirnen dieser Klasse suchen sich auf öffentlichen Orten, auf den Straßen, ihren Zuspruch, sie machen sich bemerklch, lächeln die vorübergehenden Männer an, nicken ihnen zu, reden sie wohl auch an und verständigen sich mit den Männern, die an sie herantreten, sofort über den Preis.

Eine vierte Klasse von Gewerbhuren hat keine Wohnung für sich, sondern bedient sich nur der Absteigequartiere. Diese Dirnen kleiden sich bald als Dienstmädchen, bald als Ladenmädchen, nehmen Körbchen oder so etwas in die Hand, thun sehr geschäftig, betrachten sich die Läden, richten Fragen an vorübergehende Mannsperionen und brauchen mancherlei Lockmittel.

Die niedrigste Klasse der Gewerbhuren macht zur Abendzeit gewisse Stellen der Promenade unsicher, haust an den Thoren in entlegenen Stadttheilen. Die Subjecte dieser Klasse reden alle Männer an, stürzen zuweilen wie Harpyen aus ihren Schlupfwinkeln hervor auf diejenigen Männer, die in ihre Nähe kommen, sie sind schmutzig gekleidet, benutzen Gebüsch, das freie Feld, entlegene Winkel zur Vollziehung der Unzucht; viele von ihnen sind ziemlich besahnt, die meisten noch außerdem dem Trunke ergeben.

Eine sechste, fast genau umgrenzte Klasse von beträchtlicher Größe bilden endlich die Gewerbhuren, die in öffentlichen Häusern, die unter der Leitung eines Wirthes, einer Wirthin stehen, also in förmlichen Bordellen die Unzucht betreiben. Diese Dirnen sind nach der Feinheit des Bordells besser oder schlechter, aber stets auffallend und auch freigeleitet, tragen große Frisuren und schminken sich nicht selten. In

weniger feinen Bordellen sehen die Dirnen zum Fenster heraus, oder stehen an den Thüren, winken den Mannspersonen, fordern sie auf etc.

Wir könnten noch mehr Distinctionen unter den Lustdirnen Leipzigs machen, die Charakteristit weisläufiger ausmalen, noch speciellere Klassenabtheilungen aufstellen, allein dies würde uns zu weit führen, abgesehen davon, daß die angegebenen Klassen nicht einmal fest umgrenzt werden können, da die eine Klasse in die andere übergeht, da die Dirnen selbst nach Umständen diese oder jene Klasse wählen oder in diese oder jene hineingeträngt werden.

Die Lohn- und Gewerbhuren Leipzigs sind theils Einheimische, theils Auswärtige und Ausländerinnen; die Zahl der Auswärtigen ist größer. Die Vollziehung der Unzucht geschieht in Bordellen, Absteigequartieren, Privatwohnungen, Gasthäusern, in Spielunten, auf den Promenaden, im freien Felde.

Wir müssen nun der Beförderung der Unzucht d. h. der richtigen und vulgären Kuppelerei noch einige Worte widmen.

Vorerst denken wir der Bordellwirthe und Wirthinnen. Wir haben zur Zeit die Kleinigkeit von 48 Bordellen in Leipzig, denen industrielle Ehepaare, oder eine geschiedene Frau, selten ein Mann allein vorsteht. Zumeist sind es bei den Ehepaaren die Frauen, die den Geschäftsbetrieb in der Hand haben; die Männer sind gewöhnlich privilegierte Nichtsthuer, die sich zwecklos, meist gut gekleidet, herumtreiben, Kneipen besuchen, gern die Feinen spielen und ein behagliches Leben führen. Wo die Frau ein strengeres Hausregiment eingeführt hat, bekommt der Mann sein tägliches Taschengeld zum Vertheuen. Die Männer sind gewöhnlich Leipziger von Geburt, vormalige Handwerksgehilfen. Die Wirthinnen haben wohl alle früherhin das Geschäft einer Gewerbhure selbst getrieben, einige treiben es noch unter der Hand oder besitzen einige stehende Kunden. Diese Frauen haben das Geschäft übernommen oder auf Versuch angefangen, weil sie sich einiges Geld erworben hatten und dieses gut anzulegen und sich für die Zukunft zu decken beabsichtigten. Das Geschäft selbst wird nun folgendergestalt betrieben: Die Vorsteher und Vorsteherinnen der Bordelle suchen das Publikum immer mit neuer Waare zu fördern, in der Messe schaffen sie sich einige Dirnen mehr an. Die Dirnen, die

in der Regel Auswärtige sind, denn Leipzigerinnen sind gefährlich, weil sie, wenn sie entlassen worden, aus der Schule schwagen und Verdrießlichkeiten hervorrufen können, werden von den größeren Städten Hamburg, Berlin, München, Dresden, Braunschweig u. s. w., aus kleineren Städten der Nachbarschaften herbeigeholt oder verschrieben. Die Dirnen haben gewöhnlich schon an den Orten, von welchen sie bezogen werden, die Lohn- und meist die Bordellhuren gemacht. Da sie in der Regel Schulden haben, so bezahlt der Bordellwirth, der sie nach Leipzig verschreibt oder holt, die Schulden, und da die Dirnen mit wenigen Kleidungsstücken ankommen, so werden sie mit Kleidern ausgestattet, die ihnen sehr hoch angerechnet werden. Die Dirnen müssen von dem Hurenlohn den dritten Theil, auch wohl noch mehr abgeben, von dem übrigen Lohne haben sie Kost, Logis, Wäsche und die contrahierten Schulden zu bezahlen. Die Einnahme der hübscheren Dirnen, namentlich in der Messe, ist oft enorm, dessemungeachtet behalten diese Dirnen kaum je etwas übrig. Die Bordellwirthe und Wirthinnen sorgen dafür, daß die Dirnen sich kein Geld zurücklegen können; anfänglich treiben sie sie an, ihnen ihre Auslagen zurückzuerstatten, sind diese bezahlt und verdient die Dirne viel Geld, so verleiten sie dieselbe zu allen möglichen unnützen und thörichten Ausgaben, sie wirken auf den bekannten Reichtum derselben hin, daß dieselben zu keinem Geldersparniß kommen können. Diese den Verdienst vernichtende Maßregel halten die Bordellwirthe und Wirthinnen bedwogen aufrecht, weil sie wohl erfahren haben, daß eine Dirne, die sich ein Stück Geld erspart hat, zu etwas mehr Selbstbewußtsein kommt und den Anordnungen und Anforderungen ihrer Tyrannen sich nicht mehr willig fügt. Wird eine Dirne krank, hört sie auf gehörig zu verdienen, so entleibt man sich derselben, nach der Herstellung sucht man sie auswärts in einem Bordelle wieder unterzubringen und wenn sich keine Condition derart findet, wird sie mit nothdürftigen Kleidern und Reisegelde versehen nach ihrer Heimath zurückgeschickt. Dieser großartige Menschenhandel ist in der That empörend. Die Bordellwirthe bewohnen kleine eigenthümliche oder meist gemietete Häuser und Stagen; wohnen sie zur Miete, müssen sie enormen Zins zahlen und leider haben wir Hausbesitzer, die kein Bedenken tragen einen derartigen übermäßigen Zins in die

Tasche zu stecken. Wenige der Bordellwirthe haben Schankgerechtigkeit, dessenungeachtet verschenken alle nicht nur Thee und Kaffee, sondern auch Orog, Punsch, Wein, Champagner namentlich und liefern zu dem höchsten Preise das erbärmlichste Getränk. Die Bordelle sind so eingerichtet, daß sie Jeder wie Restaurationen besuchen kann. In einem Zimmer sind die Dinnen versammelt, die Besucher können sich dieselben ansehen, mit ihnen losen u. s. w.; kleinere Zimmer mit den nöthigen Einrichtungen dienen zur Vollbringung der Unzucht.

Eine zweite Klasse von Beförderern der Unzucht sind die Inhaber von Absteigequartieren; auch hier wird das Geschäft in der Regel von Frauenzimmern getrieben, auch hier sind die das Geschäft treibenden Frauenzimmer früher meist alle Gewerbhuren gewesen. Buhldinnen, bei welchen das Geschäft flodert, verheirathete Huren, die allein nicht genug verdienen können, versuchen durch Ablassen von Stuben und Kammern mit Sopha's und Betten Geld zu verdienen. Zuweilen unternehmen sonst nicht übel berückte Frauenzimmer dieses Geschäft aus Gewinnsucht, ja wir finden sogar arme Familien, welche, um den nothdürftigsten Lebensunterhalt sich zu verschaffen, da der Verdienst nicht hinreicht, das Absteigen zulassen. Die Absteigequartiere zeigen mehrfache Abstufungen, nur wenige sind etwas besser eingerichtet, die meisten sind ziemlich ordinär, ja ganz schlecht ausgestattet. Der Verdienst ist gleichfalls verschieden, manche machen gute Geschäfte, werden stark bezahlt, die meisten erhalten nur mäßige und selbst geringe Bezahlung. Die Absteigequartiere, welche besser eingerichtet sind, für welche mehr gezahlt werden muß, gestalten sich öfters allmählig zu Bordellen um.

Die dritte Klasse von Beförderern der Unzucht und wirklichen Kuppeln bilden folgende Subjecte. Alte Gewerbhuren, die keine Geschäfte mehr machen, geben sich damit ab für Bordelle Dinnen aufzuweisen oder diese Herren anzubieten und zu verhandeln. Wäschfrauen, Schreierinnen, Aufwärterinnen, die früher ebenfalls mitgemacht haben, verleiten leichtfertige Dienstmädchen sich preiszugeben, verschaffen Dienstmädchen Galane, sorgen für Orte der Zusammenkunft. Hier ereignet es sich nicht selten, daß leichtgläubige, etwas leichtsinnige Mädchen wirklich verführt werden. Die vornehmeren Kuppelerinnen dieser Art versorgen gut zahlende Männer mit Dinnen oder sie unter-

nehmen es, eine bestimmte von einem Herrn bezielte Frauensperson, die nicht Jedem feil ist, zur Unzucht zu bewegen. — Unter die kuppelnden Männer müssen wir die Ghemänner und Zubälter von Gewerbhuren rechnen, die sich nicht allein von dem Geschäfte ihrer Gattin oder Dulcinea ernähren, sondern nicht selten auch Schweigegeld fordern und erpressen. Ferner Väter, welche von der Unzucht der Töchter leben, ihre Töchter selbst empfehlen und feil bieten. Als leichtere Beförderer der Unzucht sind Lohnbedienten, Kellner anzusehen, die sich ein Stück Geld mehr dadurch machen, daß sie Huren empfehlen, anbieten, zuführen. —

Von der Wahrheit dieser kurzgefaßten Schilderungen, die viel weiter hätten ausgekehrt werden können, kann Jeder, der die betreffenden Verhältnisse kennen zu lernen wünscht, sich gar bald überzeugen.

Sollten nun, fragen wir, diese hier kürzlich angeführten Verhältnisse, welche sehr vielen Männern Leipzigs bekannt sind, welche viele regelmäßige Besucher der Messen genau kennen, den Behörden und insbesondere der Polizei unbekannt sein? Gewiß nicht. Da nun im Sinne der Gesetzgebung gegen die Betreibung der Unzucht als Gewerbe, gegen die Beförderung der Unzucht und gegen die Kuppelerei von amtswegen, ex officio eingeschritten werden soll, so dürften wir eigentlich erwarten, daß die Polizei gegen das offenkundig getriebene Wesen fortwährend unterdrückend und vernichtend einzuschreiten sich gedrungen fühlen müßte. Allein dies geschieht nicht und wir entschuldigen die Polizei zum Theil, denn sie würde ihr ganzes dermalen vorhandenes Personal fortwährend zu nichts Anderem, als zur Ermittlung von Hurengeschichten nöthig haben und dabei wichtigere ihr obliegende Geschäfte verabsäumen müssen. Die Polizei hat auch wohl begriffen, daß die strengsten Maßregeln, die genauesten Nachforschungen, abgesehen davon, daß sie in Härte ausarten, die Freiheit der Einzelnen zu sehr beeinträchtigen würden, kaum etwas Anderes herbeizuführen im Stande sind, als ein sehr verstecktes und mit Betrügereien aller Art verbundenes Betreiben der Gewerbsunzucht.

Die Polizei ist also in Leipzig (und natürlich auch in Dresden) ganz außer Stande den Bestimmungen der Art. 354 und 355 nachzukommen, sie befindet sich in den Verhältnissen zuzusehen, wie die

Gewerbsunzucht und das gewerbmäßige Befördern der Unzucht offen-
kundig betrieben wird. Um aber doch den gedachten Artikeln des
Strafgesetzbuches einigermaßen Geltung zu verschaffen, so schreitet die
Polizei ein: 1) nach gegebenen Anzeigen und Beschwerden, wo die
Untersuchung sich nicht mehr ablehnen läßt und 2) wenn die Polizei
aus irgend welchen Gründen einzuschreiten für gut findet. Das
Einschreiten erstreckt sich also nicht auf alle diejenigen, die sich that-
sächlich und fortwährend der Vergehen, die Art. 354 und Art. 355
nennt, schuldig machen, sondern die Polizei nimmt aus der Gesamm-
theit der Schuldigen Einzelne, einige Wenige heraus.

Die Anzeigen bei der Polizei erfolgen von verschiedenen Seiten;
von Hauswirthen und Miethleuten, die nicht leiden wollen, daß
Huren in ihrem Hause wohnen, aus- und eingehen und ihr Wesen
treiben; von Personen, die in der Nachbarschaft von Gewerbhuren
und Bordellen wohnen und die den Spektakel, der öfters stattfindet,
aufgehoben wissen wollen; von Eltern, Geschwistern, Vormündern
der fellen Dirnen, welche wünschen, daß letzteren das Gewerbe ge-
setzt werde; von Männern, die von Gewerbhuren in und außer den Bor-
dellen angeheft worden sind u. Den Anzeigen dieser Art, die in der
Mehrzahl der Fälle als ganz in der Ordnung zu betrachten sind,
kommt die Polizei in so weit allerdings nach, als sie die betreffenden
Dirnen citirt, sie zur Ruhe verweist, ihnen verbietet dieses oder jenes
Haus zu besuchen, sich herumzutreiben, daß sie den Bordellwirthen
einschärft Ruhe und Ordnung zu halten u., allein in der Weise ein-
zuschreiten, wie sie es nach dem Strafgesetzbuch thun sollte, unterläßt
sie in der Regel. Es ist doch gewiß nicht so schwer den Beweis zu
schaffen, daß diese oder jene Dirnen Gewerbsunzucht treiben, leblich
davon leben, ja die meisten der Dirnen können sogar ohne große Mühe
auf frischer That ergriffen werden. Nur nach Anzeigen von An-
stetzung dehnt die Polizei, wosfern die Anzeige sich bestätigt, manchmal
die genauere Untersuchung auch auf die Gewerbsunzucht aus. Am
meisten wird das beliebte Princip der Polizei: das Nichtsahnwollen,
das Dulden, in Bezug auf die Bordelle ersichtlich. Wo bleibt bei
48 Bordellen die Thätigkeit der Behörden, die einschreiten, von an-
wegen einschreiten und gar nicht erst Anzeigen und Beschwerden ab-

warten sollten? Hier wo so viele Mannspersonen täglich des Ge-
schlechtsgenusses halber ein- und ausgehen, hier wo Tausende und
abermals Tausende bezeugen können, daß sie für schweres Geld schlechte
Getränke und abgebrauchte felle Dirnen bekommen haben, hier sollte
es der Polizei an Beweismitteln mangeln, um die Existenz der Bor-
delle darzuthun?

Da es auf der Hand liegt, daß die Polizei den gesetzlichen Be-
stimmungen eigentlich nicht nachkommt, so sucht sie ihre Toleranz da-
durch zu beschönigen, daß sie eine polizeiliche Verordnung, „das
liederliche, arbeitslose Herumtreiben und Ausflügen betreffend“, benutzt
und einen Theil der Gewerbhuren selbst in den Bordellen zwingt,
Arbeit nachzuweisen. Die Polizei glaubt sich hierdurch sicher zu stellen,
sie giebt vor, daß sie keine Gewerbhureri dulde, daß sie nach Anzeigen
und wenn Dirnen ihr verdächtig scheinen, nachforsche und wenn sich
die Anzeige, der Verdacht bestätigt gefunden, die Subjecte zur weiteren
Untersuchung den Gerichten überantwortet habe. Leider bestätigt sich
nach den gewöhnlichen polizeilichen Untersuchungen weder der Verdacht,
daß Gewerbsunzucht, noch der Verdacht, daß gewerbmäßige Beförde-
rung der Unzucht getrieben worden sei, obgleich jeder Andere, der von
Polizei nichts versteht, bei den betreffenden Personen über allen Ver-
dacht erhaben ist und volle Gewißheit besitzt. Die Polizei begnügt
sich, wenn keine Arbeit, kein Dienst nachgewiesen werden kann, das
liederliche Herumtreiben zu urgiren, wohl auch zu bestrafen; wird
Arbeit, Dienst nachgewiesen, so beschränkt sich die Polizei auf eine
mehr oder weniger salbungsvolle Ermahnung, ja nicht Vohnhureri zu
treiben und sich dabei erweisen zu lassen.

Was ist nun der Erfolg dieses beschönigenden Verfahrens der
Polizei? Die Gewerbhuren geben Arbeit oder Dienst vor, obgleich
Beides entweder gar nicht vorhanden ist, oder nur in einem solchen
Verhältnisse existirt, daß es den geringsten Theil des Verdienstes aus-
macht. Es giebt immer Leute genug, welche um irgend eines Vor-
theils willen, meist aber für Geld, das Verhältniß der Scheinarbeit,
des Scheindienstes begünstigen und einer Frauenderson, welche wegen
Verdacht der Gewerbhureri von der Polizei befragt worden ist oder
Auswärtigen, welche, um das Hurengeschäft zu treiben, eine Aufent-

haltigeftaltung erlangen wollen, das Zeugniß geben, sie ständen in Dienst oder Arbeit bei ihnen. Die Täufchung, welche die Gewerthuren, auf diese Weise von der Behörde selbst dazu veranlaßt, ausführen, um ungehört ihr Gewerbe betreiben zu können, liegt eigentlich klar zu Tage, zumal wenn die Arbeitsgeber, wie so häufig, Subjecte sind, die selbst keine Arbeit haben, allein die Polizei vermeidet auf diese Scheindienstverhältnisse einzugehen, um nicht mehr zu erfahren, als sie will. Am widerigsten springt das Scheindienstverhältnis bei den Bordellhuren in die Augen. Die Bordellwirthe brauchen für ihre Wirthschaft feile Dirnen, die sie, wie schon gedacht, meist von auswärts her beziehen. Die Dirnen bringen nun ein Dienstbuch bei und die Polizei erlaubt ihnen als Dienstmädchen in die Bordelle zu treten. Die Polizei leidet aber nur zwei, höchstens drei solcher Dienstmädchen, respective Huren in einer Wirthschaft, weil aber die Bordellwirthe sich mit so wenigen Dirnen nicht begnügen und in den Messen, um der Nachfrage gehörig zu entsprechen, nicht begnügen können, so helfen sie sich dadurch, daß sie in Leipzig Anläßige veranlassen, auf ihren Namen die Lustirren als Dienstmädchen einschreiben zu lassen und diese dann ihnen zum Geschäftsbetriebe zu überlassen.

Es werden aber auch Anzeigen über Gewerbdunzucht von nichtswürdigen gemeinen Subjecten gemacht, die durch die Anzeige einen Akt der Rache ausüben wollen. Jeder Mann, der, nachdem er eine Hure aufgesucht und für Lohn gebraucht hat, diese denuncirt, begeht eine gemeine und schufstige Handlung und sein Verfahren kann nur dann gerechtfertigt werden, wenn er von der Lohnhure bestohlen, betrogen, oder angehetzt worden ist; im ersten Falle richtet er seine Anklage auch nicht gegen die Lohnhure als solche, sondern gegen die Betrügerin und Diebin, im zweiten Falle will er verhindern, daß die Hure nicht ferner Quelle der Infection für andere Mannspersonen werde (dieser letztere Grund ist schon etwas verdächtig, da es noch gelindere Mittel als Anzeige, der dann Strafe folgt, giebt). Wer aus Aerger über das gezahlte Geld, aus Rache, weil er von der feilen Dirne, die nicht creditiren wollte, zurückgewiesen, weil er als ein Angeetrunkenen nicht angenommen, weil ein Anderer vorgezogen wurde u. s. f., den Ankläger macht, der ist dem moralischen Charakter nach vielleicht noch unter

die Gewerbhure zu setzen. Hier tritt nun der Uebelstand ein, daß die Polizei, trotzdem, daß sie die Nichtswürdigkeit der Anzeige und des anzeigenden Subjectes recht wohl erkennt, doch einschreiten muß, während es wahrlich gerechter sein würde, wenn man dem Angeber den Kopf tüchtig wüschte. Uns sind Beispiele bekannt, wo Männer, weil sie im betrunkenen Zustande von den Huren, die sie gewöhnlich zu besuchen pflegten, nicht angenommen wurden, weil die Huren sich nicht zu gewissen Schamlosigkeiten verstehen wollten, aus Rache Anzeige machten. Wir sahen es, daß ein junger Vagabond, dessen Schwester Gewerbhure war und von welcher er unaufhörlich Geld erpreßt hatte, als die Schwester sich ferner Geld zu geben weigerte, diese denuncirte, Beweise ihrer Schuld beibrachte und angab, er könne es nicht länger ansehen und verantworten, daß seine Schwester die Hure mache. Wir kennen einige trunksüchtige Väter, die ihren Gewerbdunzucht treibenden Töchtern formwährend Geld abnehmen und sie desseneungeachtet aus vergeblich moralischem Drange von Zeit zu Zeit denunciren. So, derartige Nichtswürdigkeiten sind selbst von Mannspersonen verübt worden, die jahrelang mit einer Gewerbhure ein Liebesverhältnis unterhalten haben, jahrelang von derselben unterstützt und ernährt worden sind. Wir müssen unter solchen Umständen die Behörde beklagen.

Zu welchen Uebelständen die mangelhaften gesetzlichen Bestimmungen und das Nichtstheuenwollen der Behörde führt, darauf müssen wir noch ganz kurz hinweisen. Sind einmal in Folge von Anzeigen Gewerbhuren und Bordellwirthe eingezogen worden, so suchen sich die Inculpaten bei den Vernehmungen auf alle Weise herauszulügen. Die notorische Gewerbhure stellt Alles in Abrede, behauptet, sie sei ein solches Dienstmädchen und der Bordellwirth ist erstaunt, daß seine Dienstmädchen Lohnhurerie betrieben haben sollen, da in seinem Hause etwas Derartiges gar nicht sich ereignen kann. Beide Theile bleiben bei ihren Lügen, stimmen in ihren Aussagen überein, wenn nicht einmal eine dumme Dirne, welche die Instruktionen, die sie erhalten hat, aus Angst vergißt, herausplagt und den ganzen Schwindel zu Tage bringt. Die Gerichtspersonen, denen es nun wenig daran liegt die Wahrheit zu ermitteln, schreiben die offenkundigen und frechsten

Lügen in die Akten, und so kommt es denn, daß allbekannte Gewerhuren freigesprochen, oder höchstens als verdächtig erklärt werden, daß Bordellwirth, die schon seit Jahren das lenocinium vulgare treiben, häufig straflos, ja selbst verdachtlos ausgehen. Wären die Beweise bei anderen Vergehen und Verbrechen so leicht und sicher zu schaffen, wie bei Gewerbdunzucht und gewerbmäßiger Beförderung der Unzucht, so würde kaum je etwas unentdeckt bleiben, und wollten die Behörden bei anderen Vergehen und Verbrechen eben so wenig und oberflächlich untersuchen und forschen, als bei Gewerbdunzucht und gewerbmäßiger Beförderung derselben, so würde nie ein Vergehen oder Verbrechen ermittelt werden.

Die Polizei schreitet zweitens, wie wir gesagt haben, gegen Gewerbdunzucht und gewerbmäßige Beförderung der Unzucht ein, wenn sie es für gut findet. So lange dieses Einschreiten nur gegen solche Dinen gerichtet ist, die ihr Gewerbe mit zu großer Frechheit, Unfläthe und zum öffentlichen Aergernisse betreiben, oder anderer strafbarer Handlungen verdächtig sind, sobald es Bordellwirth betrifft, bei welchen Spektakel, Tumult, Betrügereien aller Art vorgekommen sind, so ist dieses Verfahren in so weit zu billigen, als die Polizei wenigstens einige Ordnung in die verwirrte Sache zu bringen und gewisse Grenzen zu setzen sucht. Leider sind aber die Grenzen, welche die Polizei der Unzucht setzt, sehr variabel, und in den Meisten fallen sie ganz weg. Die niederen Polizeibeamten liefern die Motive für das Einschreiten und so ereignet es sich leider oft genug, daß gerade die Besten der räudigen Schafe bestraft werden, während die Schlechtesten und Niederträchtigsten leer ausgehen. Daß bei so mangelhaften und fehlerhaften Gesetzen, die sich nicht ausführen lassen, Umgehungen des Gesetzes, Willkürlichkeiten aller Art und selbst Ungerechtigkeiten nicht vermieden werden, ist wohl klar. Wir wollen nur noch andeuten, daß, da die Beaufsichtigung der Gewerbhuren und Bordelle meist in den Händen der niederen Polizeibeamten sich befindet, die Konflikte mit dem Gesetze auch in sofern schlecht einwirken, daß sie eine Corruption des niederen Polizeipersonals leicht veranlassen.

Zum Troste für unser Vaterland, Sachsen, können wir soviel sagen, daß die gesetzlichen Bestimmungen der übrigen deutschen

Staaten um kein Haar besser sind, daß die Handhabung der Gesetze ebenso wie in Sachsen umgangen wird, daß dieselben Uebelstände sich herausstellen. Wollten wir aber die Institutionen von Paris zum Beispiel als mustergültige betrachten, so würden wir nur noch einer größeren Verschlechterung das Wort reden, in dieser Weltstadt hat die Gewerbdunzucht und Kuppelerei eine Höhe erreicht, von der wir hoffen, daß sie von Deutschland auf ewige Zeiten fern bleibe; dort sind mit der Prostitution Verbrechen verbunden, die wir hier nicht kennen oder von welchen wir nur ausnahmsweise etwas hören.

Wenn wir uns in eine Vergleichung der deutschen Gesetzgebungen in Bezug auf die Vergehen und Verbrechen gegen die Sittlichkeit einlassen, wenn wir die Institutionen des Auslands noch herbeiziehen wollten, so würden wir, abgesehen davon daß wir einer solchen Arbeit nicht gewachsen wären, statt einer Abhandlung voluminöse Bände schreiben müssen. Wir sehen daher von solchen Vergleichungen ab. Wir wenden uns jetzt zur Besprechung der wichtigen Frage: welche Stellung der Staat der unverfügbaren Prostitution gegenüber einzunehmen habe.

Die Prostitution hat schon im vorigen Jahrhunderte eine Reihe von sorgfältigen Untersuchungen und eingehenden Erörterungen veranlaßt, hauptsächlich hat man sich aber in den letzten drei Jahrzehnten vielfach mit derselben beschäftigt. Die Literatur über die Prostitution ist außerordentlich umfangreich geworden; dabei ist bemerkenswerth, daß die meisten Schriftsteller über Prostitution dem ärztlichen Stande angehören, und daß nur wenige Staatsmänner, Juristen und Polizeimänner dieses Thema besprochen haben. Mögen nun viele Schriftsteller von einseitigen Standpunkten ausgegangen sein, die Prostitution nur nach einer Richtung hin verfolgt haben, so viel sieht fest, daß in der Neuzeit sich immer humanere Anschauungsweisen Bahn gebrochen haben und daß man zu der Ueberzeugung gekommen ist, daß mit Härte und Zwang nichts genügt, sondern nur geschadet werden kann.

Man hat die Prostitution verschiednen definiert, dem Begriffe Prostitution eine engere oder weitere Ausdehnung gegeben. Viele beziehen die Prostitution nur auf die Gewerbdunzucht, Einige schließen sogar die Unzucht in Bordellen aus. Nach unserer Ansicht kann und

darf die Prostitution nur in einem und zwar in einem sehr umfangreichen Sinne aufgefaßt werden, weil die Gröfserung derselben nach allen Richtungen hin gleich wichtig scheint. In das Gebiet der Prostitution gehört der ganze außereheliche Verkehr mit seinen verschiedenen Abstufungen, und es bezieht sich die Prostitution nicht allein auf das weibliche Geschlecht, sondern eben auch auf das männliche. Die christliche Sittenlehre als Inbegriff aller jener Vorschriften, nach welchen wir unbedingt handeln sollen, gestattet einzig und allein den ehelichen Verkehr. Ein Frauenzimmer, welches den außerehelichen Verkehr selbst nur ein einziges Mal zuläßt, beschimpft, prostituiert sich. Der Grad des Schimpfs, den sich ein Frauenzimmer durch außerehelichen Verkehr zufügt, hängt von verschiedenen Umständen ab. Es ist zu unterscheiden, ob sich ein Frauenzimmer einmal oder mehrmal, häufig, Einem oder Mehreren hingegeben habe, es ist die Bildung des Frauenzimmers zu berücksichtigen, die begünstigenden Momente der Verkehrshaltung müssen erwogen werden, hauptsächlich sind aber auch die Motive in's Auge zu fassen, welche das Frauenzimmer zur Verkehrshaltung bewegen. Ein anticipirter ehelicher Verkehr bildet die niedrigste Stufe der Leiter, von dieser bis zur schändlichsten Gewerbsunzucht rückt das Vergehen in unzähligen Sprossen aufwärts.

Die Geschichte aller civilisirten Staaten hat deutlich gezeigt, daß der außereheliche Verkehr nicht unterdrückt werden kann, daß alle Maßregeln, die man angewendet hat, um ihn zu verbannen, gescheitert sind, daß insbesondere aber das Straf- und Beschimpfungssystem sogar als sehr schädlich sich erwiesen hat, indem es, ohne eine Beschränkung zu bewirken, die Inculpaten häufig, um der Strafe und Schmach zu entgehen, zu Begehung anderer und größerer Verbrechen bewegen hat.

Die Staaten haben wohl begriffen, daß, obgleich sie die Verpflichtung haben, die Sittlichkeit bei ihren Unterthanen möglichst zu fördern, was sie durch Schule und Kirche von jeher zu thun bemüht gewesen sind, ihnen doch keineswegs das Recht zusieht, alle moralische Vergehen zu strafen. Unter den unmoralischen Handlungen, die begangen werden, hat der Staat nur dann ein Recht strafend einzugreifen, wenn dadurch die Freiheit, Ehre, das Leben oder das Eigen-

thum Anderer angetastet, wenn dadurch der Staat in Gefahr gebracht wird. Nach diesen leitenden Grundsätzen sind die Strafgesetzbücher entworfen worden. Liegen begangene unmoralische Handlungen außerhalb des Bereichs der Strafe, so stehen dem Staate zuweilen noch polizeiliche Maßregeln zu Gebote, um derartigen Vergehen entgegenzuarbeiten, nicht selten kann aber von staatswegen bei derartigen Vergehen gar nichts gethan werden.

In der neuesten Zeit haben nun die meisten Staaten, und auch Sachsen, den außerehelichen Verkehr als ein Vergehen der Art anerkannt, welches, obgleich unmoralisch, doch die Bedingungen einer criminalrechtlich strafbaren Handlung nicht in sich trägt. Man hat sich aber in Deutschland nicht dazu entschließen können, die Straflosigkeit auch auf jenen außerehelichen Verkehr auszudehnen, der für Lohn und zum Erwerb betrieben wird. Die Unzucht als solche ist unanständig, und nur wenn sie für Lohn und als Gewerbe betrieben wird, soll sie strafbar sein. Wenn nun irgend eine unmoralische Handlung nach den gesetzlichen Bestimmungen nicht strafbar ist, so kann dadurch, daß sie für Lohn vollzogen wird, ein Unterschied nicht erwachsen. Das Geld kann die Handlung, die sonst dieselbe bleibt, wohl etwas schimpflicher machen, wenn sie aber ohne Lohn, Geld, nicht strafbar ist, kann sie dadurch nicht erst strafbar werden. Aber zugegeben, die Unzucht für Lohn verdiene gestraft zu werden, müsse gestraft werden, so begreifen wir nicht, warum diese unmoralische Handlung, zu deren Vollziehung ein männliches und ein weibliches Individuum gehört, nur an dem letzteren und nicht auch an dem ersteren gestraft wird. Der Mann, der eine feile Dirne aufgesucht, sich mit ihr heimlich und dann eingelassen hat, kann weder von Verführung noch von Betrug reden; er wollte seinen Geschlechtstrieb befriedigen, und da er diese Befriedigung nicht umsonst haben konnte, so bezahlte er sie. Ist die Befriedigung des Geschlechtstriebes auf außereheliche Weise für Geld überhaupt strafbar, so muß, wenn Sinn darin sein soll, die Strafe auf den zahlenden wie auf den bezahlten Theil sich erstrecken.

Unduldsame Moralisten haben sich zur Vertheidigung des einseitigen Strafverfahrens dahin ausgesprochen, die moralische Verworfenheit einer Weibsperson, welche ihr höchstes Gut, die weibliche

Ehre, zum Gegenstand des Handels gemacht und für Lohn preisgegeben habe, dürfe nicht unbeftraft gelassen werden und schon die erste derartige Verletzung der weiblichen Ehre müsse gestraft werden, weil sie gemeinlich viel schimpflicher sei, als alle nachfolgenden. — Ohne über den Ausdruck „moralische Verworfenheit“ rechten zu wollen, der jedenfalls unpassend und zu hart ist, können wir jene unbestimmten Redensarten, wie „das höchste Gut, die weibliche Ehre“ in der Beweisführung nicht gelten lassen. Um zu begreifen, daß die weibliche Ehre, die weibliche Tugend, kurz die Keuschheit für Frauenzimmer das höchste Gut sei, müssen die Frauenzimmer durch Erziehung auf den Standpunkt gebracht werden, dieses einzusehen. Trotzdem daß die Schulerziehung bei uns in einer solchen Weise ausgebildet ist, daß wir in dieser Beziehung Sachsen fast allen Vändern voranstellen können, so geht doch die Erziehung bei den Frauenzimmern der niederen Volksklassen keineswegs so weit, um sie zur Schätzung jenes gedachten höchsten Guts zu bringen. In den Schulen wird den Mädchen die christliche Sittenlehre erläutert und vorgetragen, allein wenn der Lehrer auch auf weibliche Tugend und Keuschheit dabei zu reden kommt, so kann und darf er doch Kindern die geschlechtlichen Verhältnisse und Beziehungen nicht weiter auseinanderlegen. Die erst mit beginnender Pubertät aus der Schule entlassenen Frauenzimmer bleiben bei den höheren Ständen unter unausgesetzter Aufsicht der Eltern, insbesondere der Mütter; in den niederen Ständen werden sie häufig sofort in die Welt hinausgeschoben, um für sich selbst zu sorgen, ihren Unterhalt zu erwerben, und wenn sie im Hause bleiben, wird ihnen kaum eine Beaufsichtigung oder Unterweisung zu Theil, welche sie zur Beurtheilung und Schätzung der Keuschheit bringen könnte. Der erwachende Geschlechtstrieb, das Alledastehen, die Arbeitsverhältnisse, durch welche Mädchen niederen Standes oft in weit nähere Berührung mit dem männlichen Geschlechte kommen, als dies in höheren Ständen vorzukommen kann, das Beispiel anderer Mädchen, nicht selten leider auch das elterliche Beispiel und noch eine Menge anderer begünstigender Momente tragen dazu bei, daß das Frauenzimmer vorzeitig und ohne recht zu wissen, was sie thut, ohne Kämpfe oder Vorwürfe dem Geschlechtsgenusse ihre Virginität opfert. Haben die Frauenzimmer erst

einmal dem Geschlechtstrieb gehuldigt, so wird die Begierde drängend, und da sie über den Verlust der Virginität sich keine moralischen Vorwürfe machen, so werden sie auch keine weiteren Bedenken hegen, die Befriedigung des Geschlechtstriebes fortzusetzen. Was sind nun die Folgen? Die Frauenzimmer werden häufig von den Männern, die sie zur Unzucht brachten, verlassen, oft erst, nachdem sie geschwängert worden sind. Aufhörender Erwerb, Mangel, Kränkungen aller Art, getäuschte Hoffnung, Kummer, alle diese Plagen stürmen auf die Unglücklichen ein. Wenn sie endlich die Bedrängnisse überwunden haben und mit ungeschwächter Gesundheit davon gekommen sind, so werden sie durch das Erlebte nicht zu besseren Grundfägen geführt, zur Tugend zurückgeleitet, sondern sie werden nun vorsichtiger und klüger, benutzen künftighin bei der Wahl ihrer Liebhaber ihr pecuniäres Interesse, geben sich in Städten, wo sie so leicht Anlaß finden, für Lohn hin. Was soll aber das Strafen frommen? Kann das Frauenzimmer vernünftigerweise deswegen gestraft werden, weil es ein Gut verkaufte, welches es schon seit geraumer Zeit nicht mehr befeßen hat? Der Staat sieht ruhig zu, wie das höchste Gut, die weibliche Ehre, bei jungen, unerfahrenen, in die Welt geschobenen Frauenzimmern verloren geht, er läßt die Unzucht so lange walten, bis sie zum Gelderwerb benutzt wird, dann aber meint er durch Strafen die betreffenden Frauenzimmer zum Bewußtsein der Bedeutung der weiblichen Tugend bringen zu müssen.

Warum aber überhaupt noch strafen, wo die bürgerliche Gesellschaft schon selbst ein hinlängliches Strafrecht ausübt? Die käuflichen und als solche bekannten Frauenpersonen werden von Allen ihres Geschlechts, außer von gleich feilen, verachtet, zurückgewiesen, verhöhnt, selbst die Männer, die sich mit den feilen Dirnen zu vergeben pflegen, wollen nach der Befriedigung ihrer Lust nichts von ihnen wissen, ignoriren, verspotten sie, und wohl selten dürfte es eine feile Dirne geben, die nicht die Erniedrigung, die sie sich zugezogen, und das Brandmal, welches ihr das Publikum aufgedrückt hat, tief empfinden sollte.

Auf dem Lande und in kleinen Orten sind die Folgen der Unzucht häufig nicht so beklagenswerth, als in großen Städten. Die getriebene Unzucht endet nicht selten mit der Ehe zwischen den theilhaftigen Per-

sonen; der Verlust des höchsten Guts wird für nicht so beträchtlich erachtet, um einen Mann abzuhalten, ein Mädchen zu heirathen, von welchem er weiß, daß es schon mit einer Reihe von Männern geschlechtlichen Umgang gehabt habe. In gewissen Gegenden heirathen die Männer niederen Standes kaum eher, als nachdem sie sich von der Fruchtbarkeit eines Frauenzimmers überzeugt haben. Auf dem Lande ist die Unzucht groß genug, aber die Unzucht für Lohn selten und die Gewerbsunzucht erstirt kaum. In den großen Städten ist dies anders. Die jungen, unerfahrenen Frauenzimmer sind dem Antrage einer Menge von Männern ausgesetzt, die weder heirathen können, noch wollen, die Geschlechtsgenuss suchen und alle Mittel brauchen, um diesen Zweck zu erreichen, und namentlich der Gürtel, der Püßigkeit der Dienen schmeicheln, häufig auch noch von Kuppelrinnen in ihren Bestrebungen unterstützt werden oder sich unterstützen lassen. Rarger Lohn, schlechte Kost und Behandlung, dazu noch Leichtsin, Unersahrenheit, Naturtrieb, sowie directe und indirecte Verführung bringen hier die Frauenzimmer sehr leicht auf die Bahn des Lasters, auf welcher sie unaufhaltsam vorwärts gedrängt werden, und von welcher der Rücktritt zur Ehrsamkeit durch Staat und Behörde eher erschwert als erleichtert wird.

Sehen wir ab von dem Nachtheile, den die Prostitution auf die Enstütlung habe und fassen wir nur den körperlichen Schaden in's Auge, so ist derselbe oft sehr groß, und er ist nicht selten bei der unbezahlten Prostitution eben so groß und fast noch größer, als bei der bezahlten. Durch vorzeitigen übermäßigen Geschlechtsgenuss wird die Gesundheit des einen wie des andern Geschlechts untergraben und vernichtet. Wir erinnern an die gefährliche Prostitution, wie sie an Fabriorten vorzukommen pflegt und wie sie auch in Leipzig, wo viele Fabriken sich etablirt haben, sich auszubilden begonnen hat. Mädchen von 14, 15 Jahren werden, kaum nachdem sie die Schule verlassen haben, von nicht älteren oder wenig älteren Knaben desorirt. Die sich dem Geschlechtsgenusse ergebenden unreifen Subjecte wechseln unter einander und ruiniren sich gegenseitig. Solche Knaben und Mädchen wohnen auf den benachbarten Dörfern Leipzigs und wandern täglich zur Arbeit nach den Fabriken in und bei der Stadt.

Wir haben bis jetzt nur von dem weiblichen Theile der Prostitution gesprochen und den männlichen Theil kaum berührt, es erscheint nothwendig, über die an der Prostitution theilnehmenden Mannspersonen auch einige Worte zu sagen.

Es ist merkwürdig und nicht zu erklären, warum bei dem Vergehen der Unzucht, bei der Prostitution, die Hauptlast der Schmach und Schande, die dieses Vergehen trifft, und die alleinige Last der Strafe auf den weiblichen Theil gewälzt wird. Gegenüber der weiblichen Ehre, diesem so hoch angeschlagenen Gute, scheint eine männliche Ehre gar nicht zu existiren. Die Unzucht wird als ein Vergehen betrachtet, welches wohl Frauenzimmern die Ehre rauben, aber die Ehre der Mannspersonen nur in sehr geringer Weise beeinträchtigen kann. Bei dem niederen Stande ist das Verhältniß beider Geschlechter in Bezug auf die Kränkung der Ehre weit gleicher, und die desorirte Frauensperson wird von dem Manne nicht viel geringer geachtet als die jungfräuliche. Anders ist es bei den höheren Ständen, hier verlangt der Mann von dem Frauenzimmer, welches er ehelicht, Unberührtheit, Keuschheit, während an den Mann diese Forderung nicht gestellt wird. Nur wenige Männer bestiegen als keusche Junggesellen das Ehebett. Ein Mann, der den Geschlechtsgenuss sucht und ihn außer ehelicher Weise sich fleißig verschafft, gilt für einen Ehrenmann durch und durch, und wenn er es recht toll treibt, fügt man dem günstigen Urtheile über ihn höchstens die Worte bei: „aber er liebt die Weiber ein wenig.“ Man giebt vielfach den Rath, junge Männer auch in Bezug auf den Geschlechtsgenuss austoben zu lassen, und will durch die Erfahrung bestätigt gefunden haben, daß solche Wüßlinge späterhin als Ehemänner recht gut anstücken. Wenn wir bei Töchtern der höheren Stände Virginität finden, so ist dies kein Wunder, sobald wir sehen, daß dieselben aus der Aufsicht und dem Unterrichte nicht herauskommen bis zur Heirath, ja ein begangenes Unzuchtsvergehen würde bei diesen Mädchen um so verächtlicher sein müssen, weil es eine besondere, alle Rücksicht misachtende Lust und Hang zur Intrigue voraussetzen ließe. Wir wollen auch die Männer, die bei ihrer selbständigeren Stellung im Leben in mancherlei Lagen gebracht werden, welche wohl das Aufbrausen des Temperaments entschuldigen können, nicht zu hart be-

urtheilen, allein den Grad von moralischer Lässheit, welchen so viele Männer in Bezug auf Unzucht namentlich in großen Städten beßigen, können wir unmöglich ungetadelt lassen.

Wir müssen aber unser Augenmerk noch auf die geschlechtlichen Beziehungen der Männer und insbesondere der Männer der besseren Stände zu den Frauenzimmern des niederen Standes richten. Die jungen Frauenzimmer des niederen Standes werden, wenn sie nur einigermaßen hübsch sind, von Männern, die keinen andern Zweck als Geschlechtsbefriedigung haben, aufgespürt und förmlich belagert. Man redet die Mädchen an, begleitet sie, trifft sie auf Vergnügungs-, namentlich Tanzorten. Die gebildeten und außerdem noch mit Geld versehenen Männer machen eine gefährliche Ueberredungskunst geltend und lassen den allmächtigen Hebel, das Geld, anfänglich unter der Form von kleinen Aufmerksamkeiten, dann unter der Form von Geschenken aller Art, welche der Eitelkeit schmeicheln, wirken, bis sie endlich directer vorwärts gehen und das Ziel erreichen. Ausfucht, Eitelkeit, die Sehnsucht nach einer unabhängigeren, besseren Stellung, ein gewisser Grad von Faulheit, Sorglosigkeit und Leichtsinne sind die gewöhnlichen Eigenschaften der ungebildeten und unerfahrenen Frauenpersonen, die ihren Fall erleichtern helfen und sie auf jenen Weg bringen, der zur Gewerbdunzucht führt. Bei ihrem Falle ist die größere Schuld gewöhnlich auf der Seite des Mannes, eine Verführung kommt häufig in Anwendung, der zu widerstehen die ungebildeten Frauenpersonen die Kräfte nicht haben. Hat der Mann der Prostitution seine Lust gefühlt, so verläßt er gewöhnlich die Verführte, indem er sich nach Abwechslung umsieht, und diese, wenn sie nicht die Ueberzeugung gewinnt und sich anstrengt ein anderes Leben zu beginnen, was meist Unerfahrenheit und Leichtsinne verhindern, wenn sie vielmehr den angefangenen Wandel fortzuführen will, muß sich einen andern Liebhaber anschaffen. Nicht selten bringen auch Mangel und Elend Frauenzimmer, die sich jahrelang hielten, endlich zur Preisgebung. Hunger thut weh, und es gehört viel dazu, in der Noth Anerbietungen auszuschilagen, die temporäre schlechte Umstände talich verbessern können. Wir brauchen die Schilderung über die Verführung der Mädchen niederen Standes in großen Städten nicht weiter auszudehnen; die

Verführung wird um so häufiger vorkommen, je größer die Menge der Männer ist, die Zeit und Geld oder beides übrig haben und bemüht sind, dem außerordentlichen Geschlechtsgenuße nachzujagen. Soviel steht fest, daß bei der Prostitution von weiblicher Seite fast nur die Frauenzimmer des niederen Standes theilhaftig sind, während von den Männern alle Stände bis zu den höchsten hinauf an der Prostitution Theil nehmen; daß ferner der männliche Theil den vornehmlich activen Theil bildet und die Hauptschuld bei dem Fall des Frauenzimmers trägt.

Es ist noch von mehreren und selbst ärztlichen Schriftstellern zur Entschuldigung der Männer bei Beurtheilung der Unzucht als Thatfache hervorgehoben worden, daß der Geschlechtstrieb bei den Männern stärker sei, weniger leicht gezügelt werden könnte, als bei den Frauen. Diese Behauptung muß als physiologisch total falsch zurückgewiesen werden, das Weib ist sogar von den Geschlechtsfunctionen als weit abhängiger zu betrachten als der Mann. Letzterer kann ohne den Geschlechtsfunctionen nachzukommen, den Höhepunkt seiner körperlichen Entwicklung erlangen; ist der Mann geschlechtstheilig geworden, so trägt die Ausübung der Geschlechtsfunction zu seiner Körpervollendung nichts mehr bei. Beim Weibe ist dies anders, dieses erreicht erst durch die Ausübung der Geschlechtsfunction den Höhepunkt der körperlichen Ausbildung, ohne Geschlechtsbefriedigung schrumpft das Weib vorzeitig ein. Beim Manne sind die Geschlechtsverrichtungen durchaus vom Willen abhängig, während beim Weibe ein Theil der Geschlechtsverrichtungen gar nicht dem Willen unterthan ist, so geht z. B. der Ausscheidung des Monatsflusses Aufschwellung der Gebärmutter, der sämtlichen Geschlechtstheile, Aufregung des ganzen Organismus voraus. Beim Manne ist der Geschlechtsgegnuß kurz, vorübergehend, beim Weibe nachhaltiger. Wäre der Geschlechtstrieb beim Weibe nicht stärker als beim Manne, so ließe sich kaum erklären, warum das Weib wiederholt den lästigen Schwangerschaftsbeschwerden, dem schmerzhaften Gebärakte und dem Wochenbette sich aussetzt, es ließe sich gar nicht einsehen, warum ein Weib nach einer künstlichen, sehr schmerzhaften und gefährlichen Entbindung, ja nach dem Kaiserschnitte, sich abermals könnte schwängern lassen. Bemerken wir nun endlich noch, daß beim Manne im allgemeinen größeres

Gefühl, größere Urtheilskraft vorhanden ist, daß die Zurechnungsfähigkeit des Mannes dem vom Gefühle abhängigeren Weibe gegenüber größer ist, so können wir uns nicht entschließen bei Unzucht, vergehen den Mann zu entschuldigen und dem Weibe die Hauptschuld aufzubürden.

Da die Prostitution von dem einfachsten und unter den entschuldigbarsten Umständen vollzogenen außerehelichen Beischlafe an, bis zu der niedrigsten Gewerbsunzucht herab, jene Bedingungen nicht enthält, welche der Staat bei strafbaren Handlungen voraussetzt, da durch die Prostitution weder die Freiheit, das Leben, noch das Eigenthum eines Andern angetastet, der Staat nicht in Gefahr gebracht wird, so ist der Staat ungerecht zu nennen, wenn er aus der Prostitution bloß die bezahlte Unzucht als strafbar heraushebt, doppelt ungerecht, wenn er die Strafe nur bei der bezahlten prostituta eintritt und den bezahlenden prostitutor laufen läßt.

Hört der Staat auf die Gewerbsunzucht zu bestrafen, so erspart er sich selbst die Kränkung zu sehen, daß sein Gesetz umgangen, mißachtet wird. Der Staat wird also die Prostitution ebenso zu dulden haben, wie andere moralische Vergehen, die eben auch die Bedingungen criminalgesetzlich strafbarer Handlungen nicht in sich tragen. Das Ignoriren seiner Gesetze darf aber der Staat nicht zugeben, wenn er nicht sein eignes Ansehen schwächen will.

Wenn dem Staate das Recht nicht zusteht, alle unmoralischen Handlungen zu strafen, sondern wenn nur gewisse unmoralische Handlungen seiner Strafgewalt anheimfallen, so ist damit nicht gesagt, daß die unmoralischen Handlungen, die außerhalb der Strafe liegen und die gebuldet werden müssen, eine Berechtigung hätten und daß der Staat sie zu billigen oder gar anzuerkennen habe. Der Staat hat die Moral als oberstes Princip festzuhalten, er darf der Unsitlichkeit nie Vorstübchen leisten. Wenn der Staat die Prostitution zu dulden hat, so darf er sie doch nicht anerkennen und für berechtigt halten. Der Staat hat bei dem fortwährenden Streben die Sittlichkeit der Unterthanen zu fördern auch dahin zu arbeiten, der Prostitution möglichst entgegenzuwirken. Der Staat darf nicht einen müßigen Zuschauer

der Prostitution abgeben, sondern er hat Maaßregeln zu erfinden und auszuführen, welche das Zustandekommen der Prostitution hemmen, welche die begünstigenden Momente für die Ausbildung der Prostitution beschränken, er hat ferner die bestehende Prostitution zu überwachen, daß sie nicht zu weit ausartet, zu noch anderen Verbrechen führt, er hat endlich seine Bemühungen dahin zu richten, die der Prostitution Ergebenen und namentlich die in derselben verfunkenen Frauenpersonen aufzurichten und zurückzuführen. Wir werden weiter unten auf die Präventivmaassregeln, auf die Ueberwachungs- und Rückführungsmaassregeln der Prostitution zurückkommen.

Auf die Beförderung der Unzucht und auf die Kuppelerei darf sich die für die Prostitution im allgemeinen beantragte Straßlosigkeit nicht erstrecken. Die leichteren Vergehen der Beförderung der Unzucht ausgeschlossen, bildet der gewerbmäßige Betrieb der Beförderung der Unzucht und die Verleitung und Verführung unverleumdeter Frauenpersonen zur Unzucht mit Andern im höchsten Grade insamirnde Verbrechen. Wir kommen bei dieser Gelegenheit auf die Vordelle zu reden, die noch immer von vielen Seiten her als zulässig, zweckmäßig, ja selbst als nothwendig betrachtet werden.

Aus dem nothwendigen Einverständnisse der Politik mit der Moral wird schon a priori bewiesen, daß der Staat, da er die Unzucht nicht gutheissen, noch weniger die gewerbmäßige Beförderung derselben erlauben, concessioniren oder gar privilegiren darf. Der Staat darf überhaupt nie dem Sittenverfalle seines Volks das Siegel der Autorität aufdrücken. Das Laster, welches unter der Autorität und Protection des Staates begangen werden kann, verliert in den Augen der Nation seine Schändlichkeit und macht diese mit dem Laster vertrauter. Man hat aber eingewendet, daß unter Umständen die Politik mit der Moral nicht in Einverständniß gebracht werden könnte und daß dann der Staat wohl in die Lage gesetzt werden dürfte, das Laster zu gestatten, als solches nicht zu strafen, es sogar zu schämen, wenn er dadurch anderen größeren Lasten vorbeugen könnte. Diese Art der Schlussfolgerung hat man auf die Zulässigkeit, Zweckmäßigkeit und Nothwendigkeit der Vordelle in Anwendung gebracht. Wäre die Schlussfolgerung richtig, so müßten wir sie schon deswegen verwerfen,

weil sie dem Staate den gefährlichen jesuitischen Grundsatz, nach welchem der Zweck das Mittel heiligen soll, aufzuringen würde. Es läßt sich aber auch mit leichter Mühe darthun, daß alle jene Gründe, welche man vorgebracht hat, um die Zweckmäßigkeit und Nothwendigkeit der Bordelle zu erweisen, total falsch sind. Man hat unter Anderem gesagt, die Bordelle wären ein Mittel die Nothzucht und gewaltsame Angriffe auf die weibliche Unschuld zu verhüten; der zügellose, rasende Geschlechtstrieb der Männer würde dadurch in Schranken gedämmt, zwischen welchen er weniger Schaden anrichten könnte; diejenigen Männer, welche nicht heirathen, einen Hausstand nicht unterhalten könnten und doch den Geschlechtstrieb nicht zu bändigen vermöchten, würden die Schuld, Mädchen zu verführen, nicht auf sich laden; da die Dirnen in den Bordellen erfahrungsgemäß in Folge des übermäßigen Geschlechtsgenusses nur selten schwanger würden, so würde durch Bordelle die Zahl der außerehelichen Schwangerschaften und Geburten beschränkt; durch die Bordelle würde man auf zweckmäßige Weise die liebreichen Dirnen einer Stadt von den soliden Frauenzimmern absondern können; in den Bordellen ließe sich die Unzucht besser überwachen, zügeln, unschädlicher machen, während die heimlich getriebene Gewerbdunzucht weit gefährlicher sei; endlich würden die Bordelle als Abzugskanäle in der Art, daß sie das männliche Geschlecht vor Selbstbefleckung bewahrten.

Es kann uns nicht einfallen alle jene nichtigen und thörichten Gründe vorzubringen und zu besprechen, die man zur Verschönerung und Begünstigung des Bordellwesens vorgebracht hat. Nur kurzlich entgegen wie folgendes: Die Nothzucht, ein Verbrechen, welches glücklicherweise nur selten vorkommt, setzt eine solche Rohheit, Grausamkeit und Verthiertheit voraus, daß Jemand, der dieses Verbrechen zu begangen sich vorgenommen hat, dadurch, daß er weiß, es seien Bordelle vorhanden, in welchen er ohne Zwang seiner Brutalität Genüge thun könne, sich schwerlich von seinem ruchlosen Vorsatz wird abhalten lassen. Der soll vielleicht eine scheinliche Nothzucht deswegen strenger oder milder beurtheilt werden, je nachdem in der Stadt, wo sie verübt wurde, Bordelle bestanden oder nicht? Was andere gewaltsame Angriffe auf Frauenzimmer betrifft, wie das Anpacken auf den Straßen,

so werden diese durch bestehende Bordelle gewiß nicht verhindert, da auch sie Ausdrücke der Rohheit und Gruslichkeit sind. Die dem außerehelichen Geschlechtsgenusse ergebenden Männer, die jungen und alten Wellüftlinge, die Zeit und Geld zur Disposition haben, finden die Baare in den Bordellen nicht etwa stets nach ihrem Geschmacke und suchen leichtsinnige, aber noch nicht so heruntergekommene Dirnen, als die Bordellhuren auf. Werden nicht jene begünstigenden Momente für die Entstehung der Prostitution, die wir oben erwähnten, beseitigt, werden die Frauenzimmer des niederen Standes nicht zur besseren Erkenntniß der weiblichen Würde gebracht, so kann die Unzucht in keiner Weise durch die Bordelle beschränkt werden, die letztern bilden nur noch eine überflüssige Zugabe zu der bestehenden Unzucht. Die Bordelle werden auf die außerehelichen Schwängerungen und Geburten aus gleichen Gründen nicht den entferntesten beschränkenden Einfluß äußern, abgesehen davon, daß Huren in Bordellen gar nicht etwa so selten schwanger werden. Kein lächerlich ist es, sich nur auf einen Augenblick dem Gedanken hinzugeben, als ließe die Prostitution einer größeren Stadt sich in eine Anzahl von Bordellen einsperren; vergessenswärtig man sich die verschiedenen Gradationen der Prostitution, bedenkt man, daß ein großer Theil derselben aller Beaufsichtigung und Inspection völlig unzugänglich bleibt, so fällt jeder Gedanke an Begrenzung weg. Was sollen wir aber von der Solidität der Frauenzimmer für hohe Vorstellungen bekommen, die in Absonderung von den Bordellen ihre Begründung findet? Was endlich noch das Verbrechen der Selbstbefleckung anlangt, welches von Manns- und Frauenpersonen getrieben wird, so wird dies leider am häufigsten von Kindern und namentlich Knaben geübt und die erwachsenen Manns- personen, die den Geschmack daran aus der Knabenzeit mitgebracht haben, werden durch Bordelle von der lasterhaften Gewohnheit, die noch obendrein kein Geld kostet, nicht abgebracht, und wir finden nicht selten, daß Onanisten die Selbstbefleckung dem Weisklase vorziehen, von dem letztern selbst gar nichts wissen wollen.

Sehr wichtig aber und durchaus constatirt sind die Nachteile, welche die Bordelle mit sich bringen und welche so groß sind, daß, selbst wenn dem Bestehen des Bordellwesens eine gewisse Nützlichkeit nicht

abgesprochen werden könnte, diese doch im Vergleich zu den Nachtheilen auch nicht die mindeste Beachtung erfahren dürfte.

Durch das Bestehen der Bordelle wird ein eigenthümlicher, ehe- loser Stand, der der Bordellwirthe und Bordellwirthinnen begründet und gebildet, der sonst gar nicht in's Leben treten kann, ein Stand der niederträchtigsten und habüchlichsten Subjecte, die man mit Recht als Schinder bei lebendigem Leibe, als Menschenveräufer betrachten kann. Die Brutalität, die in den Bordellen niederen Rangs zur Schau getragen wird, ist kaum ekelhafter als das Raffinement, welches in den Bordellen höheren Rangs herrscht. Hier erreicht die Entwürdigung einen Grad, wie sie außerhalb der Bordelle nicht vorkommt, hier werden Anerbietungen, Zumuthungen gestellt, angeboten, normirt und angenommen, daß wir mit Recht sagen können, die meisten der außerhalb der Bordelle ihre Geschäft treibenden Frauenzimmer sind lange nicht so tief gesunken, um derartige Gräuelt zu treiben. Die Bordelle sind es, denen wir am meisten die Verführung der männlichen Jugend zur Last legen müssen; Knaben, die sich sonst kaum getrauen würden ein Frauenzimmer anzureden, werden von etwas älteren Knaben verleitet mit in ein Bordell zu gehen; das erste und zweite Mal vielleicht bekämpfen sie sich und verzehren nur etwas, dann aber folgen sie der bösen Lust, finden Geschmack an dieser Art der Unzucht und fahren fort sich körperlich und geistig zu entkräften. Endlich richten wir noch einen Blick auf die unglücklichen oft körperlich schönen Frauenzimmer, die aus Leichtsinne oder verleitet den Bordellen anheimgefallen sind, die, wie wir schon oben gezeigt haben, ein Stüd Waare bilden, welches auswärts eingehandelt, und wenn es abgebraucht ist, wieder verkauft oder weggeworfen wird. Die Blüthenzeit dieser Frauenzimmer ist nach einigen Jahren beendet, sie werden an Körper und Geist ruiniert, sinken unaufhaltsam, gehen meist vorzeitig zu Grunde, verfallen den Arbeitshäusern, Versorgungshäusern, Armenhäusern und Krankenhäusern. Wenn die Gewerthure außerhalb des Bordells sich körperlich ruiniert, so hängt dies wenigstens von ihr selbst ab, sie befügt ihre Selbstbestimmung, in den Bordellen müssen sich die Dinen zu Grunde richten, sie mögen wollen oder nicht; während außerhalb des Bordells die Puhle dirne sich nicht nur etwas erwerben, sondern selbst sparen kann, wenn

sie durch eignen Willen und glückliche Umstände begünstigt aus ihrer entwürdigenden Stellung sich herauszureißen im Stande ist, so ist dies bei den Dinen in dem Bordelle fast unmöglich, zu Verdienste kommen sie nicht, den eignen Willen haben sie verloren und glückliche Zufälle gehen für sie unbenutzt verloren. Solche unglückliche und verlorene, fast unrettbar verlorene Mädchen schafft das Bordellwesen.

Der Staat, der, wenn er das Laster nicht bestrafen kann, es doch nimmermehr gutheissen und autorisiren kann, darf daher die Gewerthung der Unzucht nicht privilegiren noch weniger aber die gewerbmäßige Beförderung der Unzucht concessioniren. Während der Staat die Gewerthung der Unzucht nur möglichst zu überwachen hat, liegt es ihm ob, die Beförderung der Unzucht und Kuppelerei der Bordelle unnachlässiglich zu verfolgen und derartigen Wirthschaften vorsehende Wirthe und Wirthinnen sofort zur Rechenschaft und Strafe zu ziehen.

Eine ausführlichere Besprechung müssen wir jetzt noch der Prostitution als Hauptquelle einer ansteckenden und über den Erdball verbreiteten Krankheit, der Syphilis widmen. Von diesem Standpunkte aus haben auch die Staaten ihre Aufmerksamkeit vielfach auf die Prostitution gerichtet, von diesem Standpunkte aus hat eine große Menge von Ärzten die Prostitution betrachtet. Dadurch, daß Staat und Aerzte diesen mehr untergeordneten Standpunkt bei ihren Betrachtungen, Massregeln und Vorschlägen an die Spitze gestellt und ihn im Eifer zu nützen als maßgebend angesehen haben, ist man in eine beklagenswerthe Einseitigkeit verfallen, die unbeabsichtigte Härte und Ungerechtigkeit zur Folge gehabt hat.

Die Syphilis ist eine ansteckende Krankheit, die lediglich durch ein fixes Contagium ansteht; der Träger des syphilitischen Contagium ist in der Regel das Secret des syphilitischen Geschwürs, der Schanker etc. Um die Ansteckung zu bewirken ist nothwendig, daß das Contagium mit einer wunden Stelle, mit einem Hautrißchen in Berührung gebracht, oder durch Friction an einer mit dünner Haut bedeckten Körperstelle eingerieben werde. Die Ansteckung erfolgt also durch Einimpfen, Inoculiren, Einreiben, Inprägiren. Den gewöhnlichen Sitz des ansteckenden syphilitischen Geschwürs geben die Geschlechtsorgane ab. Aus der Natur des Contagium und dem Sitze des syphilitischen Geschwürs

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folgt, daß Ansteckung in den meisten Fällen durch Vereinigung der Geschlechtstheile, durch Weischlaf zu Stande kommt. Im allgemeinen sind beide Geschlechter gleich empfänglich für die syphilitische Infection. Durch Unzucht, außerehelichen Weischlaf wird die Ansteckung am häufigsten vermittelt, die Krankheit weiter verbreitet.

Dem Staate liegt es ob durch die medizinische Polizei die Gesundheit der Bevölkerung zu wahren und er hat dabei namentlich auf die Abwehr von Schädlichkeiten seine Aufmerksamkeit zu richten. Von der Abwehr schädlicher Speisen und Getränke, von der Abwehr von Schaden durch gefährliche Arzneistoffe und Gifte kann hier nicht die Rede sein, wohl aber von der Abwehr von Ansteckungstoffen. Eine ganze Reihe von Krankheiten und darunter mehrere Thierkrankheiten haben die Eigenschaft sich durch Ansteckungstoffe verschiedener Natur fortzupflanzen, ja es giebt Krankheiten, die an und für sich nicht ansteckend sind und es erst durch Umstände, besondere Einflüsse werden können. Bei allen ansteckenden Krankheiten ist es in Bezug auf die Abwehr wichtig, die Natur und Beschaffenheit des Ansteckungstoffes und die Art der Ansteckung und Verbreitung desselben zu kennen. Leider ist nun bei vielen ansteckenden Krankheiten und grade bei den pestartigen, die auch noch durch endemische und epidemische Einflüsse begünstigt werden, die Natur des Ansteckungstoffes ganz unbekannt und wir wissen nur so viel, daß diese Krankheiten ein flüchtiges Contagium haben müssen, welches sich auf die verschiedenste Weise, aber leicht, rasch, selbst rapid und wie der Wind zu verbreiten vermag. Der Staat stößt nun bei den Maasregeln der Abwehr auf nicht geringe Hindernisse und kann bei den pestartigen Krankheiten kaum etwas anderes vornehmen, als Absperrung, Isolirung der Kranken, Absperrung von ganzen Städten und Districten, Quarantainen etc., er muß sich vorsehen, diese Absperrungsmaasregeln nicht unzeitig, unnöthigerweise zu ergreifen, weil sie leicht mehr Schaden als Nutzen anrichten und in Verordnungsmaasregeln ausarten können. Bei einer ansteckenden Krankheit nicht pestartiger Natur mit ihrem Contagium, wo der Einzelne sich selbst und leicht zu schützen vermag, da hat die Sanitätspolizei den Schutz zu übernehmen eigentlich keine Pflicht. Die Syphilis gehört, so schlimm sie auch sein mag, nun weiter zu den

pestartigen Krankheiten, noch zu jenen, vor welchen die Einzelnen sich nicht zu schützen vermöchten. Vor der Syphilis kann sich Jeder sicher bewahren; alle Mannspersonen und Frauenzimmer, die ehrbar leben, sich nie bei der Prostitution betheiligen, nicht eher und anders den Weischlaf vollziehen, als nachdem sie sich mit gleich ehrbaren Personen verheirathet haben, werden gewiß von Syphilis verschont bleiben. Nur wenige Fälle giebt es, wo die Syphilis ohne alle Schuld erworben wird, und diese Fälle, auf welche wir noch später zu reden kommen, vermögen die Richtigkeit der vorsehenden Behauptung im allgemeinen nicht zu entkräften. Die Weiterverbreitung der Syphilis wird gehindert werden, wenn die infectirten Personen vom Momente der Ansteckung an bis zu ihrer vollständigen Herstellung allen geschlechtlichen Umgang unterlassen, allein leider hat es, noch abgesehen von dem Willen, mit dem Erkennen der Syphilis von Seiten der infectirten Personen seine Schwierigkeiten. Die Männer, deren Geschlechtstheile ihren Augen zugänglich sind, bemerken einige Tage nach einem vorausgegangenen Weischlase wenigstens soviel, daß sich an ihren Geschlechtstheilen etwas gebildet hat, was nicht dahin gehört, ob dies aber Syphilis sei, dieses können sie nur nach einer größeren Erfahrung über diese Krankheit beurtheilen, denn es kommen an den Geschlechtstheilen erythematöse, erysipelatöse, ekzematöse und pustulöse locale Haut- und Schleimhautentzündungen vor, die durchaus nicht syphilitischer Natur sind. Noch schwieriger ist die Erkennung syphilitischer Geschwüre für die Frauenzimmer, deren Geschlechtstheile die eigne Beschauung nur zum geringsten Theile zulassen; es kann hier sehr leicht vorkommen, daß die kranken Frauenpersonen gar nichts merken, daß die versteckt liegenden Geschwüre lange bestehen und fortgeführt werden. Ob also Syphilis, ob ansteckende syphilitische Geschwüre in einzelnen Fällen bestehen, darüber kann nur der geübte Arzt entscheiden; der Mann kann ohne den Arzt eher vermuthen, daß er syphilitisch angestrichen sei als das Weib, beide können im Vermuthungsfalle nur durch den Arzt sicheren Aufschluß erhalten. Wenn man also Jedem es zur Pflicht machen wollte, so wie er eine syphilitische Ansteckung sich zugezogen hat, sich sofort des Weischlafs zu enthalten, so würde man zu viel und selbst Unmögliches verlangen. Fast alle erwachsenen Männer und auch

ein großer Theil der erwachsenen Frauenzimmer wissen, daß es eine ansteckende Krankheit, die Syphilis, giebt, sie wissen auch, daß diese Krankheit durch den Beischlaf fortgepflanzt wird, allein wie sie sich äußert, welche Symptome sie bei ihrem Entstehen und in ihrem weiteren Verlaufe darbietet, darüber können sie keine Auskunft geben und nur ein Theil gewisiger und durch Schaden klug gewordener Personen haben davon einige, wenngleich sehr mangelhafte Kenntniß. Ohne daher die Absicht der Verheimlichung zu haben, können Mannspersonen, vorzüglich aber Frauenzimmer geraume Zeit syphilitische Geschwüre mit sich herumtragen und, wenn sie Geschlechtsgegnen sich bereiten, Ansteckung vermitteln, die Syphilis verbreiten, ohne es zu wissen. Nun kommt noch hinzu, daß außer Unkenntniß noch Noth, Sorglosigkeit, Nachlässigkeit, Furcht vor Schande etc. Motive abgeben können, daß eine syphilitische Ansteckung verschwiegen wird und daß das betreffende Individuum sich nicht abhalten läßt, fleischlichen Umgang zu treiben, so daß der Ansteckung durch die Syphilis und der Weiterverbreitung Thore und Thüren geöffnet sind.

Viele Staaten haben nun in Bezug auf Syphilis einzuschreiten für notwendig erachtet und sanitätspolizeiliche Maasregeln in Ausführung gebracht, durch welche ein zeitiges Erkennen der Syphilis und Verhinderung der Weiterverbreitung ermöglicht werden soll. Von den sanitätspolizeilichen Maasregeln, welche die Staaten bisher zu treffen sich gemüßigt gefunden haben, können wir leider nur so viel sagen, daß sie höchst unzulänglich sind, daß, wenn ihnen auch einiger Nutzen in Bezug auf Verbreitung der Syphilis nicht ganz abzuspochen ist, sie doch diesen geringen und noch zweifelhaften Vortheil durch weit größere Nachtheile, d. h. durch Eingriffe in die Freiheit und Sittlichkeit der Bevölkerung erkaufen müssen.

Alle sanitätspolizeilichen Maasregeln, die man in Ausführung gebracht hat, sind nur für große Städte eingerichtet worden, für kleine Städte und für das Land hat man sie nicht für nöthig befunden, obgleich sich die Syphilis wenig um Stadt oder Land kümmert. Die polizeilichen Maasregeln beziehen sich zumeist nur auf Frauenspersonen, und zwar nur auf diejenigen Frauenspersonen, die wegen Loh- und Gewerburerei bekannt oder dieser dringend verdächtig sind.

Man hat in großen Städten die in Bordellen sich aufhaltenden Dirnen einer speciellen ärztlichen Controle unterworfen, hat diese Dirnen aller 8 oder 14 Tage durch verpflichtete Aerzte untersuchen lassen und hat die bei der Exploration syphilitisch krank befundenen Subjecte sofort zur Behandlung in Krankenhäusern untergebracht. Von den außerhalb der Bordelle Gewerbenzucht treibenden Frauenspersonen (der sogenannten vagirenden Prostituirten) hat man die notorischsten, oder diejenigen, deren man habhaft werden konnte oder wollte, durch Verordnung gezwungen, sich wöchentlich oder alle 14 Tage in bestimmten polizeilichen Lokalen untersuchen zu lassen. Man hat den Gewerburereien, wie den Bordellburen Bücher und Karten eingehändigt, in welchen der Tag der Untersuchung von dem untersuchenden Arzte bemerkt wird und der Befund der Untersuchung gleichfalls angegeben ist. Von der Wichtigkeit und Erpresslichkeit dieser sanitätspolizeilichen Untersuchungen überzeugt, hat man sogar in einigen Städten dem Bordellburen Vorschub geleistet, um möglichst viel Buhldirnen beisammen zu haben, und man hat sich geschmeichelt treffliche Resultate erzielt zu haben, wenigstens auf dem Papiere.

In Berlin, von woher wir zuverlässige Nachrichten erhalten haben und wo die sanitätspolizeiliche Untersuchungsmaasregel der Buhldirnen im Gange ist, befanden sich vor einigen Jahren etwa 300 Dirnen in den Bordellen, die regelmäßig explorirt wurden; von den außerhalb der Bordelle Lohn- und Gewerburerei treibenden Frauenzimmern wurden etwa 5—600 in bestimmten Lokalen wöchentlich untersucht und mit Controlbüchern versehen. Da nun die Zahl der Lohn- und Gewerburerei treibenden Frauenzimmer in Berlin niedrig tarirt auf 14—15000 geschätzt werden muß, so erhebt man schon hieraus deutlich die Unzulänglichkeit der Untersuchungsmaasregel. Man hat von jeher die halben Maasregeln getabelt, aber hier kann man nicht einmal von einer halben, sondern man muß von einer Zwanzigstelmaasregel reden. Von den vulgo angenommenen Verbreiterinnen der Syphilis können also $\frac{1}{20}$ dieses thun und nur bei $\frac{1}{20}$ bestrift man sich, es zu verhindern.

Kann man sich bei solchen Verhältnissen nun wohl einbilden, die ganze Männerwelt der Stadt, die mit Buhldirnen Umgang sucht und

liebt, würde sich an das sanitätspolizeilich überwachte Zwangsgel der Dirnen wenden und die anderen $19/20$ ganz unberücksichtigt lassen? Gewiss nicht, denn das Zwangsgel würde selbst bei bestem Willen nicht einmal im Stande sein, die Anforderungen der sich andrängenden Männerwelt zu befriedigen. Die $19/20$ ununtersucht gelassenen Dirnen werden ihren Zuspruch wie früher behalten, ja man könnte sie für besser erachten, weil sie die Polizei nicht als solche, die sich untersuchen lassen müssen, markirt hat. Sollten aber wirklich gewisse Manns- personen ihrer Sicherheit halber den Besuch bei dem beaufsichtigten Zwangsgel vorsehen, so werden gar Manche über kurz oder lang zu ihrem Schaben belehrt werden, daß auch hier keine Sicherheit vor An- steckung zu finden ist.

Die sanitätspolizeilichen Untersuchungen sollen, wie man ver- geben, den Zweck haben, einmal die Buchtdirnen selbst, andererseits die mit ihnen verkehrenden Männer vor Ansteckung zu warnen. Nehmen wir die Erfahrung zur Hand, die man über die Ansteckung netterischer Buchtdirnen, namentlich der in Bordellen beschäftigten, gewonnen hat, so stellt sich heraus, daß trotz allen ärztlichen Untersuchungen nur wenige von denselben der entlichen syphilitischen Ansteckung entgehen, daß die meisten derselben bald früher bald später sich Syphilis erworben. Die Untersuchung kann auch ersichtlich die Ansteckung der Dirnen nicht hindern, und somit ist das Vorgehen der Wahrung in Bezug auf die Dirnen nichtig; man überläßt dieselben der Ansteckung und bemüht sich nur, die stattgehabte Ansteckung derselben baldmöglichst zu erkennen. Wollte man billig sein, wollte man den Buchtdirnen wirklich einen Schutz gegen Ansteckung gewähren, so müßte man alle Männer, welche sich mit Gewerdburen abgeben wollen, gleichfalls untersuchen lassen. Man sieht wohl ein, zu welchen lächerlichen Maaßregeln dies führen würde. Man kann doch wahrlich nicht auch die Männer in Lokale zur sanitätspolizeilichen Untersuchung treiben und ihnen Gesundheits- bücher ausfertigen.

Fassen wir die Untersuchungsmaaßregel in Bezug auf die Ver- breitung der Syphilis auf, so stellt sich heraus, daß die Gesamtheit der betheiligten Männer und alle Frauenzimmer, die nicht sanitäts- polizeilich untersucht werden, bei der Verbreitung der Syphilis nicht

gehemmt sind. Wie sehen also, daß die ganze Maaßregel nur zu Gunsten der Männer, die sich mit sanitätspolizeilich beaufsichtigten Dirnen einlassen, eingeführt worden ist, und daß sie selbst in dieser Auffassung mangelhaft ist.

Man hat hervorgehoben, das angesteckte Weib und namentlich die Gewerdbure könne auf mehr Personen die Syphilis übertragen, als der Mann, und dies ist in so fern richtig, als das Weib öfter und selbst ohne Lust dazu zu haben, den Beischlaf vollziehen kann, während der Mann einer Erregung bedarf, die seine Geschlechtstheile zu der zur Vollziehung des Beischlafs nöthigen Schwellung bringt. Allein wenn Unkenntniß und Leichtsinns, wie die reciproke Ansteckung zeigt, auf beiden Seiten gleich ist, so wird die durch ein einziges Frauen- zimmer veranlasste Ansteckung mehrerer Männer dadurch wieder ins Gleich gebracht, daß die angesteckten Männer nun wieder auf eine entsprechende Anzahl Weiber die Krankheit übertragen.

Die sanitätspolizeiliche Untersuchungsmaaßregel scheint aus der irrigen Ueberzeugung hervorgegangen zu sein, als käme die Syphilis unter anderen Frauenzimmern als den von ihr zur Untersuchung designirten Gewerdburen gar nicht vor, als könne man mit der Unter- suchung einiger Gewerds- und Bordellburen die Syphilis an der Wurzel fassen. Leider haben wir durch die Anschauung eine ganz andere Ueberzeugung gewonnen, wir haben bei Landmädchen, Dienste- mädchen, Rätherinnen, Pugmacherinnen u. s. w., kurz bei Frauen- personen, die keineswegs zu den Lohn- und Gewerdburen gehörten, allein sich prostituirt hatten, Schankergeschwüre und syphilitische Affec- tionen aller Art gesehen, ja wir haben sogar bei diesen Frauen- zimmern mehr Fälle von hochgradiger Syphilis beobachtet, als bei Gewerdburen und zwar einfach aus dem Grunde, weil diese Mädchen keine Vorstellung von ihrer Krankheit hatten und gar nicht daran dachten, daß sie syphilitisch sein könnten.

Sollen Untersuchungsmaaßregeln auf Ansteckung und Verbreitung der Syphilis einen entschieden hemmenden Einfluß haben, so müssen sie auf die ganze Prostitution im weitesten Sinne, auf die dabei be- theiligten Frauenpersonen wie Manns personen sich erstrecken. Solche Maaßregeln lassen sich aber gar nicht ausführen, und wenn man sie

in's Werk zu setzen versuchen wollte, so würde man die Freiheit jedes Einzelnen antasten, beeinträchtigen müssen, man würde eine enorme Zwangsmannschaft in Bewegung zu setzen haben.

Die sanitätspolizeilichen Untersuchungsmaaßregeln können aber auch in so fern als höchst einseitige und von unrichtiger Auffassung ausgegangene bezeichnet werden, als sie die Syphilis, die keine notwendige, aber eine häufige Folge der Unzucht und der Gewerbsunzucht ist, an die Spitze der traurigen Folgen, die aus der Unzucht entspringen sollen, stellen. Viel wichtiger, tiefer eingreifend und nachtheiliger als die Syphilis ist die tiefe Schwächung und Entkräftung des Körpers und Geistes, welche die Prostitution so häufig bei beiden Geschlechtern hervorruft. Gegen diesen Einfluß, gegen diese Folgen hat man keine Maaßregeln getroffen. Die Theilnehmer der Prostitution können sich getrost körperlich und geistig ruiniren, nur gegen Syphilis ist man in Bezug auf die Männer empfindlich.

Hi nun etwa der Staat, der sich so besorgt in Bezug auf syphilitische Ansteckung zeigt, eben so besorgt in Bezug auf andere ansteckende Krankheiten als Syphilis, eben so besorgt für die Gesundheit seiner Unterthanen überhaupt? Leider nein. Masern, Scharlach, Keuchhusten sind ansteckende Krankheiten, die gewiß häufiger als Syphilis den Tod herbeigeführt haben, allein wir können uns nicht erinnern, je von einer Maaßregel gehört zu haben, um die Weiterverbreitung dieser Krankheiten bei vorkommenden Epidemien zu hindern. Kommt es einmal zur Cognition, daß eine Reihe beliebiger Männer sich Schanker geholt hat, so wird Polizei und Sanitätspolizei in Alarm gesetzt, denn man findet dies für erschrecklich, und doch sind die Angeheften keine unschuldigen Wärmer, sondern sie haben sich in den meisten Fällen ihre Schanker wohl und mit vollem Rechte erworben. Rafft aber einmal eine Scharlach-Masernepidemie viele, viele Kinder dahin, so macht man höchstens die Bemerkung, daß es recht über die Kinder hergehe. Wir müssen vor unseren Augen sehen, wie die armen Säuglinge der niedrigsten Volksklasse, insbesondere die armen unglücklichen Ziehkinder durch unpassende schlechte Kost, durch Mangel aller Pflege und Abwartung dahinsinken, verkrüppeln und manchmal fast dahin gemordet werden, allein eingegriffen, energisch eingegriffen wird in keinem

Weise. Man steht ruhig zu, wie der Brantwein Tausenden Gesundheit und Leben kostet, wie auch der sonst fleißige Arbeitermann, der ihn zur Stärkung und weil er nichts anderes haben kann, mäßig genießt, dennoch dem schädlichen Einflusse dieses Getränks unterliegt, allein es geschieht nichts, um den Brantwein zurückzudrängen. Syphilis, Syphilis muß es sein, um Eingriffe, wenn auch ungewöhnliche, zu veranlassen.

Man hat noch zu Gunsten der polizeilichen Untersuchungen vorgebracht, die Verbreitung der Syphilis müsse deswegen so viel wie möglich gehindert werden, weil zuweilen Personen ganz schuldlos angesteckt worden wären. Fälle von syphilitischer Ansteckung ohne alle Schuld kommen allerdings vor. Am häufigsten ereignen sich diese beklagenswerthen Fälle bei Untreue eines Ehegatten, der sich durch außerehelichen Verkehr angesteckt hat und dann seine Gattin inficirt, und leider ist auf diese Weise schon manchmal den Frauen der höchsten Stände Syphilis mitgetheilt worden. Seltner findet der umgekehrte Fall statt, daß die Gattin den Gatten mit Syphilis beschenkt. Ziemlich oft ist es auch beobachtet worden, daß gewissenlose Mütter und Ammen Säuglinge angesteckt haben. Endlich kommen noch manchmal bei Ärzten, namentlich bei Geburtshelfern schuldlose Ansteckungen durch Explorationen, Operationen syphilitischer Frauenzimmer vor. Allen diesen Fällen nun, die gerade wirksame Vorbeugungsmittel gegen syphilitische Ansteckung am wünschenswertheften machten, kann gar nicht vorgebeugt werden, die sanitätspolizeiliche Untersuchung der Gewerkschuren kommt aber gar nicht in Betracht. Der Ehemann, der einmal Ehebruch zu treiben sich angewöhnt hat und alle Dornen, die ihm gefallen und die zu haben sind, beschläft, und der bei seiner fortgesetzten außerehelichen Befriedigung auf seine Geschlechtstheile wenig achtet und leichtsinnigerweise auch wieder einen ehelichen Verkehr unter die außerehelichen einmischet, wird seine Frau stets anstecken können und wenn die Untersuchungsmaaßregeln noch hundertmal weiter ausgedehnt würden. Vor der Ansteckung eines Säuglings durch eine Amme kann keine sanitätspolizeiliche Untersuchung sicher stellen. Ammen gehören meist zu den Prostituirten, aber nicht zu jenen Prostituirten, welche die Untersuchungsmaaßregel trifft. Wenn aber eine Amme nur dann erst

angenommen wird, nachdem eine genaue ärztliche Exploration dieselbe als frei von Syphilis gefunden hat, so erwächst hieraus keine Sicherung, denn wenn die Amme leichtfertig ist, sich Männern hingibt und Ansteckung erleidet, kann sie erschütterungsweise wieder anstecken, den ihr anvertrauten Säugling inficiren.

Wir haben noch endlich die sanitätspolizeilichen Untersuchungsmaaßregel vom Standpunkte der Moral aus zu beleuchten, und hier zeigt sie sich als ein unerlaubter, willkürlicher Eingriff, der unter der Maske der Besorgtheit um Gesundheit nur die Unsitlichkeit fördern muß.

Wird die Unzucht mit ihren Gradationen bis zur Gewerbdunzucht nicht für gesetzlich strafbar erachtet, so darf sie doch der Staat in keine Weise begünstigen, ihr Vorschub leisten. Ist es aber nicht eine Begünstigung, wenn der Staat sich bemüht, die Puhleirnen recht hübsch sauber zu erhalten, damit ihre Besucher in ihren unzüchtigen Belustigungen ja durch die unangenehme Syphilis keine Störung erleiden?

Wenn der Staat Maaßregeln ergreift, die dem Entstehen der Prostitution entgegenwirken, wenn er Maaßregeln in Anwendung bringt, um die bestehende Prostitution zu überwachen, wenn er endlich durch Maaßregeln den Rücktritt von der Prostitution zur Ehrbarkeit zu befördern sucht, so handelt er löblich und pflichtgemäß. Wenn aber der Staat Maaßregeln in Ausführung bringt, die unter die genannten drei Kategorien nicht gehören, wenn er eine Maaßregel einführt, die nicht gegen das Laster gerichtet ist, sondern diesem nur den Stachel nehmen soll und es dann wünschenswerther und begehrendwerther erscheinen läßt, so heißt dies der Unsitlichkeit Vorschub leisten.

Unter die Maaßregeln, welche die Entstehung der Prostitution beschränken sollen, können die sanitätspolizeilichen Untersuchungen deswegen nicht gerechnet werden, weil sie sich auf der Prostitution bereits ergebene Frauenzimmer und zwar auf die niedrigsten beziehen. Eine Maaßregel, welche den Rücktritt von der Prostitution zur Ehrbarkeit fördert, bildet die sanitätspolizeiliche Untersuchung auch nicht, denn sie bewirkt gerade das Gegentheil, sie stempelt, brandmarkt die Untersuchten. Es bliebe nur noch übrig, die Maaßregel als Ueberwachungs-

maaßregel zu betrachten, allein auch als solche kann sie nicht genommen werden. Die Maaßregel überwacht durchaus nicht die Sittlichkeit, die Dirne kann frech sein wie sie will, nur syphilitisch darf sie nicht werden.

Wenn die Puhleirnen, wie ungewisselhaft, immer noch einen Rest von Sittlichkeit und Schamhaftigkeit besitzen, so vernichtet diesen die sanitätspolizeiliche Untersuchungsmaaßregel total. Die Frauensperson muß sich eine regelmäßige Untersuchung ihrer Geschlechtstheile gefallen lassen, nicht etwa damit sie vor ansteckender Krankheit bewahrt werde, nein, sondern damit eine Mannsperion, die in Betreff der Ansteckung den weiblichen Körper nicht im mindesten zu respectiren braucht, ja nicht möglicherweise eine Ansteckung erleide. Die Untersuchte wird durch die sanitätspolizeiliche Maaßregel in ihren Menichenrechten demmaßen gekränkt und verletzt, daß man sie zum Abtritt der geschlechtlichen Defecation herabwürdigt. Würde die sanitätspolizeiliche Untersuchung nur bei den Dirnen in Anwendung gebracht, die syphilitisch krank gewesen und erwiesenermaßen Männer angesteckt haben, so hätte die Maaßregel wenigstens noch einen Schein von Veredlung für sich, der ihr so ganz abgeht.

Wo bleibt aber eine Spur von Gerechtigkeit und Billigkeit, wenn wir sehen, daß zu diesen sanitätspolizeilichen Untersuchungen die Subjecte nach Willkür ausgelesen werden? Die zur Exploration Verdammten müssen sich mit eigenen Augen überzeugen, daß sie unter ihren Genossinnen ausgelesen sind und sich die Schmach müssen gefallen lassen, während andere um kein Haar bessere Dirnen, weil sie von Vornehmen unterhalten werden, weil sie Protection haben, schön eingerichtet sind, einen hohen Preis auf ihre Gunst setzen, kurz aus nichtswürdigen Gründen von den Untersuchungen verschont bleiben. Der Staat, der den gewerbmäßigen Betrieb der Unzucht nimmermehr autorisiren darf, mengt sich durch seine sanitätspolizeiliche Maaßregel hinein und regulirt das unsittliche Gewerbe.

Man hat entgegnet, ein feiltes Frauenzimmer könne dadurch, daß sie sich zur regelmäßigen Exploration stellen müsse, sehr wenig gekränkt werden. Wir müssen diesen Einwand, durch Erfahrung belehrt, ablehnen. Alle Puhleirnen, wenn sie nicht bis zur niedrigsten Staffel herabgestiegen sind, fühlen tief das Schmachvolle der Untersuchung.

Wenn ein Frauenzimmer wegen unverschuldeter Krankheit an den Geschlechtstheilen von dem Arzte sich untersuchen läßt, so geschieht dies freiwillig und in der Ueberzeugung, die Untersuchung müsse behufs richtiger Erkennung und Behandlung der vorhandenen Krankheit geschehen. Wenn aber eine Buhldienerin regelmäßig sanitätspolizeilich untersucht wird, so geschieht dies wider ihren Willen, sie wird gezwungen, sie ist gar nicht krank, aber sie muß die Organe der Zeugung entblößen, untersuchen lassen, als wäre sie krank. Das kranke Frauenzimmer bezieht seine Schamhaftigkeit der Gesundheit halber, die Buhldienerin soll seine Schamhaftigkeit mehr behalten und die noch vorhandene der Behörde opfern. Werden die sanitätspolizeilich zu Untersuchenden in größerer Anzahl in einem von der Behörde bestimmten Lokale untersucht, so kommen noch mehrere Uebelstände hinzu. Die Befreier der Dienen, die ihre Selbsterniedrigung wohl begriffen haben und dies nicht gern aller Welt möchten wissen lassen, müssen sich an den Ort der Untersuchung begeben, wo die Nachbarschaft sie in das Lokal eintreten sieht, wo sie mit anderen Dienen zusammenkommen, die sie nicht kennen, von welchen sie auch nicht gekannt sein wollen, mit Dienen, deren Freizügigkeit sie selbst verabscheuen. Wenn dann endlich nach jahrelangen Untersuchungen die Dienen ohne schamhafte Aufregung sich der Maaßregel unterziehen, dann hat die Maaßregel, deren sonstiger Nutzen so zweifelhaft ist, unzweifelhaft alle Scham vernichtet.

Wie steht es aber mit den Ärzten, die zu dem Geschäfte der Untersuchung verpflichtet sind? Sie dürfen sich durchaus nicht als Ärzte betrachten, die Kranke untersuchen und ihren Rath, ihre Hilfe ertheilen, auch nicht als Gerichts- und Polizeiarzte, welche Krankheits-symptome zu erörtern haben, nein als Schamreißer, die bei gewissen Frauenpersonen, mögen sie sonst gesund oder krank sein, gleichgültig, syphilitische Geschwüre aufspüren sollen; als Abreißer-Inspektoren für die geschlechtliche Defäkation solltriger Burschen. Dem Privatärzte steht es frei, Buhldienern auf ihr Ansuchen regelmäßig über Syphilis zu untersuchen, er kann das Gesuch annehmen oder zurückweisen. Aber dem Polizeiarzte wird durch die Untersuchungsmaaßregel eine Verpflichtung aufgetragen, die wohl kaum in seinen Bereich gehört. Die Polizeiarzte sind angewiesen, mit größter Sorgfalt zu untersuchen,

sie müssen sich daher des Mutterspiegels bedienen, müssen dieses Instrument nach jeder Untersuchung selbst reinigen, müssen bei widerwärtigen und schmutzigen Frauenzimmern den Schlamm der Grube wegwischen und abtupfen, um der Behörde die Nachricht zu geben, der Grund der Grube sei in Ordnung. Kann man voraussetzen, daß die Ärzte solche Untersuchungen mit dem Eifer und dem Interesse fortzuführen im Stande sind, wie sie wirklich Untersuchungen zu machen stets geneigt sind? Und dann zuletzt, nachdem das elchhafte Untersuchungs-geschäft beendet ist, dann muß der Arzt auch den Gewerbschein der Buhldienerin noch unterzeichnen, damit diese auf seinen Namen sich empfehlen kann.

Der Werth der sanitätspolizeilichen Untersuchungsmaaßregel ist so gering anzuschlagen, daß man sagen kann, es wird dadurch aus dem Strome der Syphilis, der ungehindert seines Wegs zieht, löffelfeise etwas Weniges herausgeschöpft, wodurch den Strom geschwächt zu haben, man sich doch unmöglich einbilden kann. Wäre aber die sanitätspolizeiliche Untersuchungsmaaßregel wirklich in Bezug auf Syphilis sehr hilfreich, so würde sie immer noch, oder vielmehr erst recht verwerflich sein, weil die Hilfe der niedrigsten Unzucht zu Gute käme; weil das letzte Motiv, die Furcht vor Ansteckung noch wegfiele, welches, so unlauter es sein mag, doch thatsächlich gar Manche dazu gebracht hat, ihre geschlechtliche Genußsucht zu mäßigen.

Der Einbruch in die Sittlichkeit, den die sanitätspolizeiliche Untersuchungsmaaßregel veranlaßt, ist so groß, daß er den geringen und zweifelhaften Nutzen derselben tausendfach überwiegt.

Wir haben den sanitätspolizeilichen Untersuchungsmaaßregeln deswegen eine so lange Erörterung gewidmet, weil in Bezug auf dieselben viele Fachmänner von Kenntniß sich haben verleiten lassen, sich dem gedankenlosen Urtheile des Publikums anzuschließen und die Dringlichkeit und Nothwendigkeit derselben zu bejahen.

Noch einige wenige Worte müssen wir über jene statistischen Berichte sagen, die man ausgearbeitet hat, um durch Zahlenverhältnisse den Nutzen der sanitätspolizeilichen Untersuchung bezüglich der Anstreuung und Verbreitung der Syphilis in's Klare zu setzen. Ist je die empirische Methode der Statistik, die unter Umständen treffliche Be-

lehrung geben kann, unverkündig angewendet werden, so ist es hier der Fall gewesen. Wie will man in einer großen Stadt die Zahl der syphilitischen Erkrankungen ermitteln? Die Spitäler umfassen nur einen kleinen Theil der zur Zeit syphilitisch Erkrankten, der vielmal größere Theil der Syphilitischen befindet sich in Privatbehandlung und ein nicht unbeträchtlicher Theil Erkrankter läuft ohne alle Behandlung herum. Nimmt man nun die Liste der Spitäler, so wird man finden, daß das eine Jahr mehr, das andere Jahr weniger Personen mit Syphilis zur Behandlung gekommen sind, es würde aber falsch sein hieraus den Schluß ziehen zu wollen, die Syphilis habe in dem einen Jahre zugenommen, in dem anderen Jahr sich vermindert. Da die Hospitäler nur eine kleine Anzahl syphilitischer Erkrankungen enthalten, so kann die Zahl der im Spitale aufgenommenen Syphilitischen keine Spur von Anhalt über den Stand der Syphilis geben, und es ist wohl möglich, daß bei vielen Hospitalfällen dennoch die Syphilis in sehr geringer Weise verbreitet ist, während sie bei wenig Hospitalfällen enorm herrschen kann. Die syphilitischen Erkrankungen in der privatärztlichen Praxis und die frei herumlaufenden Syphilissubjecte sind der Zählung und Schätzung nicht zugänglich. Zählt man nun bloß die Frauenspersonen, die bei den sanitätspolizeilichen Untersuchungen syphilitisch krank gefunden wurden, so hat man ein noch viel kleineres Material als die Hospitäler liefern, welches noch weniger Haltpunkte für eine allgemeine Beurtheilung giebt. Kann man sich wohl verleiten lassen, wenn in einem Jahre bei den Untersuchungen ein paar syphilitische Gewerbburen weniger gefunden worden sind, anzunehmen, die Syphilis in der Stadt habe abgenommen? Ebenfalls wenig läßt sich aus einer größeren Anzahl erkrankt gefundener Dinnen schließen, daß auch mehr Männer erkrankt sein, denn dazu müßten wir den Anspruch, den die Untersuchten und krank Gefundenen erhalten haben, verfolgen können. Werfen nun die Verächter der statistischen Berichte noch die Fälle von primärer und secundärer Syphilis untereinander, mischen sie noch die Condylome, Menorrhoeen, ja gar auch die Krätze mit bei, so kommen ganz unbrauchbare Anekdoten heraus und alle Schlußfolgerungen aus denselben müssen thöricht ausfallen. Um nur ein paar Pöbchen derartiger Statistik anzuführen,

erinnern wir daran, daß in Berlin dieselben Zahlenverhältnisse über die syphilitischen Erkrankungen von Gewerbburen von Verschiedenen benutzt worden sind, um die Nützlichkeit des Vordellwesens und die Schädlichkeit desselben in Bezug auf Syphilis darzuthun; daß ein Hospitaldirector, der die syphilitisch Kranken mit Hunger, Schwißen, Schmierien und Varieten behandelte, aus dem Umstande, daß Niemand mehr in das Spital kam, den Schluß zog, seine Bestrebungen hätten die Syphilis sehr vermindert. —

Wir sind durch geschichtliche Studien und durch Erfahrung zu dem Schluß gekommen, von dem wir überzeugt sind, daß er die Bestimmung der tüchtigsten und angesehensten Syphilidologen finden wird: die Syphilis hat in der Neuzeit, d. h. in diesem Jahrhundert im allgemeinen einen immer mildereren Charakter angenommen. Diesen mildereren Charakter verdankt sie aber keineswegs den prophylaktischen und therapeutischen Bestrebungen der Aerzte, sondern dem Umstande, daß sie, wie alle seit Alters her bekannten ansteckenden, verärrigen, epidemischen und endemischen Krankheiten im Verlaufe der Jahrhunderte sich verändert hat. Wenn aber ärztliche Bestrebungen in Bezug auf Vorbeugung und Behandlung der Syphilis wirklich von Nutzen gewesen sind, so können die bisher getroffenen sanitätspolizeilichen Maaßregeln ein solches Lob nicht in Anspruch nehmen. —

Wir wollen jetzt kürzlich die Maaßregeln andeuten, die geziemender Weise und mit Rücksicht auf Nutzen gegen die Prostitution im Allgemeinen ergriffen werden können.

Was zuerst die Präventivmaassregeln betrifft, welche die Entstehung der Prostitution beschränken sollen, so haben die Staaten bisher diesem wichtigen Punkte kaum einige Aufmerksamkeit geschenkt. Der weibliche Theil des niederen Standes der Bevölkerung, der am meisten unter der Prostitution leidet, muß hierbei hauptsächlich berücksichtigt werden. Da die vierzehn- bis fünfzehnjährigen Mädchen, welche in die Welt geschoben werden, um für sich selbst zu sorgen, unmöglich jene festen moralischen Grundsätze haben können, welche sie nöthig haben, um den Anfechtungen der Sittlichkeit getrübt entgegenzutreten, so wird es zweckmäßig sein, den Unterricht, die Belehrung noch weiter fortzusetzen, Schulen zu errichten, welche nach der Confirmation auf die

Befestigung der moralischen Grundsätze bei den Mädchen hinarbeiten. Die Herrschaften, namentlich in großen Städten, müssen aufgefordert und angehalten werden, sich ihrer weiblichen Diensthboten mehr als bisher anzunehmen; sie dürfen in denselben nicht bloß Individuen sehen, die man nach Gefallen annimmt und ablohnt, sondern sie müssen sich gewöhnen ihre Diensthboten als Familienglieder anzusehen, für deren moralische Ausbildung sie Sorge zu tragen die Verpflichtung haben. Es muß darauf gesehen werden und dies ist namentlich in großen Städten wichtig, daß die Mädchen in Fabriken oder die Putzmacherinnen, Näherinnen u. s. w. nicht so unter aller Würde bezahlt werden, daß sie mit ihrem Verdienste nicht auskommen können. Die Männer und Frauenzimmer in Fabriken beschäftigt sind, da müssen die Arbeitsloale für beide Geschlechter getrennt werden. Eltern, Vermünder, Arbeitgeber müssen eine größere Theilnahme an der Sittlichkeit ihrer Töchter, Mündel und Arbeiterinnen betheiligen. Die öffentlichen Vergnügungen der niederen Stände müssen beschränkt, beaufsichtigt und überwacht werden u. s. w. Daß alle diese angeordneten Maasregeln auf Widerstand und selbst auf unüberwindliche Hindernisse stoßen, glauben wir wohl, immerhin wird aber das fortgesetzte Bestreben ihnen Eingang zu verschaffen viel leisten und der geistliche Stand und Lehrstand wird seine Beihilfe nicht versagen.

Die Maasregeln, um die bestehende Prostitution in Schranken zu halten, haben die doppelte Aufgabe, einmal zu verhindern, daß die einfache Unzucht nicht zur Lohn- und Gewerbdunzucht herabfinke, und andererseits, daß durch die letztere die öffentliche Sittlichkeit nicht verletzt werde. Die Polizei hat die Arbeit zu controliren, dem arbeitslosen Herumtreiben, Ausliegen, dem Betteln zu steuern, das Eindringen in die Häuser mit Kleinigkeiten zum Verkauf zu verbieten. Erlaubt die Polizei, daß ein Mädchen zwar Arbeit hat, aber nebenbei Lohnhurelei treibt, oder daß ein Mädchen bloß von Gewerbdunzucht lebt, so wird dasselbe zur besondern Beaufsichtigung notirt. Die Polizei wacht nun, daß solche Dinen nicht öffentlich die Sittlichkeit verletzen, sich nicht Männer anlockend überall sehen lassen und herumtreiben, daß sie weder auffällig noch frech gekleidet sind, in den Häusern, wo sie wohnen, keinen Skandal oder sonstiges Aergerniß veranlassen. Ver-

halten sich die Huhldinnen still und ruhig, benehmen sie sich dem Anscheine nach anständig, so lasse man sie walten, während, wenn sie das Gegentheil thun, nach Verwarnung sie für ihre Frechheit und für ihren Ungehorsam gestraft werden. Unfolgsame Auswärtige werden nach der Strafe fortgeschickt, weggelesen. Die Wirtschaften, Vergnügungsorte und Tanzorte, deren Wirthe den Huhldinnen Eintritt gestatten, müssen ganz besonders aufmerksam überwacht werden, damit weder von männlicher noch von weiblicher Seite die Sittlichkeit in irgend einer Weise verletzt werde. Doch was sollen wir weiter alle jene Maasregeln verfolgen, welche der Polizei zur Ueberwachung der bestehenden Prostitution zu Gebote stehen? Auf den Einwand, den Einige vorbringen könnten, wenn die Gewerbdunzucht nicht mehr straffällig wäre, so ließe sich gegen die Frechheit der Huhldinnen gar nichts mehr vornehmen, brauchen wir nicht weiter einzugehen. Niemand wird es der Polizei freitig machen ihre Thätigkeit und Gewalt gegen alle übelberücktigten Personen zu richten.

Maasregeln, welche tief gefallenen Prostituirten den Rücktritt zur Ehebarkeit anbahnen, sind sehr schwer in's Werk zu setzen. Man sorge für freiwillige Arbeitshäuser mit humaner Behandlung (nicht Strafhäuser), man gründe Vereine von achtbaren Damen, welche die Reuigen auswärts unterbringen, ihnen Arbeit, Dienst verschaffen, welche denen, die gar nichts gelernt haben, keine Arbeit verschaffen, vorerst Belehrung und Unterricht ertheilen lassen. —

Was soll aber aus der Syphilis werden, wenn die Huhldinnen nicht mehr inspicirt werden? Wird sich die Krankheit nicht rasend verbreiten und die Population untergraben? Wir glauben es nicht und haben den Beweis bezüglich Leipzigs in der Hand; hier wurde bis vor Kurzem nur sehr wenig sanitätspolizeilich und meist nur nach gegebenen Anzeigen explorirt und die Syphilis war weder rasend verbreitet, noch untergrub sie die Bevölkerung. Viel besser als durch die unzulässigen und unzulänglichen sanitätspolizeilichen Untersuchungen der notorischen Lohnhuren kann auf folgende Weise die Syphilis beschränkt werden. Die Polizei nimmt die Anstichungsanzeigen entgegen und untersucht sie; die krankgefundene Dirne wird zur Behandlung gebracht, die Polizei muß aber auch ebenso die Anzeige einer Dirne in

Bezug auf einen kranken Mann annehmen und verfolgen. Nach Einrichtung besonderer Krankenanstalten, Kliniken und Polikliniken muß gefordert werden, daß jeder Unbemittelte des einen oder anderen Geschlechts nicht nur unentgeltlich Rathes sich erhalten, sondern auch geeignete Behandlung finden kann. Ist die Polizei human, hört das Strafen und Maaßregeln in Bezug auf syphilitische Erkrankung auf, sind die Rathungen und Krankenanstalten human, so wird dadurch der Verheimlichung, die in Bezug auf die Weiterverbreitung der Syphilis so großen Schaden stiftet, vorgebeugt. —

Die vorstehenden Besprechungen und Erörterungen waren unerläßlich, um den Standpunkt zu bezeichnen, den der Staat bei den in Rede stehenden Vergehen und Verbrechen einnehmen muß, und um den Weg zu bezeichnen, den der Staat einzuschlagen hat, um gerechten Anforderungen Genüge zu leisten.

Wir kommen jetzt zu dem letzten Abschnitte unserer Arbeit, zur Kritik der Maaßregeln, die ganz neuerdings in Leipzig nach Vorgang der Residenzstadt Dresden eingeführt worden sind, um der Prostitution wirksam entgegenzutreten. —

Die Polizeidirection Dresdens, die sehr wohl fühlen mochte, daß derjenige Theil der Prostitution, der die Lohn- und Gewerbsunzucht umfaßt, sehr im Argen liege, glaubte besser und eingreifender als bisher ihre Wirksamkeit betheiligen zu müssen, und schritt im Jahre 1853 zur Einführung von Maaßregeln, die in einem besonderen Regulativ zusammengestellt waren. Nachdem die Maaßregeln des Regulativs fünf Jahre etwa in Anwendung gekommen waren und wie die Polizeidirection das Zeugniß (sich selbst) gegeben, ihre guten Folgen gehabt hatten, gab auch im Jahre 1858 das königl. Ministerium des Innern seine Genehmigung. Die Ausübung der gedachten polizeilichen Maaßregeln wurde vom Ministerium gebilligt, dabei jedoch hervorgehoben, daß das gesetzlich bestehende Princip, wonach gewerbmäßige Unzucht, Beförderung der Unzucht und Kuppelei zur Strafe gezogen werden sollen, ungeändert bleiben müsse.

Die städtische Polizei Leipzigs zögerte mehrere Jahre die Maaßregeln der Dresdener Polizei anzunehmen, nicht etwa weil in Betreff der

Gewerbsunzucht und Beförderung derselben der Zustand in Leipzig ein befriedigender gewesen wäre, sondern wohl mehr aus der Ueberzeugung, es handle sich bei den bestehenden gesetzlichen Bestimmungen hier um ein Noli me tangere. Endlich aber wurde die Polizeidirection Leipzigs veranlaßt, sich der Dresdener Polizei anzuschließen und so wurden denn im März 1861 in Leipzig die polizeilichen Maaßregeln der Dresdener Polizei angenommen und in Ausführung gesetzt.

Das nachstehende Regulativ ist die wörtliche Abschrift des Dresdener und nun auch Leipziger Regulativs.

Regulativ

über die polizeilichen Maaßregeln in der Stadt Leipzig zur Beschränkung des Prostitutionswesens und Verhütung der Lustfeuche.

Zu Beschränkung des Prostitutionswesens in der Stadt Leipzig und zu Verhütung der daraus für die Gesundheit, die allgemeine Sittlichkeit und Wohlfahrt zu befürchtenden nachtheiligen Folgen wird das unterzeichnete Polizeiamt nach folgenden Bestimmungen verfahren.

§. 1.

Frauenzimmer, welche der gewerblichen Unzucht überführt oder verdächtig sind, sowie der Kuppelei überwiesene oder verdächtige Personen sind besonderer polizeilicher Ueberwachung unterstellt.

§. 2.

Als verdächtig der gewerblichen Unzucht, beziehentlich der Kuppelei werden unter andern erachtet:

- a) Personen, welche wegen dieser Vergehen zur Untersuchung gekommen und nicht völlig freigesprochen sind;
- b) diejenigen Frauenzimmer, welche als der gewerblichen Unzucht verdächtig, bereits auf Anordnung der Polizeibehörde der ärztlichen Untersuchung unterworfen gewesen sind;

- c) Frauenzimmer, welche mit Personen der unter a. gedachten Art zusammen wohnen oder wiederholt besonders zur Abend- oder Nachtzeit in deren Wohnung oder mit ihnen auf Straßen, öffentlichen Plätzen und Spaziergängen betroffen worden sind;
- d) Frauenzimmer, welche sich auf Straßen, öffentlichen Plätzen und Spaziergängen die Anlockung von Mannspersonen zu Schulden bringen;
- e) Frauenzimmer, welche zur Polizeihast gekommen und bei der ärztlichen Untersuchung als syphilitisch krank befunden worden sind;
- f) die weiblichen Diensthöten der der Kuppelerei überwiesenen oder verdächtigen Personen;
- g) Personen, welche den der gewerblichen Unzucht überführten oder verdächtigen Frauenzimmern wiederholt Auflage bei sich gestatten;
- h) Frauenzimmer, welche im Aufse gewerbliche Unzucht zu treiben stehen und über ihren Unterhalt nicht genügende Auskunft zu geben vermögen.

§. 3.

Ueber die in §§. 1 und 2 gedachten Personen sind von dem dazu beauftragten Beamten genaue Verzeichnisse unter jedermaliger Angabe der Wohnungen zu führen.

§. 4.

Die gewerbliche Unzucht überwiesenen oder verdächtigen Frauenpersonen, welche sich in den späteren Abendstunden oder zur Nachtzeit in den Promenaden, auf öffentlichen Plätzen, Straßen oder öffentlichen Wirthschaften herumtreiben, sind sofort zur Haft zu bringen, und ist wegen des weiteren Verfahrens wider sie im geeigneten Wege Entschliessung zu fassen.

Die Polizeiofficianten haben sich in der Verhaftung also betroffenen Dirnen durch den von denselben zu brauchenden Vorwand, bei ihrem abendlichen oder nächtlichen Herumtreiben erlaubte Zwecke verfolgt zu haben, niemals beirren zu lassen.

§. 5.

In derselben Weise ist gegen diejenigen Dirnen zu verfahren, welche zu irgend einer Tageszeit auf Straßen, öffentlichen Plätzen oder Spaziergängen Mannspersonen anlocken, oder sonst auf eine freche, den öffentlichen Anstand verletzende Weise sich benehmen.

§. 6.

Alle zur Polizeihast gekommenen der gewerblichen Unzucht oder sonst eines niedrigen Lebenswandels überführten oder verdächtigen Frauenzimmer sind daselbst der Untersuchung durch einen der hierzu besonders verpflichteten Aerzte zu unterwerfen und werden, sobald sie syphilitisch krank oder kräftig sind, sofort dem Georgenhause oder nach Befinden dem Jacobshospital zur Aufnahme empfohlen.

§. 7.

Eine gleiche polizeiliche Erörterung des Gesundheitszustandes kann bei einzelnen Dirnen dann eintreten, wenn dazu der Behörde durch glaubwürdige Anzeigen besondere Veranlassung gegeben worden ist.

§. 8.

Unbeschadet der in den §§. 6 und 7 gedachten Untersuchungen sind die der gewerblichen Unzucht überführten und verdächtigen Frauenpersonen mindestens alle zwei Wochen durch die Polizeiarzte einer regelmäßigen Untersuchung zu unterziehen.

§. 9.

Die der §. 8 gedachten Untersuchung zu unterwerfenden Frauenpersonen sind hierzu unter dem Bedeuten zu bestellen, daß sie im Falle des Ausbleibens oder nicht rechtzeitigen Erscheinens ohne vorherige triftige Entschuldigung, der sofortigen Requisition und wegen Ungehorsams polizeilicher Gefängnißstrafe sich zu gewärtigen haben, auch ist gegen sie eintretenden Falles demgemäß zu verfahren. Diejenigen Frauenpersonen, bei denen Zeichen von Lustsuche sich vorfinden, sind unverzüglich dem Georgenhause, oder nach Befinden dem Jacobshospital zu übergeben.

Sollten Dirnen sich freiwillig öfterer, als im §. 8 vorgeschrieben ist, polizeilich exploriren lassen wollen, so können sie sich zu dem Ende bei den in jeder Woche drei Mal im Polizeiamtsgebäude stattfindenden unentgeltlichen Untersuchungen einfinden.

§. 10.

Jede der regelmäßigen polizeilichen Untersuchung unterliegende Frauenperson erhält ein Buch, in welches die Polizeiarzte den jedesmaligen Befund der ärztlichen Untersuchung einzutragen haben.

Dieses Buch ist nach jeder Untersuchung dem Polizeibeamten, welcher mit Führung der in §. 3 erwähnten Verzeichnisse beauftragt ist, vorzulegen und dabei ein etwaiger Wohnungswechsel anzuzeigen.

§. 11.

Die in §. 8 gedachte Untersuchung geschieht unentgeltlich. Es bleibt aber denjenigen Frauenpersonen, welche sich den allgemeinen in den Räumen des Polizeihauses vorzunehmenden polizeiarztlichen Untersuchungen nicht unterwerfen wollen, freigestellt sich in ihren Wohnungen — vorausgesetzt daß sich solche dazu eignen — durch die Herren Polizeiarzte alle zwei Wochen untersuchen zu lassen. Sie haben aber dafür eine Gebühr von — 10 Mgr. — für jedes Mal zu entrichten, welche in die Polizeikasse fließt und nach jeder Untersuchung bei Vorlegung der Quittung an den in §. 10 gedachten Polizeibeamten abzuführen ist.

Diejenigen, welche sich in Verhütung dieser Gebühr säumig erweisen, oder irgend wie die polizeiarztliche Untersuchung in ihren Wohnungen vereiteln oder erschweren, werden unnachlässig zu der allgemeinen, im Polizeihause stattfindenden Untersuchung gezogen.

§. 12.

Den prostituirten Frauen ist das Herausliegen aus den Fenstern und das Stehen an den Hausthüren durchaus bei Vermeidung von Gefängnißstrafe verboten.

Eben so müssen

§. 13.

die Fenster in dem Aufenthaltsorte der prostituirten Frauenpersonen mit dichten Gardinen gehörig verschlossen sein und ist eine übermäßig und auffallende Erleuchtung der Wohnungen nicht erlaubt. Auch dürfen derartige Frauenzimmer im Schauspielhause das Amphitheater, die Logen ersten Ranges, die erste Gallerie und das Parquet nicht betreten.

§. 14.

Die Wohnungen der der Kuppelei verdächtigen oder überwiegen Personen sind polizeilich öfters zu revidiren, um zu ermitteln, daß nicht etwa Frauenzimmer sich daselbst aufhalten, welche weder daselbst in Buch eingetragen, noch für das betreffende Quartier mit Aufenthaltskarte versehen sind.

§. 15.

In allen den Schenkwirthschaften, welche sich in Häusern befinden, wo prostituirte Frauenzimmer wohnen oder verkehren, muß Punkt 11 Uhr Abends geschlossen und etwa daselbst stattfindende Musik — soweit denn

Abhaltung den betreffenden überhaupt nachgelassen ist — darf nicht länger als bis 10 Uhr Abends ausgedehnt werden.

Auch ist es verboten, jungen unselbstständigen Leuten, z. B. Schülern der hiesigen Lehr- und Erziehungsanstalten, Lehrlingen u. d. den Zutritt zu gestatten.

§. 16.

Bei öffentlichen Tanzmüßten ist auf die der Prostitution ergebenden Frauenzimmer strenge Obacht zu führen, auch sind an solchen Tanzabenden die nächsten Umgebungen der betreffenden Schankstätten, besonders deren, wo die Dirnen zu verkehren pflegen, wohl im Auge zu behalten.

Nicht minder sind alle Dirnen, denen der Besuch der öffentlichen Schankstätten von dem Polizeiamte ausdrücklich verboten ist, von den Tanzsälen wegzurufen und zur Anzeige zu bringen, auch, dafern sie nicht Folge leisten, zu arretilren.

§. 17.

Bei Revision der sogenannten Kuppelstuben ist mit Vorsicht zu verfahren, insbesondere sind dabei die betroffenen Mannspersonen in der Regel unbehelligt zu lassen, es müßte denn Verdacht vorliegen, daß dieselben sich absichtlich dort versteckt hielten und eine nachweisliche sonstige Wohnung nicht hätten.

Leipzig, am 31. März 1861.

Das Polizei-Amt der Stadt Leipzig.

Sollen wir dieses Regulativ wirklich einer ausführlichen Kritik unterwerfen? Wir können uns Zeit und Mühe sparen, weil derartige bessere und umfassendere Regulative aus dem verwichenen Jahrhunderte bekannt sind, die trotzdem sich nicht bewährt haben und bei Seite gelegt worden sind. Wir erinnern beispielsweise an die polizeilichen Verordnungen Berlins im Jahre 1782 und später. Wir wollen daher nur in kurzen Zügen unsere Bedenken gegen die Maßregeln des Regulativs vorbringen.

Ziehen wir den prunkenden und ruhmredigen Titel „Regulativ über die polizeilichen Maßregeln in der Stadt Leipzig zur Beschränkung

des Prostitutionswesens und Verhütung der Lustseuche" in Betracht, so werden wir nothwendig zu der Ansicht gedrängt, als hätten wir die Resultate eines höheren Calculs vor uns, dem es endlich gelungen wäre Maaßregeln zu finden, welche das böse Uebel der Prostitution an der Wurzel angreifen, beschränken und nebenbei auch noch die Lustseuche verhüten. Lesen wir indessen weiter, so werden wir enttäuscht, und nachdem wir uns durch das Regulativ durchgearbeitet haben, finden wir in demselben nichts Anderes, als allbekannte polizeiliche Uebervachungsmaaßregeln, wie sie überall gegen sich herumtreibende, lästliche Frauenpersonen seit jeher in Anwendung gekommen sind und noch angewendet werden. Eigenthümlich sind die Culturmaaßregeln gegen die gewerbmäßigen Beförderer der Unzucht. Aber, wenigstens für Leipzig neu sind auch die regelmäßigen polizeiarztlichen Untersuchungen der Puhleirnen, die Gesundheitsbücher und Gewerbscheine.

Wir wollen nichts darüber sagen, daß die Prostitution nach dem Regulativ in der engen Bedeutung von Gewerbsunzucht aufgeführt wird, allein die prahlende Ueberschrift müssen wir denn doch entschieden zurückweisen. Kann man sich wirklich einbilden, daß polizeiliche Maaßregeln, welche, wenn sie gehörig gehandhabt werden, allerdings das freche Gebahren der Puhleirnen zu zügeln und in Schranken zu halten vermögen, aber keineswegs die Unzuchtsovergehen hindern, sondern nur Puhleirnen zwingen den äußeren Schein zu wahren, daß solche Maaßregeln im Stande sind, auf die Prostitution, deren Wurzeln so tief liegen, irgend einen beschränkenden Einfluß zu üben? Noch härter würde es aber sein, wenn man sich einbilden wollte, die regelmäßigen polizeiarztlichen Untersuchungsmaaßregeln könnten nunmehr die Syphilis verhüten. Was den eifrigen Forschungen der Aerzte nach Jahrhunderten nicht gelungen ist, nämlich die Syphilis zu verhüten, das sollten diese neuen ärztlichen Untersuchungsmaaßregeln aus der Dreßdener Schule erreichen? Es kommt leider häufig vor, daß eitrige Wüther dem Titel, den sie führen, nicht entsprechen, und so ist denn auch der Titel der polizeilichen Maaßregeln unpassend ausgefallen. Wenn Autoren, die ihren Büchern falsche Titel gegeben haben, sich herbe Zurechtweisung müssen gefallen lassen, so handeln wir in Bezug auf die

Autoren des Regulativs gelind, wenn wir bloß den Vorwurf der Unmündigkeit und des falschen Gebrauchs der Muttersprache geltend machen.

Doch zur Sache, zur Betrachtung der Art und Weise, wie man die Maaßregeln des Regulativs in Leipzig in Thätigkeit gesetzt hat.

Alle der Gewerbsunzucht überwiesenen und derselben verdächtigen Frauenpersonen sollen nach dem Regulativ unter spezielle polizeiliche Aufsicht kommen und sich zu regelmäßigen ärztlichen Untersuchungen stellen, eben so werden die der Kuppelerei dringend verdächtigen Personen der speziellen Aufsicht unterstellt.

Die Leipziger Polizei hat nun 300 und etliche Dimen zur Aufsicht und zu den ärztlichen Untersuchungen notirt, von welchen gegen 240 in Bordellen beschäftigt sind. Wie glücklich wäre Leipzig, wenn in seinem Reichthum nur die geringe Zahl von 300 der Lohn- und Gewerbsunzucht überwiesenen oder derselben dringend verdächtigen Frauenpersonen sich vorfänden! Leider ist dies aber nicht der Fall, denn wenn wir alle Puhleirnen zusammenrechnen und diejenigen mitzählen, die unter der Hand mitmachen, die auf dem Lande, vor der Stadt wohnenden Dirnen, die auch ihr Wesen in der Stadt treiben, so beträgt die Summe weit über 3000. Die Polizei hat also von den der Gewerbsunzucht verdächtigen Frauenpersonen gar keine notirt, sondern sie hat nur unter den Lohn- und Gewerbsunzucht die bekanntesten ausgelesen. Sollte sich die Polizei die Mühe nehmen, allen verdächtigen Frauenzimmern nachzuspüren, so würde sich die Wichtigkeit unserer Angabe vollkommen bestätigt finden. Die Polizei weiß dies auch selbst recht gut, allein sie vermeidet es, die erdrückende Lawine sich aufzuwälzen. Schlimmer ist es noch mit den der Beförderung der Unzucht überwiesenen oder verdächtigen Personen; die Polizei hat bloß 50 unter Aufsicht gestellt, und darunter befinden sich 48 Wirthe oder Wirtheinnen von Bordellen; Personen, welche Abtheilungsquartiere halten, welche gelegentlich zur Unzucht Zimmer überlassen, einzelne Gelegenheitsmacher und Nachtrinnen, sowie Kuppeler und Kuppelerinnen nach dem Sinne des Strafgesetzbuchs sind gar nicht in Aufsicht genommen worden. Von verdächtigen Kuppelstuben kann bei den 48 in Leipzig

vorhandenen thatsächlichen Bordellen nicht die Rede sein. Treten wir in die vulgo Kuppelstuben ein, so finden wir in denselben von der Polizei eingeschriebene Dienstmädchen versammelt, die auffallend angezogen sind, manchmal in Sammt und Seide prangen, unverhältnißige Haar Touren und auch mehr oder weniger entblößten Busen zur Schau tragen, so daß schon der Anblick hinreicht, den Charakter der Dienstmädchen und den Zweck der Wirtschaft zu beurtheilen. Man findet nicht bloß bis 10 oder 11 Uhr Abends, sondern bis 2, 3 Uhr Morgens Eintritt in diese Wirtschaften; Getränke aller Art werden in denselben für betrügerische Preise in der schlechtesten Qualität verabreicht, obgleich den meisten die Schankgerechtigkeit mangelt. Da die Polizei dies alles nicht zu wissen vorgiebt, obgleich sie es sehr gut weiß, so erhellt daraus die Art der Beaufsichtigung, die geführt wird, und die nach dem Regulativ eine sorgfältige sein soll. Will die Behörde einen strengen Beweis haben, daß die genannten 48 Bordelle diesen Namen verdienen, so kann sie diesen auf mannigfache Weise erlangen, sie kann sogar, wenn sie einige Geldkosten nicht scheut, ermitteln, wie viele Male in den gedachten Wirtschaften der bezahlte Beischlaf täglich vollzogen worden ist, sie kann von den Dienern und von ihren Besuchern die zuverlässigsten Berichte erhalten. Wo bleibt aber das Regulativ? Auf dem Papiere.

Wenn die Polizei wirklich mit möglichster Sorgfalt alle Loh- und Gewerbhuren und die Verdächtigen aufhöbert, wenn die Polizei alle Bordelle und Abtheilungsquartiere visitirt und somit dem Regulativ vollständig nachkame, so würde sie dann nach den Gesetzen nichts anderes thun dürfen, als die Untersuchung gegen alle diese Personen einzuleiten und die Voruntersuchungen den Gerichten zur weiteren Untersuchung und Verurtheilung zu übergeben. Da dies die Polizei aber nicht einmal mit den 300 notorischsten Gewerbhuren, nicht einmal mit den 48 Bordellwirthin und Wirthinnen thut, so geht hervor, daß mit den Maaßregeln des Regulativs die Handhabung der Gesetze genau in derselben Weise wie früher, ehe das Regulativ noch nicht in Wirksamkeit gesetzt worden war, umgangen wird.

Die Maaßregeln des Regulativs selbst aber sind vollständig ungesegnet, sie ordnen ein Verfahren gegen Gewerbhuren und Bordelle

an, welches den gesetzlichen Bestimmungen durchaus zuwider läuft. Der Gewerbhurer verdächtige Frauenspersonen, die zur Untersuchung gebracht werden sollten, läßt die Polizei alle laufen, Gewerbhuren, die sofort nach dem Gesetze eingekerkert und bestraft werden sollten, läßt die Polizei bloß ärztlich exploriren, und die Bordellwirthin und Wirthinnen werden bloß aus der Entfernung überwacht. Man hat zwar bei der Abfassung der Maaßregeln des Regulativs vermieden, irgend etwas darüber zu sagen, daß die notirten Dienern und Bordellwirthin so ein Geschäft treiben dürfen; man nennt die Diener solche, die einmal wegen Gewerbdunzucht bestraft worden sind, und von welchen man nur vermuthet, daß sie nebst den Verdächtigen Gewerbdunzucht zu treiben geneigt wären, man bezeichnet die Bordelle als verdächtige Kuppelstuben, allein diese geschräubte Fassungswiese kann den sachlichen Verhalt in keiner Weise ändern.

Wie läßt sich die ministerielle Genehmigung der Maaßregeln des Regulativs in Einklang mit den Gesetzen bringen? Das Ministerium genehmigt die Maaßregeln, die den Gesetzen durchaus entgegenarbeiten, mit der Beifügung, daß das Princip der gesetzlichen Bestimmungen in keiner Weise gefährdet werden dürfte. Alle Logik, die uns inne wohnt, zusammengenommen, vermögen wir uns mit bestem Willen aus diesem Dilemma nicht herauszufinden. Wenn gewisse Gesetze unbedingt aufrecht erhalten werden sollen, und dennoch Maaßregeln eingeführt und gebilligt werden, die den Gesetzen direct entgegenarbeiten, nun so muß unsere Mißbilligung der ministeriellen Genehmigung, sowie der ungesegneten Maaßregeln des Regulativs eine allgemeine Billigung erlangen.

Noch einmal kommen wir auf die Bordelle. Wie genau und ausführlich sind die polizeilichen Maaßregeln, welche vor achtzig Jahren in Berlin unter ministerieller Erlaubniß eingeführt wurden! sie enthielten genaue Vorschriften, wie der Bordellwirth sich gegen die Dienern und das Publikum zu verhalten, wie die Diener sich zu benehmen hätten; jede Zuwiderhandlung wurde gestraft. Wie ungenau sind dagegen die Maaßregeln des Regulativs in Bezug auf die Bordelle! die Wirthin und Wirthinnen der Bordelle in Leipzig schinden die eingehandelten Dienern, prellen das Publikum mit theuren und schlechten

Getränken und Niemand hindert sie. Die Berliner Verordnung konnte deswegen so genaue Bestimmungen treffen, weil sie der Wahrheit die Ehre gab. Gleich im ersten Paragraph der Verordnung heißt es: *gefeßlich erlaubt ist diese Wirthschaft freilich nicht, sie wird aber als ein unvermeidbares Uebel geduldet.* Wahrlich, statt mit prunkender Ueberschrift hätte das Leipziger Regulativ besser damit angefangen, seine Maasregeln zuerst als nicht gefeßliche zu bezeichnen, dann würde man auch zweckmäßigere Bestimmungen haben treffen können. Wenn in Berlin das alte Regulativ, nachdem es mehrere Jahre streng gehandhabt worden war, dennoch aufgegeben wurde, so lag dies daran, daß man mit Recht anfang, an der Nothwendigkeit der Bordelle zu zweifeln, daß die Gewerbschurerei außerhalb der Bordelle so zugenommen hatte, daß die Bordelle ganz überflüssig wurden. Die wachsende Bevölkerung und wachsende Prostitution, namentlich die zunehmende Gewerbschurerei, haben in Berlin immer neue und wechselnde Maasregeln hervorgebracht; daß man aber noch nicht zu einer bestimmten Entscheidung gekommen sei, zeigt namentlich der Umstand, daß in Berlin innerhalb von fünfzig Jahren fünfmal Bordelle gestattet, geduldet und wieder abgeschafft worden sind.

Die Bordelle mit ihren schandbaren und nichtswürdigen Wirthin und Wirthinnen dürfen aber um keinen Preis geduldet werden, die Behörde hat sie unumschüßlich zu verfolgen, so daß sie nicht länger als wenige Tage aufkommen und bestehen können. Wenn die Unzucht für Vohn eine Unflute ist, die in großen Städten stets florirt wird, und die keine Maasregeln der Behörde zu verbannen je im Stande sind, so ist dies mit Bordellen etwas anderes, diese müssen und können auch beseitigt werden und auf Duldung dürfen sie keinen Anspruch machen.

Es ist vielfach das Nüchlichkeitsprincip hervorgehoben worden, und für Leipzig könnte man dieses Nüchlichkeitsprincip in Bezug auf die Duldung der Bordelle umso mehr geltend machen, weil hier durch die Messen ein temporäres Zustromen einer großen Menge von Männern bedingt wird. Worin soll aber der Nutzen, den man von den Bordellen erwartet, bestehen? Fürchtet man etwa, daß ohne diese trefflichen Institute ehbare Frauen und Mädchen sich gar nicht dürften auf den Straßen und Spaziergängen sehen lassen, ohne rohen und

brutalen Angriffen ausgesetzt zu sein? Hier müssen wir die Partei der Fremden nehmen, die eben auch aus gestüteten Ländern und Staaten kommen. Sollten sich aber einige fremde Männer wirklich so vergehen und die Straßen für Frauenzimmer unsicher machen, nun so dächten wir doch, die Polizei hätte Tag und Nacht, energisch und solcher Rohheit bald gründlich steuernd einzugreifen. Die Polizei wird aber doch nicht erklären, sie könne ohne Bordelle die Ordnung und öffentliche Sittlichkeit nicht erhalten, sie sei ohne diesen Beistand (!) zu schwach? Die Erfahrung hat gelehrt, daß in großen Städten mit der Zunahme der männlichen Bevölkerung, wenn sonst im Lande kein Mangel an Frauenzimmern ist, sich immer eine ausreichende Zahl von Buhldirnen bildet; sobald man nun in Leipzig die Buhldirnen nicht besonders verfolgt, so wird auch die Zahl derselben stets genügen und mit der temporär wachsenden Nachfrage sich vergrößern. Sollte dies aber wirklich nicht der Fall sein, so hat doch die Behörde für die Bildung und Herbeischaffung derselben weder direct noch indirect sich zu bemühen. Wir wiederholen nochmals, wäre der Nutzen der Bordelle unbefristbar, was nicht der Fall ist, so darf nimmermehr das Sittlichkeitsprincip durch das Nüchlichkeitsprincip zurückgedrängt werden. Oder geht die einseitige Anschauung der Verteidiger des Nüchlichkeitsprincips so weit, daß sie sagen: Leipzig gewinnt durch die zu den Messen herbeiströmenden Fremden außerordentlich, Leipzig hat auch für die Bequemlichkeit und Belustigungen der Fremden Sorge zu tragen, daher müssen für die Fremden Bordelle mit Buhldirnen zur Auswahl angeschafft werden. Will der Fremde Unzucht treiben, so möge er nach willsfähigen weiblichen Subjecten suchen, und er wird sie, wir sind dessen gewiß, ohne große Mühe finden; entgegenzutragen braucht man sie ihm nicht. Sollte der Fremde keinen Spitzgeist haben, sollte er wirklich klagen, daß in Leipzig gar keine Etablissements existiren, sollte er dieses Umstands halber nicht mehr nach Leipzig kommen, nun gut, so mag er warten, bis er nach Hause kommt, mag wegbleiben, wenn er es für besser findet, Leipzig wird ohne solche Herren Leipzig bleiben. Einen einzigen Nutzen untergeordneter Art können die Bordelle allerdings wohl haben. Da Diebe und Betrüger den Besuch von Bordellen zu lieben pflegen, so wird die Polizei bei gehörigen Revisionen der Bordelle

und unter Beihilfe der Dinnen und Wirthe gar manche Diebstähle und Betrügereien zu ermitteln im Stande sein, die vielleicht sonst unentdeckt geblieben wären. Allein diese Erleichterung der polizeilichen Nachforschungen ist doch von zu geringem Belang, und es würde von mehr Belang sein, wenn die Polizei damit anfänge, die Diebstähle und Betrügereien der Bordellwirthe und Wirthe an den unglücklichen Dinnen und die Pöbelereien des Publikums zu legen.

Das Regulativ ist besorgt, und zwar mit Recht, daß der Schein der Sittlichkeit gewahrt werde, es verbietet den Dinnen alles Herumtreiben, Stehenbleiben, Anlocken, Anreden u. s. w., es verbietet, daß die Häuser, wo die Dinnen sich befinden, nicht auffallend erleuchtet sind, daß die Dinnen nicht an den Thüren stehen, zum Fenster heraussehen u. s. f. Alle diese Maassregeln stehen zur Zeit nur auf dem geduldeten Papiere, in Wirklichkeit aber ist das Herumtreiben der Lohnhuren an allen Orten, das freche Betragen in jeder Weise noch niemals in Leipzig so groß gewesen, als gerade jetzt nach Einführung des neuen Regulativs. Will man einen Beweis haben, wie weit der Creß, der Verstoß gegen die Sittlichkeit geht, so braucht man nur zu Mittag gegen Abend durch eine bekannte, mit Bordellen besetzte Gasse der Vorstadt zu gehen. Ein Strom von Männern und Knaben wogt hier auf und ab; schamlose Dinnen stehen an den Thüren oder schauen aus den Fenstern, rufen die Männer an, unterhalten sich mit ihnen, es wird gekost, gelacht, allein es entspinnt sich auch Zank; die Dinnen werden verhöhnt, geschimpft, schimpfen wieder und man bekommt Redensarten zu hören, wie sie Keuschheit und Sittenlosigkeit nur erfinden und aussprechen kann.

Wo bleibt das Regulativ? Auf dem Papiere.

Wir wollen auch theilweis die Polizei entschuldigen. In neuerer Zeit hat man das Ansehen der niederen Polizeibeamten dergestalt zu untergraben beliebt, daß dieselben unter Auflauf und Skandal einzeln sich zu mischen gar nicht wagen dürfen, wenn sie nicht insultirt und selbst thätlich insultirt werden wollen. Der Trunkenbold, der froweränte Pummeler, der nichtsnutzige Straßenjunge widersetzt sich dem Polizeidiener, und das Publikum unterstützt sie und findet Vergnügen daran, die Polizeiofficianten verspottet zu sehen. Was sollen sich die niederen

Beamten mit Arreturen befassen, wo sie Insultirung zu gewärtigen haben? Zu der Beaufsichtigung, Anordnunghaltung der Gewerbhuren gehören aber zuverlässige und einsichtsvolle Diener, Diener, wie sie die Polizei nur wenige hat, und die sie besser anderwärts zu verwenden meint. Mit ein paar Officanten, die die ganze Masse in Ordnung halten sollen, ist nichts zu machen.

Die Polizei erscheint aber auch in unseren Augen gar nicht besonders tadelnswerth, wenn sie den Maassregeln des Regulativs keine Geltung verschafft; wenn die Polizei den Anforderungen der Gesetzbestimmungen nicht nachzukommen vermag, oder ihnen nicht nachkommen will, wie wir bei Betrachtung der Handhabung der Art. 354 und 355 gesehen haben, so braucht sie viel weniger Bedenken zu tragen den ungeheuerlichen Maassregeln des Regulativs nicht Folge zu leisten. Warum man dann aber das Regulativ angenommen, eingeführt hat, das bleibt allerdings räthselhaft.

Wir wenden uns zu den regelmäßigen ärztlichen Explorationen, die das Regulativ angeordnet hat, über deren Ungläubigkeit und Nichtigkeit wir schon oben und ausführlich verbreitet haben. Die kleine Anzahl der zur ärztlichen Untersuchung designirten Dinnen, gegenüber der großen Zahl von vorhandenen Lohnhuren, zeigt, daß die Maassregel eine Fünftelmaassregel sei, und die, da die Besucher der Lohnhuren nicht untersucht werden, ganz zusammenfällt. Wiederholen wir nochmals: die ganze Sorglosigkeit für die Gesundheit der Geschlechtstheile der Gewerbhuren ist, und wenn die Untersuchungen viel weiter ausgedehnt würden, nur eine Begünstigungsmaassregel der Gewerbhurer selbst und als solche verwerflich.

Wir wollen nicht davon reden, daß das Regulativ wiederum das unglückliche Wort Lussfucht braucht, müssen aber doch noch erwähnen, daß den Ärzten überhaupt gar keine Vorschriften gegeben sind, bei welchen Krankheiten der Geschlechtstheile sie auf Absonderung und Hospitalbehandlung der Dinnen dringen sollen. Ob die Ärzte blos bei primärer Syphilis oder auch bei secundärer, bei Gonorrhöen, Blennorrhöen, blennorrhagischen Geschwüren u. s. w. einschreiten haben?

Die Zusammenstellung der Kräfte mit der Lustsuche, der wir im Regulative begegnen, erscheint Alzzen höchst komisch.

Einige Worte noch über die Bücher mit dem ärztlichen Gesundheitsattesten, die den untersuchten Dirnen nach dem Regulative eingehändigt werden. Woher diese Bücher? Was braucht die Dirne ein Buch, wenn die Behörde durch ihre Alzzen erfahren hat, daß die Dirne zur Zeit frei von Syphilis sei? Es scheint die Absicht vorzuliegen, als sollten die Untersuchten die Bücher als Certificate benutzen dürfen, um sich den Männern als empfehlenswerthe reine Waare anpreisen zu können. Mit solchen Büchern kann auch noch außerdem durch Verleihen Mißbrauch getrieben werden. Für die Zuverlässigkeit der Bücher können aber, wie wir schon oben auseinandergelegt haben, die Alzzen nicht garantiren. Die Bücher sind ganz unverlässlich, weil sie den Dirnen nicht nur eine indirecte Berechtigung zur Gewerbszunucht erteilen, sondern sogar eine directe. Das Buch ist nicht anderes als ein Gewerbschein.

Der letzte Paragraph des Regulatives hätte wohl füglich weggelassen werden müssen, er empfiehlt (natürlich den Officianten) Vorsicht bei der Revision der sogenannten Kuppelstudien, d. h. der richtigen Bordelle, namentlich sollen Mannspersonen in der Regel unbehellig gelassen werden. Ihr Herrn, fürchtet euch nicht, laßt euch nicht abhalten die Bordelle zu besuchen, wir sind discret, unsere Revision ist nicht so ernstlich gemeint.

Zum Schlusse fügen wir noch die Abdrücke jener kleinen Regulative bei, welche die Leipziger Polizei den Buhldirnen und Bordellwirthcn überantwortet. Auch diese Regulative sind abermals indirecte Concessionen, die sich bemühen dem Geseze gegenüber den Schein zu wahren.

A.

Polizeiamt Leipzig.

den 18
Auf Verhüllung erscheint heute an Polizeiamtsstelle

und erklärt zunächst auf Befragen:

Ich bin	zu
geboren, habe	Altern (Vormund), welche
in	wohnen, habe bisher durch
bin mit einer auf	meinen Lebensunterhalt gewonnen und
Legitimation in No.	gültigen
	der
versehen und bisher	Strafe
	Stod
	bestraft worden.

Derselben wird eröffnet, daß sie wegen dringenden Verdachts eines unkeuschen Lebenswandels bis auf Weiteres unter specieller Aufsicht gestellt werde und erhält sie demzufolge die Bedeutung, daß sie sich des Herausliegens aus den Fenstern, des Stehens an den Hausstufen, des Anlockens von Mannspersonen und des beschäftigungslosen Umherziehens in den Straßen und auf den Promenaden, namentlich während der Abendstunden schlechterdings zu enthalten habe.

Es wird derselben ferner unterlagt, jungen, unselbstständigen Leuten, wie z. B. Schülern der hiesigen Lehr- und Erziehungsanstalten, Lehrlingen u. s. w. den Zutritt bei sich zu gestatten und ihr aufzugeben, die Fenster ihrer Wohnung stets mit dichten Vorhängen zu verschließen, sie auch bescheiden, daß das übermäßige Erleuchten ihrer Zimmer zur Abendzeit ebensowenig werde geduldet werden, als etwa darin vorzunehmende Gesänge, schallendes Gelächter, Musik u. s. w.

Hiernächst wurde der Comparsentin ausdrücklich verboten, über Nacht Jemanden zu beherbergen und ihr eröffnet, daß bei dem Besuche des hiesigen Theaters ihr der Zutritt an folgenden Plätzen bei ernster Abmahnung und sofortiger Begreifung unbedingt verboten sei:

Wenden, Verhüllung.

- a) im Amphitheater,
- b) in den Parterresalzen,
- c) im Parquet,
- d) in den Logen des ersten und zweiten Ranges.

Weiter wird der Comparsantin aufgegeben, sich aller 14 Tage wenigstens einmal der polizeiarztlichen Untersuchung zu unterwerfen und hierbei reinlich und sauber zu erscheinen und ihr gleichzeitig eröffnet, daß diese Untersuchungen

a) unentgeltlich

alle Montage, Mittwoch und Freitage Vormittags von 10 bis 11 Uhr in dem hiesigen Polizeiamts-Gebäude,

b) gegen eine Gebühr von 10 Mgr.

in ihrer Wohnung, an den von den Herren Polizeiarzten hierzu bestimmten Tagen und Stunden

stattzufinden haben, ihr hierbei auch bekannt gemacht, daß, wenn sie sich öfter als alle 14 Tage untersuchen lassen wolle, ihr dies freistehe und sie sich zu dem Ende bei den Montag, Mittwoch und Freitage stattfindenden Untersuchungen einfinden könne.

Nicht minder wird die Comparsantin angewiesen, daß sie das von dem betreffenden Polizeiarzte ausgestellte Gesundheits-, resp. Befundsanzeigeblatt der etwa zu zahlenden Gebühr jedesmal ungesäumt an den desfalls beauftragten Registrator abzugeben, bei diesem auch außer der gewöhnlichen Meldung im Einwohner-Bureau jede Wohnungsänderung anzuzeigen und in dem Falle, wo sie aus triftigem Grunde zur ärztlichen Untersuchung nicht erscheinen könne, ihre bezügliche Entschuldigung alsbald anzubringen habe.

Schließlich wird die Comparsantin beschieden, daß in Folge dieser gegen sie verfügten speciellen Polizeiaufsicht und wegen Uebervachung der genauesten Befolgung der ihr in Vorstehendem ertheilten Weisungen die Polizeibeamten der Criminalabtheilung und der Bezirkswache ermächtigt sind, nicht bloß am Tage, sondern namentlich auch zur Abend- und Nachtzeit Anwesenheit bei ihr vorzunehmen und daß sie bei etwaigen Contraventionen die Anwendung von Zwangsmaßnahmen und Gefängnisstrafen nach Verhältnissen auch und insbesondere im Wiederholungsfalle härtere Strafen zu gewärtigen habe.

Die Verhandlung ist vorgelesen und von der Comparsantin genehmigt und mit unterschrieben worden u.s.

B.

Polizeiamt Leipzig.

den 18

Auf Bestellung erscheint heute an Polizeiamtsstelle und erklärt auf Befragen:

Ich bin am geboren,
zu verheirathet
erwerbe meinen Lebensunterhalt durch
durch bin zum hiesigen Aufenthalte
legitimirt, wohne jetzt
und bin bisher
bestraft worden.

D selben wird eröffnet, daß wegen des bisher bei ihm wahrgenommenen Verkehrs leichtfertiger, unter Polizeiaufsicht stehender Frauenpersonen bis auf Weiteres Wohnung und der daselbst stattfindende Verkehr unter specieller Uebervachung gestellt werde und in dessen Folge bedeutet, daß sich bei Vermeidung harter Abmahnung der Aufnahme oder Ueberbergung jeder Frauenperson als Verhinderungs zu enthalten habe, die überhaupt nicht zum Aufenthalte in Leipzig bereits durch das Polizeiamt legitimirt ist.

Hierauf wird d Comparsant ernstlich eingeschärft, und dafür verantwortlich gemacht, daß jedes Herausliegen von Frauenpersonen aus Fenstern, Stehen an Gaus- thüren oder gar Anlocken von Mannspersonen eben so, wie das Herumtreiben auf dem Plage vor dem Hause, den Straßen und Promenaden, namentlich während der Abendstunden schlechthin nicht stattfinden und

ih . . . zur Pflicht gemacht, für Schließung der Hausthüre zur zehnten Abendstunde besorgt zu sein.

Weiter wird d . . . Comparsent . . . untersagt, jungen unehelich- ständigen Leuten, wie z. B. Schülern der biesigen Lehr- und Erziehungs- anstalten, Lehrlingen u. s. w. den Zutritt bei sich zu gestatten und ih . . . aufzugeben, die Fenster . . . Wohnung stets mit dicken Vorhängen zu schließen, . . . auch beschließen, daß das übermäßige Erleuchten . . . Zimmer zur Abendzeit eben so wenig werde geduldet werden, als etwa darin vorzunehmende Musik, laute Gesänge, schallendes Ge- lächter u. s. w.

Derner wird d . . . Comparsent . . . ausdrücklich verboten, Mannspersonen nämlich zu beherbergen und während öffentlicher Festlich- keiten und Märkte, namentlich aber während der Concerte an öffentlichen Orten sich mit mehreren unter polizeilicher Aufsicht stehenden Frauen oder Mädchen an größeren Tischen gruppenweise zusammenzusetzen. Nämlich- lich des Besuchs des Theaters ist aber Comparsent . . . bedeuert worden, daß ih . . . ebenso wie den unter Polizeiaufsicht stehenden vorerwähnten Frauenspersonen der Zutritt an folgenden Plätzen bei ernsther Abhaltung und sofortiger Wegweisung verboten sei:

- a) im Amphitheater,
- b) in den Parterreslogen,
- c) im Parquet,
- d) in den Logen des ersten und zweiten Ranges.

Sodann wird d . . . Comparsent . . . zur Nachachtung eröffnet, daß den unter Polizeiaufsicht gestellten, der Führung eines unzüchtigen Lebens- wandels verdächtigen Frauenspersonen vorgeschrieben ist, sich mindestens aller 14 Tage ein Mal der polizeiarztlichen Untersuchung zu unterwerfen, hierbei reinlich und sauber zu erscheinen und daß diese Untersuchungen

a) unentgeltlich

alle Montage, Mittwoch und Freitage im Polizeiamtsgebäude von 10—11 Uhr Vormittags,

b) gegen eine Gebühr von 10 Kr.

aber in ihrer Wohnung an den vom Polizeiarzte zu bestimmenden Tagen und Stunden

stattzufinden haben, daß aber diesen Frauenspersonen auch freistehe, sich öfterer untersuchen zu lassen und zu dem Ende bei den Montage, Mit- wochs und Freitage in jeder Woche im Polizeiamtsgebäude stattfindenden polizeiarztlichen Untersuchungen sich einzufinden.

Schließlich wird d . . . Comparsent . . . beschließen, daß in Folge dieser gegen . . . verfügten Polizeiaufsicht und wegen Uebervachung der genauesten Befolgung der in Vertheilung erteilten Weisungen die Polizeibeamten der Criminal-Abtheilung sowohl, als die Bezirkswachen ermächtigt sind, nicht bloß am Tage, sondern namentlich auch zur Abend- und Nachtzeit Revisionen . . . Wohnung vorzunehmen und daß . . . bei etwaigen Contraventionen die Anwendung von Zwangs- maßregeln und Gefängniß- oder Geldstrafen, im Wiederholungsfall auch härtere Strafen zu gewärtigen habe.

Die Verhandlung ist vorgelesen, genehmigt und mit unterschrieben worden u. s.

Aus den vorsehenden Erörterungen heben wir folgende Haupt- sätze hervor:

- 1) Der Art. 354 des sächsischen Strafgesetzbuchs ist nicht ge- recht, kann nicht in Ausführung gebracht werden und dürfte zweckmäßigerweise zu streichen sein.
- 2) Der Art. 355 bedarf einer Umstellung; in Bezug auf den gewerbmäßigen Betrieb der Beförderung der Unzucht, wie er dormalen in den Bordellen besteht, muß er seine volle Gel- tung behalten; die Behörden haben unumstößlich einzu- schreiten.
- 3) Der Staat darf nie eine Maßregel ergreifen, welche der Un- zucht und noch mehr der Gewerbmäßigkeit direct oder indirect Vorschub leistet, ihr Vertheiligung verleiht.
- 4) Das neue Regulative Leipzigs ist durchaus ungeleglich; wäre es aber auch dieses nicht, so würde es dadurch nicht an Werth gewinnen.
- 5) Das leidige Wesen der Lohn- und Gewerbmäßigkeit in großen Städten kann einzig und allein durch eine humane aber energische, durchaus energische Polizei, die den Strom zu zügeln und in Schranken zu halten den festen Willen hat und sich dabei seiner Macht, die ihr der Staat und jeder Vermünftige einräumen muß, in voller Ausdehnung bedient, in Ordnung und in dem Zustande erhalten werden, daß dadurch die Sittlichkeit, so wenig wie es überhaupt geht, leidet. —

Sollte man uns nach Lesung der Abhandlung den Vorwurf machen, wir hätten eine zu derbe Redeweise gebraucht, so entgegnen wir, daß bei den derben Uebelsänden und Mißbräuchen, bei den derben Irrthümern, die wir zu besprechen hatten, zarte Ausdrücke wenig am Plage gewesen sein würden.

Diese kurze Abhandlung haben wir mit redlichem Willen, mit Sachkenntniß und gewiß nicht ohne Befähigung geschrieben. Sollte dieselbe beitragen, zweckmäßigen Einrichtungen den Weg zu bahnen, so würden wir uns für hinlänglich belohnt halten, weder auf Lohn, noch schriftstellerische Ehre Anspruch machen, und würden erdösig sein, manches nur kurz Ange deutete weiter und ausführlicher zu erörtern, ja das ganze schwierige Feld vollständig zu bearbeiten. Gehielten wir nur die Billigung der Sachverständigen, so würden wir auch zufrieden sein. Würden uns aber auch die Sachverständigen ihre Billigung zu geben sich nicht getrauen, nun so würden wir uns mit dem Bewußtsein, der Wahrheit die Ehre gegeben zu haben, trösten und deswegen die Hoffnung auf bessere Zeiten nicht verlieren, denn magna est veritas et praevaleret. —

Druck von Otto Wigand in Leipzig.

Eine neue Milchprobe

von

Dr. Alfred Vogel,
Privatdocent und prakt. Arzt in München.

Erlangen,

Verlag von Ferdinand Enke.

1862.

Einige Mittheilungen

von G. D. Kunzmann

in Erlangen

Schnellpreßendruck von G. D. Kunzmann in Erlangen.

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Wenn Fleisch und Brod unbestreitbar die wichtigsten Nahrungsmittel im Allgemeinen sind, so nimmt die Milch doch bestimmt den ersten Platz nach diesen beiden ein. Für die Erwachsenen hat die Milch ihrer Unentbehrlichkeit nach die dritte, für Kinder aber, namentlich für die unter einem Jahre, unbedingt die erste Stelle unter allen Nährstoffen inne. Diese große Bedeutung der Milch für das Gedeihen der ganzen Menschheit ins Auge fassend, kann man sich nur wundern, wie wenig gesichert die Käufer derselben gegen Betrug und Fälschung sind.

In der That gibt es auch keinen einladenderen und bequemeren Artikel für betrügerische Manipulationen, als eben die Milch. Denn wie ist eine exakte Controle möglich, wenn das Naturprodukt selbst schon so mächtigen Schwankungen unterworfen ist?

Nach Simon's vortreflichen Untersuchungen finden sich in 1000 Theilen reiner Kuhmilch:

	Maximum.	Minimum.
Wasser:	861,0	823,0
Feste Theile:	177,0	139,0

Das specifische Gewicht der frischen Kuhmilch schwankt nach Simon zwischen 1030 und 1035 nach Scherer zwischen 1026 und 1032.

Aus einer Zusammenstellung verschiedener chemischer Analysen von Chevallier, Boussingault, Quevenne, Lecanu, Haidlen, Simon, Herberger, Poggiale,

Playfair, Regnault, Payen, Lehmann und Becquerel ergeben sich folgende procentische Schwankungen:

Käsestoff, Eiweiß und unlösliche Salze:	3,6 (Quevenne) — 7,0 (Simon)
Butter:	3,0 (Haidlen) — 4,9 (Playfair)
Milchzucker:	2,9 (Herberger) — 5,5 (Payen)
Lösliche Salze:	0,2 (Payen) — 0,7 (Herberger).

Wie kann man nun bei diesen enormen physiologischen Schwankungen ermitteln, ob eine Milch, in welcher z. B. die festen Bestandtheile im Simon'schen Minimum vorkommen, ein Naturprodukt oder in betrügerischer Absicht mit Wasser verdünnt ist? Niemand wird diese Frage lösen können und der irrende Milchverkäufer darf seine Verdünnungen trotz aller polizeilichen Controle ungestraft fortsetzen, wenn er nur so klug ist, sich auf einen bestimmten mäßigen Grad der Verdünnung zu beschränken.

In den Lehrbüchern über die Nahrungsmittel und deren Verfälschung werden zahlreiche Betrügereien der Milchverkäufer erzählt. Die Milch soll mit Stärke und Mehl, mit arabischem Gummi, Tragacanth, Dextrin, Rohr- und Stärkezucker, Eiweiß und Eigelb, Haufenblase, Hanfmilch, Mandelmilch, Gehirn, Kreidepulver und endlich gar noch mit Zinkoryd verfälscht, dickflüssiger oder dichter weiß gemacht werden. Man findet die ausführlichsten Schilderungen dieser Manipulationen bei Klencke, die Verfälschung der Nahrungsmittel und in Chevallier's Wörterbuch der Verfälschungen. Da alle hier aufgeführten Verunreinigungen einerseits sehr leicht nachzuweisen, zum Theil schon mit dem bloßen Auge erkennbar sind, andererseits mit viel zu kostspieligen Materialien ausgeführt werden, so ist einleuchtend, daß sie von routinirten Fälschern niemals in Gebrauch gezogen werden.

Eine mäßige Verdünnung mit Wasser oder ganzlich abgerahmter Milch ist außerordentlich einfach und bequem, vollkommen kostenfrei und zudem schwierig nachzuweisen, weshalb denn auch gar kein Grund vorhanden, die zum Theil mühsamen und jedenfalls kostspieligeren oben genannten Methoden zu versuchen.

Meine vieljährige Stellung als Vorstand der Reinerischen Besuchsanstalt für kranke Kinder, eines von circa 3000 Kranken jährlich frequentirten Ambulatoriums, hat nur zu oft in mir den Wunsch erregt, für das Hauptnahrungsmittel der Kinder, die Milch, eine bestimmte Garantie ihrer Güte zu ermitteln, allein alles war bisher vergebens. Ich mußte täglich mit ansehen, wie Kinder in Folge der in hiesigen Milchläden gekauften Milch Durchfall bekamen und wenn derselbe nicht in kürzester Zeit gestillt werden konnte, an Atrophie, Abzehrung, zu Grunde gingen.

Daß in München, und diese Verhältnisse sind dieselben für alle größeren Städte, kein Milchverkäufer ohne Verdünnung leben kann, läßt sich einfach durch Zahlen nachweisen. In den hiesigen Milchläden kostet allenthalben die sog. gute Milch per Maas 4 Kr., die abgeblasene 2 Kr. und der Rahm 8—24 Kr. Die meisten Milchleute, circa 400, haben keine eigenen Kühe, sondern kaufen die Milch von Dekonomen aus der Umgegend Münchens und müssen für die Maas 3 $\frac{1}{2}$ bis 3 $\frac{1}{2}$ Kreuzer bezahlen.

Sie haben also an einer Maas nur $\frac{1}{2}$ Kr. Gewinn und müssen 120 Maas täglich verkaufen, bis sie ehrlicher Weise 1 Gulden verdienen. Es könnten also erst die Milchleute nothdürftig leben, welche ein solches Quantum täglich umsetzen. Die meisten bekommen aber kaum die Hälfte, und Milchgeschäfte, welche mehr als 60 Maas täglich von den Dekonomen erhalten, werden schon zu den guten gerechnet. Daß mit 30 Kreuzer täglichem Gewinn ein Laden gehalten und eine Person, geschweige denn eine Familie bestehen kann, wird wohl Niemand ernstlich behaupten wollen. Da aber diese Milchleute sämmtlich leben, ihre Steuern, ihren Unterhalt und ihren Laden bezahlen können, so geht daraus hervor, daß man in München äußerst selten reine unverfälschte, sondern gewöhnlich nur verdünnte Milch zu kaufen bekommt.

Die mannigfachen Versuche der Sanitätspolizei, diesen Verdünnungen auf die Spur zu kommen, sind deshalb hinlänglich gerechtfertigt, sehen wir nun zu, in wie weit die bis-

her üblichen Untersuchungsmethoden ihrem Zwecke entsprochen haben. —

Eine vollkommene chemische Untersuchung der Milch ist allein im Stande, uns über den Werth und Gehalt der Milch an Nährstoffen zu belehren. Versetzt man eine gewogene Menge Milch mit $\frac{1}{2}$ ihres Gewichtes Gyps, so läßt sich dieses Gemisch ohne Schwierigkeit eindampfen und der Rückstand vollständig austrocknen. Dieser Rückstand wird gewogen und gibt nach Abzug des zugesetzten Gypses die Menge des festen Rückstandes der Milch. Aether zieht aus diesem Gemenge von trockner Milch und Gyps das Fett vollständig aus. Alkohol zieht aus demselben den Milchsucker und die in Spiritus löslichen Salze aus. Was in Spiritus und Aether unlöslich ist, ist Casein, Gyps und die unlöslichen Salze. Die Summe der Salze wird durch Verbrennung einer besonders abgedampften Quantität Milch erhalten.

Es entspricht diese Analyse allerdings den exaktesten Ansprüchen, allein sie erfordert einen geübten Chemiker und wenigstens 24 Stunden Zeit. Da in einer großen Stadt mehrere Hundert Milchläden zu überwachen sind, so bedürfte man zu deren Controle eine große Anzahl geübter Chemiker und schließlich könnte man doch niemals Milch *provi manu* confisciren, da diese Analysen erst nach vielen Stunden beendet werden können. Die chemische Analyse also ist unbrauchbar für eine anhaltende, ergiebige Controle.

Man hat sich jetzt ziemlich allgemein dahin verständigt, daß zur polizeilichen Ueberwachung die Untersuchung des specifischen Gewichtes der Milch mit dem Areometer genüge. In zweifelhaften Fällen soll dann die Milch noch im Gremometer angelegt und nach 24 Stunden der Rahm gemessen werden.

Die Areometer, Laktodensimeter, Galaktometer, wurden von verschiedenen Autoren verschieden angegeben und mit verschiedenen langen Hälften versehen. Im wesentlichen sind alle diese Instrumente eben einfache Centwaagen, durch welche sich längst herausgestellt hat, daß die mittlere Dichtigkeit der reinen ganzen Milch 1031 und der der abgerahmten, sog. abge-

blasenen 1033 ist, wenn wir die Dichtigkeit des defillirten Wassers zu 1000 annehmen. Mit andern Worten, wenn ein gewisses Volumen Wasser, ein Litre, 1000 Grammes wiegt, so wiegt dasselbe Volumen, ein Litre, ganze Milch 1031 und ein Litre abgeblasene Milch 1033 Grammes.

Diese Mittelzahlen liegen übrigens zwischen ziemlich weiten Grenzen, nach Simon 1030 — 1035, nach Scherer 1026 — 1032, im Ganzen also zwischen 1026 und 1035, so daß schon hieraus hervorgeht, daß kleinere Wasserbeimischungen ungestraft bleiben müssen. Der Hauptmischstand der Centwaage bezüglich der Milchuntersuchung liegt aber darin, daß einzelne Milchbestandtheile, nämlich der Käsestoff, der Milchsucker und die Salze schwerer, die Fette hingegen, die Butter, leichter als Wasser sind, woher es denn auch kommt, daß die abgeblasene Milch specifisch schwerer ist als die ganze.

Klenke hat in dieser Richtung einen sehr instructiven, praktischen Versuch angestellt. Er befreite normale Rahm Milch von 1029 specifischen Gewicht von dem oben angesammelten Rahme, wog nun wieder und fand das specifische Gewicht 1031. Darauf setzte er zu derselben (es waren ursprünglich 12, und nach Wegnahme des Rahmes 11 $\frac{1}{4}$ Unzen) so viel Wasser zu, daß das ursprüngliche specifische Gewicht von 1029 wieder hergestellt wurde. Er gebrauchte hierzu 2 $\frac{1}{4}$ Unzen Wasser, und hatte also durch dieses Verfahren 7 $\frac{1}{4}$ Unzen Rahm und 14 Unzen Milch von normalem Gewicht aus jenen ursprünglichen 12 Unzen gewonnen. In solchen Fällen also, und es mögen sich deren wohl täglich ereignen, ist die Areometerprobe resultatlos und man hat deshalb für zweifelhafte Befunde, wenn die Milch sehr dünn und bläulich aussieht und doch ein normales specifisches Gewicht hat, eine weitere Untersuchung mit dem Gremometer vorzunehmen.

Das Gremometer besteht aus einem Glaszylinder, der 14 Centimeter Höhe, $\frac{3}{4}$ Centimeter Durchmesser hat und in 100 Theilstiche, die oben mit 0 beginnen, graduirt ist. Man füllt diesen Cylinder bis zum 0 Strich mit Milch, läßt dieselbe 24 Stunden lang an einem kühlen Orte stehen und lieft nach dieser Zeit die Dichte der aufgeworfenen Rahmschicht ab.

Eine gute unverdünnte Milch zeigt mindestens 10 Theilstriche Rahm. Daß man mit einem solchen Glase keine genaue Rahmbestimmung machen kann, ist längst bekannt, denn es steigen nicht alle Milchkörperchen in die Höhe, sondern nur ein großer Theil, sonst könnte die unter dem Rahm sich befindende Schichte nicht weiß bleiben. Dieses Steigen der Milchkörperchen geht auch in verschiedenen Milchsorten durchaus nicht egal und nicht in derselben Zeit von Statten und wird jedenfalls durch das Sauerwerden unterbrochen. Die Ungleichheit des Rahmabstieges hängt wahrscheinlich mit dem quantitativen Verhalten der übrigen Milchbestandtheile, des Milchsuckers, Casein's u. zusammen. Ueberdies ist die Grenze zwischen der gelblichen Rahm- und der bläulichen Milchschichte keineswegs eine so scharfe, daß man sie genau ablesen könnte.

Mit der Centwaage und diesem graduirten Glaszylinder kann man allerdings ziemlich bestimmt eine mit Wasser oder abgerahmter Milch verdünnte Milch erkennen, allein erst nach 24 Stunden den Nachweis hiefür liefern, weshalb der Verkauf einer solchen verdächtigen Milch auch nicht eingestellt werden kann. Meine am Schluß zu beschreibende optische Milchprobe wird diesem Uebelstande abhelfen.

Man hat schon mehrmals versucht, auf optischem Wege die Milch zu prüfen. Eine dieser Methoden wurde von Donné angegeben und das hiezu gehörige Instrument Laktoskop genannt. Außerdem haben Vernois, Becquerel und Poggiale den Milchsucker der Molke durch den Polarisationsapparat quantitativ bestimmt.

Das Laktoskop von Donné besteht aus einer Art Loupe mit zwei in einander verschiebbaren Röhren, welche durch parallele Gläser geschlossen sind. In den Raum zwischen diese beiden Gläser wird die zu untersuchende Milch mittels eines kleinen am oberen Theile befindlichen Trichters eingegeben, ein Handgriff befindet sich an der unteren Seite.

Das eine Glas steht fest, das andere aber ist mit einer Stellschraube in Verbindung, deren Gänge so fein gedreht sind, daß durch jede einmalige Umbrehung die beiden Gläser sich um einen Millimeter verrücken, je nach der Art der

Drehung sich nähern oder von einander entfernen. An dem Einschiebepöhrle befinden sich 50 Theilstriche und Ziffern, deren jede $\frac{1}{100}$ Millimetre gleichkommt.

Man bringt nun die zu untersuchende Milch durch den Trichter zwischen die beiden Gläser, hält hierauf das Instrument nach Art einer Lupe vor das Auge, und sieht nun durch dasselbe nach einer brennenden Kerze, welche einen Metre vom Auge entfernt ist. Der Moment, in welchem die Contur des Lichtes nicht mehr zu erkennen ist, dient als Anhaltspunkt für die Beurtheilung der Milch. Ist die Milch arm an Milchfädelchen, so muß man, um das Licht verschwinden zu machen, die Gläser von einander entfernen, die Milchschichte verdicken; ist sie im Gegentheile reich an Milchfädelchen, so muß man die Gläser einander nähern, die Milchschichte verdünnen. Der Rahmgehalt wird also durch die Grade der Skala am Einschiebepöhrle angezeigt. Gute Kuhmilch gibt am Laktoskope ungefähr die Ziffer: 30. Dieses sehr sinnig erdachte Instrument, dessen ich übrigens in ganz München nicht habhaft werden konnte, hat sich, obwohl es schon vor vielen Jahren angegeben worden, nirgends rechten Eingang verschafft. Der Grund seiner Unpopularität liegt wohl darin, daß es 1) ziemlich kostspielig ist, und daß 2) die feine Mikrometerschraube mit Flüssigkeiten in Berührung, wie dieß beim Reinigen des Instrumentes nicht zu vermeiden ist, jedenfalls in kurzer Zeit rosten und gänzlich unbrauchbar werden muß.

Der zur Bestimmung des Milchsuckers dienende Polarimeter von Vernois und Becquerel besteht aus einem hohlen 0,30 Metre langen und 0,02 Metre im Durchmesser haltenden Rohre, dessen beide Enden mit Nicol'schen Prismen versehen sind. Die Lichtstrahlen fallen durch das vordere Prisma oder den Polariseur ein, das zweite oder der Analysateur wird vor das Auge gebracht. Wird das einfallende Licht bei leerem Rohre und bei einer solchen Stellung der Prismen untersucht, daß deren Hauptflächen in paralleler Richtung zu einander stehen, so sieht man die Lichtstrahlen in ihrer höchsten Intensität, dreht man aber den Analysateur um die Are

des Rohres und hebt somit den Parallelismus der Prismen auf, so hört jede Lichterscheinung auf, sobald die Hauptflächen in einem rechten Winkel zu einander stehen. Bringt man nun eine Flüssigkeit in das Rohr, welche auf die Polarisationsebene einwirkt, so muß man den Analysator auf verschiedene Weise bald mehr, bald weniger drehen, um die Dunkelheit wieder herzustellen. In der Milch hat nun fast ausschließlich der Milchsucker die Eigenschaft, das Licht zu polarisiren, denn das in der Milch vorhandene Eiweiß, das auch eine Abweichung nach links bewirkt, kommt nur in verschwindend kleiner Menge vor.

Vernois' und Becquerel's Verfahren zur Ermittlung des Milchsuckers und mittels dieser zum Nachweise der Milchverdünnung ist folgendes: Die Milch wird rasch zum Coaguliren gebracht, die Molke in das Rohr gegossen und die Deviation sowohl bei Tageslicht als auch bei Kerzenlicht untersucht.

Es ist allerdings möglich, auf diese Weise den Milchsucker in der Molke exact zu bestimmen. Allein die Bereitung einer durchsichtigen Molke mittels Ansäuerung oder Kochen und Filtriren erfordert immer schon eine viel zu lange Zeit für täglich öfters wiederholte polizeiliche Controle und zu dem ist unglücklicher Weise der Milchsucker gerade jener Milchbestandtheil, welcher überhaupt den größten physiologischen Schwankungen unterworfen ist. Wie oben gezeigt worden enthalten 1000 Theile Milch 20—55 Theile Milchsucker und es kann demnach die Controle nur mit größter Nachsicht geübt werden. Eine beträchtliche Abnahme des Milchsuckers findet nur bei bedeutendem Wasserzusatz statt, während eine Verdünnung der ganzen Milch mit abgerahmter gar keine Veränderung im Gehalt an Milchsucker zur Folge hat. Diese letztere Art der Fälschung ist gerade die gewöhnlichere. Uebrigens ist auch dieses Instrument wieder viel zu kostspielig, als daß es eine allgemeine Verbreitung finden könnte.

Nach reiflicher Ueberlegung wurde es mir klar, daß der Gedanke Donné's, die Durchsichtigkeit der Milch als Anhaltspunkt für ihre Güte zu nehmen, den meisten Erfolg verspricht,

zugleich aber sah ich ein, daß der Weg, den Donné hiefür vorgeschlagen, gänzlich zu verlassen ist.

Die unzählig vielen Versuche, die ich als Assistenzarzt auf der Pfenzerischen Klinik mit Harnuntersuchungen vorzunehmen hatte, erregten in mir die Idee, ob nicht auch eine Titrimethode für die Milch gefunden werden könnte. Ich versuchte das Einfachste und Rächstliegende, das Verhältniß der Milch zu klarem Wasser, die Mischung einer gewissen Menge Milch mit einer ebenfalls gemessenen Menge Wasser.

Mein erster Versuch bestand darin, daß ich in einem geschliffenen, flachen Flacon, wie solche zur Aufbewahrung der Parfümerien zuweilen in Gebrauch sind, 5 CC. Milch goß und hierauf so viel Wasser zusetzte, daß, wenn man durch die parallelen Glasflächen des Flacon durchsah, der Lichtkegel einer brennenden Kerze dahinter gesehen werden konnte. Es war hiezu eine erstaunlich große Menge Wasser, über 50 CC. nöthig. Um diesen großen, allmäligen Zusatz von Wasser zu vermeiden, kehrte ich nun den Versuch um. 20 CC. Wasser wurden in denselben (gegen 80 CC. haltenden) Flacon gegossen und nun aus einer graduirten Pipette so viel Milch zugelegt, bis der Lichtkegel der Kerze nicht mehr gesehen werden konnte. Zu diesem Zwecke mußte ich 1,8 CC. Milch zugeben. Der Versuch wurde wiederholt und wieder ebenso viel Milch bis zum Unsichtbarwerden des Lichtes verbraucht. Mehrfach angestellte Versuche belehrten mich, daß bei einiger Uebung immer bei demselben Tropfen Milchzusatz der letzte Schein des Lichtkegels verschwand. Es war also nun ein vortrefflicher Anhaltspunkt gegeben:

Eine gemessene Schicht Wasser zwischen zwei parallelen Gläsern wird durch eine und dieselbe Quantität Milch immer so undurchsichtig, daß man hiedurch ein Licht nicht mehr erkennen kann.

Ich verdünnte nun dieselbe Milch mit $\frac{1}{10}$, $\frac{1}{5}$, $\frac{1}{2}$ u. Wasser und fand, daß eine um so größere Quantität dieser verdünnten Milch zugelegt werden mußte, je ergiebiger die Verdünnung vorher vorgenommen wurde.

Die nächste Aufgabe war nun, zu ermitteln, welche Modificationen dieser Versuch erfährt 1) durch die Dicke der Wasserschichte, 2) durch die Dicke der Gläser, 3) durch die Quantität des zur Probe benützten Wassers und 4) durch die Art und Entfernung des dahinter gehaltenen Lichtes.

Schon beim ersten, ebenbeschriebenen, rohen Versuche bemerkte ich, daß, als die Trübung ziemlich bedeutend geworden, an einzelnen Stellen das Licht noch erkannt werden konnte, während es an anderen, gegen die Mitte und den Boden des Flacon zu, vollständig verschwunden war. Es rührte diese Erscheinung offenbar davon her, daß das Gläschen in der Mitte und am Grunde etwas mehr ausgebaucht war als oben und am Rande. Die trübe Wasserschichte war daher an den ersteren Stellen dicker als an den letzteren. Je dicker aber eine trübe Flüssigkeitsschichte ist, um so mehr nähert sie sich einer vollkommenen Undurchsichtigkeit.

Um den Einfluß der Dicke der Wasserschichte zu prüfen, mußten natürlich andere genauere Instrumente hergestellt werden. Es war nicht leicht, zwei Glasplatten parallel und wasserdicht zu einer bestimmten inneren Dichtung mit einander zu verbinden. Zuerst nahm ich eine dünne Wachskerze, ein Stück von einem Wachsstock, bog es hufeisenförmig, erwärmte es gelinde und verklebte damit zwei viereckige Glasplatten von 10 Ctmr. Länge und 8 Ctmr. Breite. Man konnte hiemit wohl einen oder einige Versuche machen, das Wachs löste sich aber immer bald an einzelnen Stellen vom Glase ab und mit dem wasserdichten Verschlus war es vorbei. Mehrere Glas-techniker wurden nun zu Rathe gezogen. Dieselben fertigten mir wohl einzelne brauchbare Instrumente, indem sie mit Schellack und anderen Glasfitten zwei Glasplatten so miteinander verbanden, daß die innere Dichtung 5 — 6 Millimeter betrug. Allein nach einiger Zeit sprangen diese Ritze mehr oder weniger und die verhältnismäßig ziemlich theuern Instrumente waren nicht mehr wasserdicht.

Wie es zuweilen zu gehen pflegt, das nächstliegende fiel mir zuletzt ein. Mit dem gewöhnlichen Glasertitt, der täglich viel tausendmal zum Einrahmen der Fensterscheiben benützt

wird, gelang es mir leicht und ohne alle Kosten, eine größere Anzahl von Instrumenten herzustellen, welche jetzt schon über einen Monat gehalten haben und deßhalb längere Dauer versprechen. Man walzt solchen Glasertitt glatt aus und läßt die Masse ungefähr 2 MM. dick, als man die innere Dichtung, (für den gewöhnlichen Gebrauch am besten 5 MM.), des zu verfertigenden Instrumentes wünscht, schneidet ihn dann in Streifen, verklebt hiemit die Ränder der Glasplatten an drei Seiten und drückt sanft mit den Fingern die Glasplatten so lange näher zusammen, bis die gewünschte Distanz erreicht ist. Nach 4 — 5 Tagen hat dieser Kitt schon so viel Festigkeit erlangt, daß man mit einiger Vorsicht diese Gläser ganz wohl benutzen kann. Ich fertigte mir nun eine Reihe von Instrumenten an, welche durch verschieden dicke Kittschichten mit einander verbunden waren und prüfte ihre Empfindlichkeit auf die Reaktion. Die hierzu verwendeten Glasplatten waren 10 Ctmr. lang, 8 Ctmr. breit und hatten eine Dicke von $1\frac{1}{2}$ MM.

Der Unterschied war ein ganz enormer. Von ein und derselben abgerahmten Milch brauchte ich auf 10 CC. Wasser bei 4 MM. Gläserdistanz $8\frac{1}{4}$ CC., bei 12 MM. Gläserdistanz auf die gleiche Quantität Wasser nur 3 CC. Milch bis zur Undurchsichtigkeit.

Folgende Versuchsreihe wurde vorläufig in dieser Richtung angestellt:

Versuchsobjekt: Abgerahmte Milch.
Wassermenge: 10 CC.

Gläserdistanz.	Milchverbrauch bis zur Undurchsichtigkeit.
4 Millimeter.	0,85 Cubikcentimeter.
4 MM. (anderes Instrument)	0,85 CC.
5 MM.	0,60 CC.
$6\frac{1}{2}$ MM.	0,50 CC.
9 MM.	0,35 CC.
10 MM.	0,32 CC.
12 MM.	0,30 CC.

Es erhellt aus diesen Experimenten der Satz:
Je kleiner die Gläserdistanz um so größer der Milchverbrauch und je größer die Gläserdistanz um so kleiner der Milchverbrauch.

Die Abnahme des Milchverbrauches steht jedoch in keinem geraden Verhältniß zu der Gläserdistanz. Sie ist um so fühlbarer, je näher die Gläser sich berühren. Zwischen 1—5 MM. Distanz haben wir einen Unterschied von $2\frac{1}{2}$ CC. Milch ($8\frac{1}{2}$ —6 CC.) zwischen 9—12 MM. Distanz aber ist nur mehr ein Unterschied von $\frac{1}{2}$ CC. Milch ($3\frac{1}{2}$ —3 CC.). Wenn die Gläser weiter auseinander rücken, verliert die Reaktion an Feinheit, rückt man sie aber zu nahe zusammen, so dauert es erstens sehr lange bis die Undurchsichtigkeit eintritt, zweitens müßte man größere als die oben bezeichneten Glasplatten nehmen um die kleine Menge von 10 CC. Wasser zu trüben, und drittens mischt sich bei so naher Berührung der Gläser die Milch nur langsam und ungleichmäßig mit dem Wasser. Diesen beiden letzteren Uebelfänden kann allerdings abgeholfen werden, indem man die Mischung der Milch mit dem Wasser nicht in dem Probeglas mit den parallelen Flächen, sondern in einem andern Gefäße vornimmt und Proben davon in das eigentliche Probeglas gießt. Eine zu große Annäherung der Glasflächen ist jedoch auch aus dem Grunde nicht rathsam, weil die Gläser in diesem Falle sehr genau auf einander gepaßt sein müßten und es schwer halten dürfte, einen so hohen Grad von Genauigkeit durch Kittung zu erreichen.

Nach vielfachen Versuchen habe ich eine Gläserdistanz von 5 MM. = $\frac{1}{2}$ Centimeter als die praktischste gefunden. Die Mehrzahl meiner provisorisch angestellten Versuche, welche weiter unten ausführlicher geschildert werden sollen, wurde mit einem aus Glasfritt selbst gefertigten Probeglas von $\frac{1}{2}$ Ctmr. innerer Dichtung auf 10 CC. Wasser angestellt. Von langer Dauer sind diese Probegläser übrigens nicht, da durch öfteren nicht ganz vorsichtigen Gebrauch sie zusammengebrückt und somit ungenau werden. Die von Greiner jetzt gefertigten Probegläser sind in Messing gefaßt und verspre-

chen deshalb neben größerer Genauigkeit eine längere Dauer. Was die Dichte der Gläser, der Glasmasse, betrifft, so konnte ich keinen Einfluß auf die Reaktion bemerken. Ich fertigte Instrumente, deren Gläser von 1—5 MM. dick waren und konnte, so fern sie nur rein weiß und ihre Distanzen die gleichen waren, keinen Unterschied bemerken.

Daß die Quantität des zur Probe benützten Wassers von keinem Belang für die Reaktion sein werde, d. h., daß man zu einer größeren Menge Wasser auch eine entsprechend größere Menge Milch brauchen müsse, ließ sich erwarten. Der Vollständigkeit wegen wurden auch hierüber Versuche angestellt und zwar in einem besonders weiten Glase von 12 MM. innerer Dichtung.

Es waren in diesem Glase von einer abgerahmten Milch nöthig zu:

10 CC. Wasser	—	$2\frac{1}{4}$ CC. Milch
20 CC. "	—	$5\frac{1}{2}$ CC. "
30 CC. "	—	$8\frac{1}{4}$ CC. "
40 CC. "	—	11 CC. "

Das Verhältniß der Milch zum Wasser ändert sich also in keiner Weise, mag man den Versuch mit 10 oder mit 40 CC. Wasser anstellen.

Ueber das Material und die Entfernung des zur Reaktion verwendeten Lichtkegels wurden auch mehrfache Versuche angestellt. Es hat sich hierbei herausgestellt, daß es gleichgültig ist, ob man mit Gas-, Lampen- oder Kerzenflamme experimentirt. Am bequemsten ist eine Stearinkerze, da sie sehr reinlich, leicht zu transportiren und anzuzünden ist.

Die Entfernung des Instrumentes vom Lichtkegel ist ebenfalls von keinem besonderen Belang. Man kann sich dem Lichte bis auf einen Schuh nähern und bis auf 10 Schuh von demselben entfernen, ohne besondere Veränderung zu bemerken. Bringt man das Instrument mit dem milchtrüben Wasser unmittelbar an das Licht, so erscheint die ganze Flüssigkeit röthlichgelb und es sieht der wirkliche Lichtkegel deshalb weniger scharf ab. Entfernt man sich zu sehr vom Lichte, so wird der Lichtkegel wegen seiner zunehmenden Kleinheit

leicht übersehen. Es ist also nicht nöthig, die Distanz zwischen dem Lichte und dem Auge ängstlich zu bemessen. Am einfachsten steht die Kerze auf dem Tische, an welchem die Milch eben untersucht werden soll.

Von viel größerer Bedeutung ist die Entfernung des Glases vom Auge. So wie man das Instrument nur einige Millimeter vom Auge entfernt, so verschwindet der eben noch etwas sichtbare Lichtkegel vollkommen. Es muß deshalb als Regel festgehalten werden, daß man bei jedem Versuche das Glas so nahe als möglich an das Auge bringt.

Von einigem Belang ist schließlich noch die Stellung der Kerze. Stellt man die Kerze so, daß sie ein Fenster zum Hintergrund bekommt, so verschwindet der Lichtkegel früher und ist allzeit undeutlicher, als wenn der Hintergrund dunkel ist. Man muß deshalb die Untersuchung immer an einer solchen Stelle des Zimmers vornehmen, wo man den Fenstern den Rücken wendet und in das Auge nicht zu gleicher Zeit direktes Tageslicht und Kerzenchein fällt. Man thut in jedem Falle gut, das Auge durch die hohle Hand möglichst zu beschatten.

So viel von den Untersuchungen über die Möglichkeit der Fehlerquellen, welche dieser Methode vorgeworfen werden könnten.

Meine ganze Milchprobe besteht nun aus folgenden höchst einfachen Gläsern: 1) das Mißglas mit der Marke, durch welche genau 100 CC. angezeigt werden, 2) das Probeglas mit den parallelen Glasflächen, welche genau $\frac{1}{2}$ Centimeter von einander entfernt sind und 3) eine feine graduirte Pipette, mit $\frac{1}{2}$ Cubikcentimetergraden.

Ebenso einfach wie die Instrumente sind die zum Versuche gehörenden Handgriffe.

Das erste ist immer, daß die ganze zu bestimmende Milch gut und gründlich gemengt wird, denn da die Milchkügelchen leichter wie Wasser und nur suspendirt sind, so ist es klar und auch schon längst bekannt, daß nach kaum $\frac{1}{2}$ stündigem Stehen die oberen Schichten eines Milchgefäßes viel reicher an Milchkügelchen sind als die unteren. Ist die zu

untersuchende Milch in einem Hasen oder Kübel, so muß man sie mittels eines großen Löffels oder breiten Holzspanes gehörig mengen, wird sie in einer Flasche oder einem anderen enghalsigen Gefäße aufbewahrt, so kehrt man das Gefäß einige Male schüttelnd um.

Man füllt nun zweitens das Mißglas genau bis an den Strich, 100 CC., mit gewöhnlichem reinen Brunnenwasser.

Man saugt drittens in die fein graduirte Pipette die zu untersuchende Milch bis über den Nullstrich an und verhindert das Wiederausfließen derselben, indem man das obere Ende rasch durch den an die Lippen gebrachten Zeigefinger verschließt. Nachdem man durch leises Dessinen des Fingers die Milch bis zum Nullstrich entleert hat, hält man die mit Milch gefüllte Pipette in das Mißglas und entleert vor Allem 3 CC. Milch in die 100 CC. Wasser. Weniger wie 3 CC. braucht man bei gewöhnlicher Kuhmilch fast nie. Will man aber einen wirklichen guten Rahm untersuchen, so darf man davon für's erste nicht mehr als $\frac{1}{2}$ CC. dem Wasser beimischen. Hierauf schüttelt man das Mißglas, welches man mit dem Finger geschlossen hat, ein paar Mal, gießt etwas aus demselben in das Probeglas und sieht nun durch letzteres nach dem Lichte. Ist der Lichtkegel noch zu erkennen, so gießt man die herausgenommene Probe wieder zurück in das Mißglas und setzt einen weiteren CC. Milch zu, nimmt nach einigem Umschütteln wieder etwas heraus in das Probeglas und sieht von neuem nach dem Lichte. Bei einiger Übung lernt man bald den Zeitpunkt kennen, wo das Licht dem Verschwinden nahe ist, und setzt dann immer nur $\frac{1}{2}$ CC. zu. Ist die Contur des Lichtkegels auf keine Weise mehr zu erkennen, so ist die Probe beendet. Man addirt alsdann die verbrauchte Milch und weiß nun, wie viel Procent von einer Milch nöthig sind, um eine Wasserfälschung von $\frac{1}{2}$ Centimeter Dichte vollständig undurchsichtig zu machen.

Wenn diese einfachen Handgriffe einige Male geübt sind, kommt man dahin, in 3–4 Minuten eine Probe anzustellen und die Gläser wieder zu reinigen, so daß man in

einer Stunde leicht ein Duzend Milchbestimmungen zu machen im Stande ist.

Es erübrigt mir noch der Beweis, daß diese Probe sich praktisch bewährt. Folgende Versuche wurden vorläufig damit angestellt.

I. Versuche über die ungleiche Vertheilung der Milchfugelchen nach längerem Stehenlassen der Milch.

8 Unzen abgerahmte Milch wurden in einem 1 Schuh hohen Glase 6 Stunden ruhig stehen gelassen, nach welcher Zeit sie noch einmal eine papierdünne Schichte Rahm aufgeworfen hatte. Ich entnahm hierauf eine Portion Milch möglichst von der Oberfläche, indem immer nur die äußerste Spitze der Pipette beim Ansaugen eingefenkt wurde und brachte von dieser so gewonnenen Milch 6 CC. Von einer 5 Zoll unter der Oberfläche genommenen mußten aber schon 9 CC. und von der ganz dem Boden entnommenen, welche übrigens schon bei der bloßen Besichtigung ganz bläulich durchscheinend sich zeigte, 50 CC. zugefugt werden. Da ich voraus sah, daß ich zu dieser letzten Probe mehr Milch brauchen würde, als am Boden durchscheinende zu bekommen war, so nahm ich in das Milchglas statt 100 CC. Wasser nur 20 CC. und brauchte zur Trübung dieser Menge 10 CC. also auf 100 CC. Wasser 50 CC. Milch.

Es wurde hierauf die ganze noch im Glase sich befindende Milch gehörig umgerührt und geschüttelt und nochmals eine Probe 1) aus der Oberfläche, 2) aus der Mitte und 3) vom Boden des Gefäßes untersucht. In allen drei Portionen ergab sich nun die gleiche Zahl, nämlich 7 CC.

Ein noch größerer Unterschied ergibt sich bei ganzer, frischgemolten Milch. Ich ließ eine solche Milch, welche sogleich nach dem Melken 5 CC. ergab, 12 Stunden lang ruhig stehen und fand hierauf an der Oberfläche eine $\frac{1}{2}$ Zoll dicke Rahmschichte, wovon man nur ein Minimum von 0,5 CC. brauchte. Diese Rahmschichte wurde nun mit einem Löffel gründlich abgenommen, weil sonst unmöglich die Pipette aus

den unteren Theilen des Gefäßes hätte Milch holen können. Milch aus der Mitte hierauf entnommen ergab 28 CC. und Milch vom Boden gar 70 CC. Dieser letzte Versuch wurde mit nur 10 CC. Wasser angestellt. Da ich 7 CC. Milch zusetzen mußte, so ergab sich für 100 CC. Wasser: 70 CC. Milch.

Diese großen Differenzen lehren uns zur Genüge, wie wichtig es ist, die Milch vorher gehörig zu mischen, wenn die Untersuchung den Gehalt an Milchfugelchen im Allgemeinen richtig angeben soll.

Will man vollkommen sicher gehen, so mischt man zuerst die ganze zur Untersuchung bestimmte Milch gehörig durch, nimmt sich eine Probe davon, etwa eine Unze, in ein gewöhnliches Arzneiglas heraus und benützt diese nochmals geschüttelte kleinere Quantität zum Versuche. Ein höheres, 3 — 4 Unzen fassendes Arzneiglas hat außerdem noch den Vortheil, daß man, während man eine Lichtprobe vornimmt, die lange Pipette in denselben bequem stecken lassen kann.

II. Versuche mit Kuhmilch bei gradatim größerem Wasserzusatz.

A. Eine gewöhnliche ganze Milch aus einem Milchladen entnommen ergab 6,0 CC.

Ich machte nun von dieser Milch folgende Verdünnungen:

9 Theile Milch	=	10 Proc. Wasser
1 Theil Wasser		
8 Theile Milch	=	20 " "
2 Theile Wasser		
7 Theile Milch	=	30 " "
3 Theile Wasser		
6 Theile Milch	=	40 " "
4 Theile Wasser		

u. u. und fand nun folgende Zahlen:

ganze Milch	6,0 CC. Milch
10 Proc. Wasser	6,5 " "

20 Proc. Wasser	7,2 CC. Milch
30 " "	8,2 " "
40 " "	9,6 " "
50 " "	12,0 " "
60 " "	14,0 " "
70 " "	20,0 " "
80 " "	34,0 " "
90 " "	120,0 " "

B. Gewöhnlicher, käuflicher Rahm der einfachsten Sorte, sog. Kreuzerrahm, ergab 3,7. Es wurden nun mit diesem Rahm dieselben Verdünnungen vorgenommen wie im vorigen Fall A und folgende Zahlen gefunden:

Ganze Rahm	— 3,7
10 Proc. Wasser	— 4,1
20 " "	— 4,8
30 " "	— 5,5
40 " "	— 6,7
50 " "	— 7,8
60 " "	— 10,5
70 " "	— 14,0
80 " "	— 21,0
90 " "	— 60,0

C. Gewöhnliche ganze Milch aus einem Milchladen entnommen, ergab die verdächtig hohe Zahl 8,5. Es ist hier jedenfalls verdünnt worden, entweder direkt mit Wasser oder, was das gewöhnlichere sein soll, mit abgerahmter Milch. Mit dieser an und für sich an Milchkörperchen sehr armen Milch wurden nun wieder dieselben Verdünnungen vorgenommen.

Ganze Milch	— 8,5
10 Proc. Wasser	— 9,0
20 " "	— 9,8
30 " "	— 11,5
40 " "	— 14,5
50 " "	— 18,0

60 Proc. Wasser	— 23,0
70 " "	— 36,5
80 " "	— 60,0
90 " "	— 260,0

Diese letzte Zahl wurde durch Umbrehung des Versuches gefunden. Ich sah voraus, daß bei der großen Verdünnung eine gewaltige Masse Milch zu 100 CC. Wasser zugelegt werden müßte und nahm deshalb statt des Wassers gleich von dieser sehr verdünnten Milch 40 CC. in das Milchglas. Nach dem nur 15 CC. Wasser zugelegt worden, wurde der Lichtkegel sichtbar. Auf 100 CC. Wasser berechnet ergeben sich hieraus 260 CC. der so verdünnten Milch.

D. Ganze Milch aus einem Milchladen.

Ganze Milch	— 6,5
mit 10 Proc. Wasser	— 7,0
20 " "	— 8,2
30 " "	— 9,2
40 " "	— 10,5
50 " "	— 13,5

Wegen Mangel an Material wurden keine weiteren Verdünnungen mit dieser Milch mehr vorgenommen. — Wir haben in diesen vier mit sehr verschiedenen Milchsorten angestellten Versuchsreihen ganz analoge Resultate. In allen vier Fällen ist bei den schwächeren Verdünnungen, bis zu 40 Proc. Wasser, der Mehrverbrauch der Milch wohl deutlich fühlbar, jedoch mäßig. Am geringsten ist der Unterschied zwischen der ganzen und der mit 10 Proc. Wasser verdünnten Milch. Er nimmt schon zu zwischen 10 und 20 Proc. und wird immer merklicher bei jeder höheren Verdünnung. Bei 50 Proc. also halb Milch und halb Wasser stimmen in allen vier Versuchsreihen die Zahlen vortrefflich. Wir erhalten stets etwas mehr als eine Verdoppelung der ursprünglichen Zahl nämlich

ganze Milch	halb Milch	halb Wasser.
bei A = 6,0		12,0
bei B = 3,7		7,8

	ganze Milch	halb Milch halb Wasser
bei C =	8,5	18,0
bei D =	6,5	13,5

Bei den höheren Verdünnungen, 60 — 90 Proc. Wasser, steigen natürlich die Zahlen außerordentlich rasch, und ein Gemisch von 90 Theilen Wasser und 10 Theilen Milch ist schon so durchscheinend, daß man nur mehr eine kleine Quantität Wasser zuzusetzen braucht, um den Lichtkegel erkennen zu können.

III. Vergleichende Versuche mit Milch von 9 Kühen.

Herr Gejer, Assistent an der hiesigen Veterinär-Schule, hatte die Güte, 8 Tage lang folgende Versuche anzustellen:

5 Kühe untersucht vom 23. Juni bis 30. Juni 1862

I Schwäbisch-limburger alte Kuh.

II Montafuner Kuh, jung.

III " " mittleren Alters.

IV " " jung.

V " " mittleren Alters.

Alle Kühe wurden mit halb Wiesenheu, halb Wiesen-
gras gefüttert.

Untersuchungszeit.	Kuh				
	I.	II.	III.	IV.	V.
Auf 100 CC. Wasser.	CC. Milch	CC. Milch	CC. Milch	CC. Milch	CC. Milch
23. Juni Abends	3	3	3	4	5
24. " Früh	konnten keine Proben gemacht werden.				
" Abends	3	5	3	2	4
25. " Früh	2	4	2	3	5
" Abends	3	5	5	2	4
26. " Früh	4	6	5	3,4	4
" Abends	3	5	5	2	4
27. " Früh	2,8	5,8	2,5	2,5	3
" Abends	3	3	4	2	2,5
28. " Früh	3,5	5	3	3	3
" Abends	3	3	3	3	—
29. " Früh	2,5	6	4	2	3
" Abends	4	4	5	4	4
30. " Früh	4	5	4	5	5
" Abends	3	5	5	4	4,5

In diesen 69 Proben schwanken die Zahlen zwischen 2 und 6, welche letztere übrigens nur zweimal und zwar bei Kuh Nr. II. vorkommt. Als Mittelzahl aus diesen 69 Untersuchungen berechnet sich: 3,7. Addirt man die Zahlen der einzelnen Kühe gesondert, so ergibt sich für

Kuh I — 42,8

II — 64,8

III — 53,5

IV — 41,9

V — 54,7

Die Milch I und IV enthielt also am meisten Milch-
kugeln, in zweiter Reihe folgte III und V und auffallend
schlechter ist die Milch II. Voreilig wäre es jedoch, den Werth
der Kühe aus diesen Zahlen allein bemessen zu wollen. So
lange man nicht weiß, wie viel Milch überhaupt jede dieser
Kühe in 8 Tagen producirt hat, ist mit den procentischen
Rahmbestimmungen nicht viel zu machen. Es gibt Kühe,

welche bei gleicher Fütterung quantitativ noch einmal so viel Milch geben, als ihre Nachbarin. Nehmen wir nun an, daß Kuh II. nur um $\frac{1}{2}$ Milch mehr gab als Kuh IV., so hat sie in 8 Tagen doch ebenso viel Futter geliefert als diese letztere.

Ich selbst machte in neuester Zeit noch weitere 4 Bestimmungen mit Milch von 4 verschiedenen Kühen eines hiesigen Milchmanns, welche mit Malztrebern, Wehl und etwas Heu gefüttert wurden.

Die wohl umgerührte Milch unmittelbar nach dem Melken untersucht ergab folgende Resultate:

Kuh I.	— 3,4 GG.
" II.	— 4,4 "
" III.	— 5,0 "
" IV.	— 5,3 "

Auf diese wenigen Untersuchungen hin dürfen die Älten noch keineswegs geschlossen werden. Vergleichende Proben müssen in noch viel größerer Anzahl an Kühen verschiedenen Alters, verschiedener Race, Fütterung etc. angestellt werden, bis man endlich die äußersten physiologischen Grenzen wird bestimmen können. Hiezu aber mangelt mir Zeit und Gelegenheit.

IV. Einige Proben mit Frauenmilch.

Aus dem hiesigen Gebäuhause bekam ich durch die Güte des H. Prof. Schmidt 3 Proben von Frauenmilch. Da die Quantität aller 3 Proben nur eine sehr geringe war, so nahm ich gleich 5 GG. Wasser in das Probeglas und tröpfelte nun aus einer feinen Pipette die Milch zu. Die so gefundenen Zahlen wurden dann mit 20 multipliziert, um den Vergleich mit den früheren Kuhmilchversuchen zu erleichtern.

I. Milch 3 Tage nach der Entbindung	14 GG.
II. " 6 " " " " "	9 "
III. " 8 " " " " "	5 "

IV. Die Milch einer Frau, welche vor 3 Wochen entbunden hatte und deren Kind an der Brust durchaus nicht zunahm, ergab an der einen Brust die Zahl 32, an der anderen gar 42 GG.

V. Die Milch einer anderen Frau, welche vor 5 Wochen entbunden hatte, ergab auf der einen Brust 9, auf der andern, an welcher sich in einem früheren Wochenbett ein großer Abscess gebildet hatte, 5 Minuten später entleert, 25 GG. Das Kind dieser Frau entwickelte sich ebenfalls gar nicht.

Bei der Frauenmilch scheint viel darauf anzukommen, in welcher Quantität sie durch die Milkpumpe entleert wird. Nimmt man die Milch aus ein und derselben Brust in mehreren Abzügen ab, so ergeben alle diese einzelnen Proben verschiedene Zahlen. Diese wenigen Versuche zeigen jedenfalls, wie enorm verschieden der Fettgehalt der Frauenmilch ist. Es wird Sache der Geburtshelfer sein, diese Verhältnisse weiter zu verfolgen. —

V. Die optische Probe im Vergleich mit der chemischen Analyse.

In dem bisher gesagten wurde von dem wirklichen procentischen Fettgehalt der Milch ganz abgesehen. Wir begnügten uns damit, die einzelnen auf optischem Wege gefundenen Zahlen unter einander zu vergleichen. Von größter Wichtigkeit erscheint es indessen, durch diese einfache Probe den procentischen Fettgehalt selbst ermitteln zu können. Zu diesem Zwecke unternahm mein Bruder, Prof. August Vogel, auf meine Bitte folgende genaue Milchanalysen.

Zu den hier folgenden Fettbestimmungen der Milch kam mit einigen Modificationen das von Brunner *) zuerst vorgeschlagene Verfahren in Anwendung. In ein mit kleinen Bimssteinstücken locker gefülltes Liebig'sches Trockenrohr, von etwas weiteren Dimensionen als gewöhnlich, wurde die zu untersuchende Milch tropfenweise gebracht und durch Schütteln möglichst gleichmäßig über die Bimssteinstücke vertheilt. Das Abbrauchen und Trocknen geschah im Wasserbade mittelst des trocknen Luftstromes. Der vollkommen getrocknete und

*) Solley's Handbuch, S. 419.

gewogene Rückstand wurde hierauf so oft mit stets erneuten Mengen Aethers ausgezogen, bis daß ein Paar Tropfen Aether auf einem Uhrgläse verdampft keine bemerkbare Spur von Fett mehr zurückließen. Um diesen Punkt zu erreichen sind sehr bedeutende Mengen Aethers erforderlich. Die Entfernung des Aethers gelang am vollständigsten durch das Austrocknen des Rohres mittelst des trocknen Luftstromes im Wasserbade. Aus dem Gewichtsverluste ergab sich die Fettmenge

Milch A.

I. Trockenrohr + Bimsstein	24,669 Grammes
" " + Milch	27,772
d. i. Milch	3,103
Nach dem Abrauchen und vollständigen Trocknen	25,089
Nach der Behandlung mit Aether	24,906
	0,183
d. i. 5,894 Proc. Fett.	
II. Trockenrohr + Bimsstein	28,301
" " + Milch	33,536
d. i. Milch	5,235
Nach dem Abrauchen und vollständigen Trocknen	29,009
Nach der Behandlung mit Aether	28,690
	0,319
d. i. 6,092 Proc. Fett.	

Milch B.

I. Trockenrohr + Bimsstein	39,225
" " + Milch	42,574
d. i. Milch	3,349

Nach dem Abrauchen und vollständigen Trocknen	39,370 Grammes
Nach der Behandlung mit Aether	39,480

0,090

d. i. 2,689 Proc. Fett.

II. Trockenrohr + Bimsstein	32,305
" " + Milch	35,973

d. i. Milch

3,668

Nach dem Abrauchen und vollständigen Trocknen	32,681
Nach der Behandlung mit Aether	32,568

0,113

d. i. 3,080 Proc. Fett.

III. Trockenrohr + Bimsstein	26,391
" " + Milch	30,596

d. i. Milch

4,205

Nach dem Abrauchen und vollständigen Trocknen	26,821
Nach der Behandlung mit Aether	26,699

0,122

d. i. 2,901 Proc. Fett.

Es wurden also 2 Milchsorten A und B Gemisch untersucht.

Milch A ergab einmal 5,894 Proc. Fett
das zweite Mal 6,092 "

Mittelzahl 5,993 "

Milch B ergab das erste Mal 2,689 "

das zweite Mal 3,080 "

das dritte Mal 2,901 "

Mittelzahl 2,890 "

Milch A. ergab bei der optischen Probe

4 CC.

Milch B. „ 8,8 CC.

Aus diesen beiden Zahlen der optischen Probe und aus obigen beiden Mittelzahlen der chemischen Analyse hatte Herr Professor Seidel die Güte, mir eine Formel zu berechnen, mittels welcher man nun den Procentgehalt an Fett für jede beliebige optische Probe, also für alle möglichen Milchsorten und Milchverdünnungen leicht auffinden kann. Versticht man unter m die Anzahl der verbrauchten CC. Milch, so ergibt sich folgende Formel für die Fettprocente X

$$X = \frac{23,2}{m} + 0,23.$$

Haben wir z. B. von einer Milch 3 CC. bis zur Verendigung der Probe verbraucht, so berechnet sich ihr procenti-

$$\text{scher Fettgehalt } X = \frac{23,2}{3} + 0,23 = 7,96 \text{ Proc.}$$

Ich habe nun aus dieser Formel folgende Fettprocente für die optische Probe berechnet:

CC. Milch	entsprechen	Proc. Fett
1	"	23,43
1,5	"	15,46
2	"	11,88
2,5	"	9,51
3	"	7,96
3,5	"	6,86
4	"	6,03
4,5	"	5,38
5	"	4,87
5,5	"	4,45
6	"	4,09
6,5	"	3,80
7	"	3,54

CC. Milch entsprechen Proc. Fett.

7,5	"	3,32
8	"	3,13
8,5	"	2,96
9	"	2,80
9,5	"	2,77
10	"	2,55
11	"	2,43
12	"	2,16
13	"	2,01
14	"	1,88
15	"	1,78
16	"	1,68
17	"	1,60
18	"	1,52
19	"	1,45
20	"	1,39
22	"	1,28
24	"	1,19
26	"	1,12
28	"	1,06
30	"	1,00
35	"	0,89
40	"	0,81
45	"	0,74
50	"	0,69
55	"	0,64
60	"	0,61
70	"	0,56
80	"	0,52
90	"	0,48
100	"	0,46

So sind wir also im Stande, eine genaue Butterbestimmung in eben so viel Minuten zu machen, als die chemische Analyse dazu Stunden bedarf. —

Was nun schließlich die praktische Bedeutung dieser optischen Milchprobe betrifft, so wird sie von Nutzen sein, vor Allem der Sanitätspolizei zur Eruirung von Verdünnungen, dann den Ärzten überhaupt zur Prüfung der Frauenmilch und Controlirung der Kuhmilch bei künstlich aufzufütternden Kindern, ferner der Landwirthschaft zur Ermittlung des Werthes verschiedener Fütterungen und endlich einem jeden Privatmanne, dem darum zu thun ist, für sein gutes Geld auch eine gute, unverfälschte Milch zu bekommen.

THE
HEALTH OF THE ROYAL NAVY
CONSIDERED,
IN
A LETTER

ADDRESSED TO THE

RT. HON. SIR JOHN S. PAKINGTON, BART.,
G.C.B., M.P.,

BY

GAVIN MILROY, M.D., F.R.C.P.,

MEDICAL INSPECTOR UNDER THE GENERAL BOARD OF HEALTH FROM 1840 TO 1854;
MEDICAL COMMISSIONER TO JAMAICA IN 1851; AND MEMBER OF THE SANITARY
COMMISSION TO THE ARMY IN THE EAST IN 1855-56.

"I conceive it to be the duty of every educated person to closely watch and study the time in which he lives, and, as far as in him lies, to add his humble mite of individual exertion to further the accomplishment of what he believes Divine Providence to have ordained."—*Speech of the Prince Consort in the City, 1850.*

"It is only by clearly and distinctly pointing out the causes which affect health that we can hope to avert disease, and reduce the rate of mortality in the Naval service."—*Statistical Return of the Health of the Royal Navy for the year 1858.*

LONDON:
ROBERT HARDWICKE, 192 PICCADILLY.
1862.

Price, Two Shillings.

TO THE RIGHT HONOURABLE
SIR JOHN S. PAKINGTON, BART., M.P.

SIR,

The active interest you evinced when First Lord of the Admiralty in all that concerns the thorough efficiency of the Navy—in regard, not only of the ships, their armaments and supplies, but also, and not less, of the hands which are to wield them—leads me to address the following observations to you, in the hope that they may thus possibly attract the notice of others besides the members of my own profession. Far too important is the subject to need any apology for seeking to bring it under the attention of all officers, the administrative and executive as well as the medical officers, of the service, and indeed of the public generally. It is now admitted, on all hands, that the strength and virtual efficiency of an armed force, whether afloat or on shore, is to be measured, not by the mere number of the names on the ship or regimental roll, however complete may be all the material equipments of the force, but really and truly by the actual number of hearty vigorous men who are, from day to day and from month to month, continuously available for fatigue duties of all sorts. Every man put on the doctor's list is so much power withdrawn from the full effectiveness of the living machine. Nay, it is more than this, for each such

B

loss forthwith becomes the occasion of extra duty being cast upon the well men to supply the void; and then, too, there is the time and labour of those who have to act as the attendants upon the sick to be taken into account. These consequences become a serious matter when sickness prevails to any considerable extent among a ship's crew. The energies of the well men are overtaxed, their hours of meal-time and sleep are interfered with, continued extra fatigue creates weariness and discontent; and this is the very state of system in which the health is liable to suffer from influences which it has hitherto resisted. Sickness thus gives rise to sickness in more ways than one; and this, too, is apt to go on in a progressively increasing ratio. Obviously, therefore, the necessity of averting or preventing to the utmost all disease, and of preserving as far as can be done uninterrupted health among a ship's crew, cannot be overestimated as one of the main objects to be aimed at by all who have at heart the duty of maintaining a powerful Navy, ready at all times for the defence and honour of our country. And the importance of the subject is yearly becoming greater, from the acknowledged difficulty that has often been experienced in recent times of promptly manning a large fleet upon an emergency, in consequence of the enormous demands of our mercantile marine for able-bodied hands in these days of increasing commercial activity.

My attention having been drawn, at various periods of my professional life, to the state of health of seamen, and specially on two occasions of official employment in the West Indies, and in the Black

Sea and Mediterranean,* along with numerous opportunities of converse with naval officers both at home and abroad, will plead my excuse for taking upon me, though not belonging to the service, the task of inviting public notice to the subject; and the extended inquiries which I have recently conducted into the general operation of quarantine have also brought to my knowledge much valuable information on the hygienic condition of ships, both of the royal and of the mercantile marine of different countries.† The very unconnectedness of my position may possibly not be without some advantage; but be that as it may, the truth and fairness of the following statements must be the best test of their value or otherwise.

Before proceeding to treat of the existing state of health of the Royal Navy, which is the special object of this letter, it will not be amiss to glance at its condition in the early part of the present century.

After the close of the great continental war, the distinguished veteran Sir Gilbert Blane, long one of the most eminent medical officers in the service, published his dissertation "On the Comparative Health of the British Navy, from the year 1779 to the year

* Report made by Dr. Milroy to the Colonial Office, on the Cholera Epidemic in Jamaica in 1850-51, &c. Printed by order of the House of Commons, May 1854.

Report to the Minister of War of the Proceedings of the Sanitary Commission, dispatched to the seat of War in the East, 1855-56. Presented to Parliament, March 1857.

† Papers relating to the Laws and Practice of Quarantine in Foreign Ports and in British Colonies, prepared by a Committee of the National Association for the Promotion of Social Science, and communicated to the Board of Trade. Printed by order of the House of Commons, Aug. 1860, and August 1861.

1814, with proposals for its farther improvement." He there pointed out the immense saving of health and life in ships of war that had been effected during his time, in consequence of successive ameliorations in the construction and sanitary arrangements of the vessels, and in the general condition and care of the men. For the last three years of the war, the annual death-rate from all causes throughout the different fleets he reckoned to average about 33 per 1000 of the entire strength, the aggregate of which for each year being put down at nearly 138,000 of all ranks and ratings—an enormous force certainly, in the gathering together of which but little attention could possibly have been paid in the selection of lives, more especially at a time when impressment was in vogue. Thirty years previously, the ratio of mortality had been far more than double, probably two-thirds, this proportion. Nor will this be wondered at when it is known that at that time ships were often like the then jails and prisons, with their holds so foul and noisome that "the air used to become so contaminated as in innumerable instances to produce instantaneous and irremediable suffocation";—and when the newly raised men were so huddled together on board filthy receiving vessels that fever was never absent from them, and numbers were cut off before they were even distributed among the ships of war, while hundreds perished of the same disease and of putrid flux before they had been many months at sea. Sailors died off in those days at a greater rate than even the troops, miserably bad as the condition of the latter was. Scurvy was then, from the bad provisions served out to the men, a

desolating scourge to the Navy; and such continued to be the case down to 1796-7, when lemon-juice was first regularly supplied to our ships of war, and the diet of the crews was improved. Yet twenty years before that time, Captain Cook had convincingly shown not only how this disease may be prevented, but also how, by the simple expedient of furnishing wholesome and suitable provisions and pure water, and by unremitting attention to the thorough cleansing and ventilation of a ship, the health of her crew may be so preserved that, in a three years voyage round the world, out of a company of one hundred and twelve men, but five in all should be lost—four from accidents, and one only from disease. Sanitary practice then, as now, advanced but slowly long after its benefits were demonstrated.

As scurvy was got rid of by improving the diet of the men, so fevers and fluxes were, if not got rid of, immensely kept down when ships were kept clean and better aired in every part, and when the crews were better treated and cared for in respect of their accommodation, clothing, and medical supplies.

By the great reduction in the death-rate of the Navy at the close of the continental war, compared with what it had been at its commencement, it was estimated that "two ships of war are now (1815) capable of more service than three of the same rate in former times"; in other words, one-third had thus been added to the effective force, and moreover an enormous expense, inevitably consequent on sickness and death, had been saved. Still the ratio of mortality in the Navy was justly considered by Sir Gilbert Blane as excessive; from disease alone, and

exclusive of all losses from accidents, drowning, and the casualties of war, it was in his opinion nearly double that among persons of the same ages in civil life. In his day, and for a good many years afterwards, there did not exist the means of determining with sufficient accuracy the vital statistics either of our general population or of the Navy. It was not till 1837 that the Registrar-General's department was instituted; and about the same time the Government first recognised the necessity of digesting and tabulating the returns of sickness and mortality in the two public services. The first issue of the statistical reports of the health of the Navy was in 1840; they embraced a period of seven years, commencing with the year 1830. Subsequently, but at long intervals, other reports for the septenniad from 1837 to 1843 were published. These bulky and unreadable Blue Books have of recent years been superseded by annual reports, which are infinitely more useful and instructive. The change was introduced after the late Russian war, and great praise is certainly due to the medical department of the service, and especially to Dr. Bryson, for the ability with which they are prepared. "The Medical Statistical Returns of the Baltic and Black Sea Fleets during 1854 and 1855" were issued in the early part of 1857; and these were followed by the "Statistical Reports of the Health of the Navy" for the year 1856, in the summer of 1858; for the year 1857 in 1859; and for the year 1858 in the spring of 1861.* It is from these more recent documents that the facts and data in the following

* The report for 1859 will, it is understood, shortly appear.

pages are mostly derived, and on which any opinions that may be expressed will be chiefly based.

The first point to be ascertained is, of course, what is the ordinary

Rate of Sickness in the Navy.

It may be necessary to premise that 1856 was nearly a year of peace; for, although the Russian war was not completely over till the early part of the spring, our fleet was not engaged in any hostile operations, at any time, either in the Black Sea or the Baltic; and it was not till the very close of the year that hostilities broke out in the Canton river. In 1857, our differences with China required a considerable increase to be made in the naval force on that station; most of the ships were long anchored in the Canton river, and their crews were much exposed to the malarious influence of its shores, besides having frequent sharp encounters with the enemy. The fleet of mandarin war-junks was destroyed on the 1st of June. On the 28th of December, the city of Canton was attacked; and next day, the walls were scaled, and the city and adjacent heights were taken. This year, too, the great Sepoy revolt took place, and the Naval Brigade, formed by detachments from the *Shannon* and the *Pearl*, joined the military force before Lucknow.

In 1858, the East India and China squadron was still further increased, and was actively engaged throughout the whole year both against the Chinese, and against the Indian mutineers. Three battalions of marines were for several months quartered in and around Canton, and had most severe and harassing

duties to perform in that unhealthy climate. The expedition against Tien-tsin on the Peiho took place in May; the defences in the river were taken by assault on the 20th, and the town was captured on the 26th of that month. The active aid given by the Naval Brigade in India is well known.

In 1856, when the aggregate mean force of the Navy was 51,730, the average number of men daily on the sick-list throughout the service was 3,132, or in the proportion of sixty-two men in every thousand; in other words, a seventeenth part of the whole force was continually off duty from the effects of sickness or injury, either on board ship or in hospitals on shore.

In 1857, the daily sick-rate was very nearly the same, being only a fraction less. The aggregate force that year was 42,470.

In 1858, the rate was higher, being in the proportion of sixty-six sick men in every thousand. The number at all times ineffective, out of a total force of 43,120, averaged 2,859, or about one in every fifteen men.

The sick-rate varies much, of course, in the different fleets into which our Navy is distributed, according to the stations where they are employed, viz.:—the Home, the Mediterranean, the North American and West Indian, the Brazilian, the Pacific, the West African, the Cape of Good Hope, the Australian, and the East Indian and China. To these nine fleets must be added the Irregular force, engaged on various stations, as the exigencies of the service require.

The amount of sickness is usually lower in the

Home, the Mediterranean, the Pacific, and the Australian stations than in the other fleets; but the rates vary so much in nearly all of them, from year to year, that no very positive or uniform statement can be made on this head. One year the North American and West Indian squadron is very healthy, the next year it is extremely sickly. The same thing may be said of the Brazilian squadron of recent years; formerly, it was invariably one of the most healthy. Two only of the fleets uniformly exhibit a high rate of sickliness, namely, that on the West Coast of Africa, and still more that on the East India and China station, which of late years has always stood highest on the list. The Irregular force also generally exhibits an unfavourable return.

The following table shows the mean strength and the average sickness-rate of the different fleets for the three years 1856, '57, and '58, taken together, and will suffice to give a pretty fair idea of the relative importance both as to numbers and the comparative healthiness of our naval forces employed in different parts of the world, however much the exact numbers, etc., vary from one year to another.

FLEETS.	Mean strength for the three years.	Average daily Sick-rate for the three years.
East Indian and China	7263	93 per 1000 men.
Irregular	7117	69 " "
West Coast of Africa	1707	68 " "
Cape of Good Hope	1030	69 " "
North America and West Indies ..	4365	58 " "
East Coast of South America.....	1323	55 " "
Pacific	2387	53 " "
Mediterranean	7867	52 " "
Home	11,814	50 " "
Australian	437	46 " "

The number of days spent in the course of the twelvemonths on the sick-list, either on board ship or in hospital on shore, affords another means of showing approximately the loss of effective service on each of the various stations, and also over the entire aggregate force. Without going into details, it will be sufficient to state that in 1856 the total number of days' sickness throughout the Navy was such as to give twenty-two days off duty to each man employed; that in 1857, the proportion was a trifle more; and that in 1858 it was still higher, being rather more than twenty-four days on the sick-list to each man.

From the sick-rate, or amount of constant temporary loss of effective strength arising from disease or injury, I now pass on to note the amount of the permanent losses caused by death and by invaliding.

The Rate of Mortality in the Navy.

This varies, as might be expected, very considerably in different years, not only on separate stations, but throughout the service generally; and this, too, quite independently of the casualties of war and of all other accidents, including drowning, to which seamen are specially exposed.

In 1856 the general death-rate from all causes was 15·5 in every thousand men. In 1857 the proportion was 22 per thousand; and in 1858 it was 25·8, or nearly 26, per thousand. As these rates include the deaths from violence and drowning, as well as those from sickness, it will be better for the present to leave the former out of consideration, and attend only to the amount of mortality from the effects of disease, as a better standard will thus be

afforded for comparing the health of the Navy with that of other bodies of men about the same period of life.

In 1856, the deaths *from disease alone*, throughout the service, were in the proportion of 12·1 in every thousand of the force; in 1857, it was in the proportion of 14·7; and in 1858, it was in the proportion of 20·2. The general average for the three years was therefore between 15 and 16 per thousand men. From the following table, which gives the mean total and the disease death-rate for the three years, the reader will be able to judge of the relative fatality of the different stations on which our ships of war are engaged.

STATIONS.	Mean Death-rate for the three years.	
	From Disease alone.	From all causes.
Home	7·8	10·3
Mediterranean	8·4	11·6
North America and West Indies	19·8	24·
Brazilian	21·	25·2
Pacific	6·9	8·7
West African	14·3	20·5
Cape of Good Hope	10·2	16·3
East Indian and China	37·7	47·6
Australia	3·6	9·9
Irregular	8·3	12·4

The extremely high death-rate of the East Indian fleet will be noticed. It has been seen that two of the three years were years of war, both in China and in the East Indies. In 1856, the death-rate was only 27 per thousand from disease, and 34 from all causes. In 1858, it was 51·9 from disease, and 62·5 from all causes.

The Rate of Invaliding in the Navy.

The other cause of permanent loss to the service, from year to year, is the discharge of all the men who are hopelessly unfit for future duty in consequence of grave organic disease, or of some incurable infirmity.

In civil life, the annual death-rate is our only available test in estimating the healthiness, or otherwise, of the community, or of any large portion of the community. No means, as yet, exist for ascertaining, as in our army and navy, the Statistics of Sickness among the population of our towns and rural districts. And as to any elimination of invalids among civilians, the only discharge of them is by death. This circumstance alone presents a marked difference in the elements of any comparison instituted between the ratio of mortality in naval and military life on the one hand, and among working-men of the same ages in civil life, on the other; greatly, of course, to the advantage of the former by so many incurable sailors and soldiers having been discharged, and whose deaths on shore go to swell the mortuary returns of the latter.* What addition should be

* Keeping this observation in view, the reader may compare, in the following table, the death-rates in different bodies of men at sailors' ages among our civil population, with that, from all causes, in our home fleet, as given above:—

London Fire Brigade	-	-	-	-	7	per 1000 of strength.
Metropolitan Police	-	-	-	-	7.6	" "
England (healthy districts)	-	-	-	-	7.7	" "
Agricultural Labourers	-	-	-	-	8	" "
Out-door Trades in towns	-	-	-	-	8.5	" "
City Police	-	-	-	-	8.9	" "
England	-	-	-	-	9.2	" "

The signal reduction in the death-rate among our troops at home,

made to the death-rate from disease in the Navy to bring it more upon a par with the death-rates in civil life, for the purpose of comparison, it is not easy to determine. It cannot be less, I should think, than six or seven per thousand of the strength at the least.

In 1856, the number of men invalided was 998, or in the proportion of rather more than nineteen in every thousand of strength. In 1857, the number invalided was 1460, or rather more than thirty-four per thousand; and in 1858, the number was 1763, or nearly fifty per thousand. The mean rate for the three years together, in the entire Navy, was therefore between thirty-one and thirty-two in every thousand men. To show how much this varied in different fleets, it may be stated that on the Home Station the rate was twenty-three, while on the East India Station it was fifty-two, per thousand.

By putting together the number of deaths, and the number of men discharged as invalids, in the course of the twelvemonths, the yearly total permanent loss to the service is ascertained. In 1856, this amounted to 1799 men, out of an aggregate strength of 51,730. In 1857, it amounted to 2404, out of an aggregate of

within the last three years, has been such as to bring it nearly to that in the healthy districts of England. Formerly, it was more than double. That this marked decrease may be in part the effect of greater care in the selection of the men, and of a larger proportion of the force consisting of men recently enlisted, is not unlikely, as the same result has been observed in the case of other bodies of men. Thus with the Fire Brigade, the deaths, for the first thirteen years of its establishment (as Dr. Guy has told us in his valuable paper on the *Mortality of the British Army*) were at the rate of 9.6 per thousand; while for the last twelve years they have fallen to 7—a result due, he thinks, mainly to the more careful selection of the men.

42,470; and in 1858, out of an aggregate of 43,120, it amounted to 2878, or a loss equal to the combined crews of three of the largest line-of-battle ships in the Navy.

To put the facts in another way; the proportion of total permanent loss to strength in 1856 was, throughout the Navy, 34·8 per thousand men; in 1857, it was 53·8 per thousand; and in 1858, it rose to 66·7, or a full fifteenth part of the whole strength. If we take one or two fleets by themselves, we find that, on the Home Station, the mean rate for the three years of the total permanent loss was thirty-three per thousand; or, deducting all the losses attributable to violence and accident, that it was at least 28 or 29 per thousand of the strength. In the East India fleet, the mean total loss for the triennial was at the rate of 95·9 per thousand, or little short of ten per cent. of the force per annum.

Diseases, &c., most prevalent in Ships of War.

Having now determined the general facts as to the absolute and relative amount of Sickness, Mortality, and Invaliding among the seamen of our Navy, the next point is to find out the principal causes which contribute to the production of the temporary, as well as of the permanent, losses sustained by the service from one year to another. Avoiding all minute details, as suited for the professional rather than for the general reader, it may be stated that about one-half of all the entries on the sick-list are due to ailments of a slight, or at least non-serious, nature, and consist mainly of venereal complaints; or of boils, ulcers, and skin diseases; or of trivial wounds and

contusions. This moiety of the sickness occasions but a fraction of the losses by death and invaliding (considerably more by the latter than by the former), and may, therefore, be passed over with the single remark that the extensive prevalence, in some fleets, of the first named group, is a subject which seems to call for some attention on the part of the authorities in sea ports, not only in consequence of the serious amount of temporary loss of service thus occasioned, but also from the consideration that the constitution of many men is thereby gravely damaged, and rendered doubly or trebly susceptible of some other diseases, which cause much of the permanent losses from one year to another.*

The diseases which occasion by far the greatest amount of mortality in the service are (1) fevers, (2) diseases of the bowels, and (3) diseases of the lungs, etc. It is, therefore, these three groups of sickness which demand our special attention, more particularly in regard to the causes which favour their production and enhance their fatality. But before going into particulars, a preliminary remark or two requires to be made.

Some Ships more Sickly than others.

The first point, which must strike every inquirer,

* "That a disease so destructive of health and happiness, which, by an acquired constitutional taint, may be transmitted to generations yet unborn, should be allowed to go on increasing in our large sea-ports to an extent unknown in any other part of the world, is greatly to be deplored; but so long as the municipal authorities of those towns, where it is most rife, refuse to cooperate with the Government in establishing hospitals for the cure of the degraded creatures that swarm along their pavements, it will be in vain to hope for any abatement of the evil."—*Report on the Health of the Navy, for 1856.*

is the marked difference between different ships of the same squadron in respect of healthiness, or otherwise. While some ships are very sickly, others similarly employed, and apparently exposed to like external or atmospheric influences, are at the same time comparatively healthy. When such an occurrence is observed in a dwelling or cluster of dwellings on shore, the idea at once suggests itself that the difference must be due to some local cause—either to the good or bad sanitary condition and arrangement of the houses, or to a difference in the constitution and hygienic peculiarities of the occupants, arising from diversity of age, diet, occupation, etc.

It is not always easy at once to discover the chief cause or causes of the insalubrity, and mistakes are often committed by men who, under the influence of a favourite doctrine, are apt to confine their attention to one set only of agencies and overlook the effect of others. A house may, to the outward eye, be a picture of neatness and beauty, it may be as trim and clean as hands can make it, it may have been well constructed, with drains and sewers which are carefully looked after to prevent all obstruction, its sitting rooms may be well aired and as fresh and sweet throughout the day as can be desired;—and yet in such a dwelling some of the inmates, whose health previously had been good, are constantly ailing; with head-ache, loss of appetite, and general malaise and debility, if not with bowel and febrile complaints. And all this may be owing to some overlooked and unknown old cess-pit under or close to the basement; or to the foundation being damp and unaired; or to a stagnant pond or manure heap close by; or it may

be that the doors, windows, and register stoves of the principal bedrooms are so carefully fitted, that not a breath of air can pass in or out of the rooms at night; or that too many of the domestics may be sleeping in one chamber, and this may be on an ill-ventilated basement floor, or over a stable or outhouse where dust and other refuse are allowed to lodge. Or failing any of these evils, the water supply may have acquired some taint or noxious impregnation, which slowly but surely affects the health. However obscure at first may appear to be the cause of the mischief, we may be assured that in sanitary problems, just as in the problems of other physical sciences, a solution of most phenomena may be confidently looked for from exact and patient inquiry. What has been said about a house equally applies to a ship, which is only a floating detached dwelling, often moored in an unhealthy site, and in frequent communication with other separate dwellings of the same sort, and with the villages and towns on shore.

The greater sickness of some ships over others may be for one season or year only, or it may continue for two or three years in succession. Occasionally a ship has been so uniformly unhealthy, whenever it has been put in commission, that she acquires a thoroughly bad name in the service.

I will give two or three examples of notably unhealthy ships.

The *Hannibal* was in 1855, and again in 1856, very sickly. When in the Black Sea in the former year, besides suffering from bowel complaints more than the rest of the fleet, no fewer than 207 cases of typhoid fever occurred among her crew of 830,—“while

other ships of the same size and similarly engaged had not a fourth or a sixth part of the number." The fever continued to infest the ship in the following year, when stationed in the Mediterranean; it appeared to be but little influenced by the locality where she was, or by the duties in which the men were engaged. As suggested in the Returns, this sickness must have depended "upon a cause within."

The *Conqueror* and the *Centurion* were commissioned about the same time at Devonport, and served in the Mediterranean during 1857 and 1858. The former had a crew of 900, and the latter one of 740; both seem to have been similarly engaged during the whole period. Yet, in both years, there were more than ten times as many cases of low fever (besides a great excess of bowel complaints, erysipelas, and ulcers, as well as of chest and throat complaints) in the *Conqueror* as in the *Centurion*. "It is obvious," Dr. Bryson remarks, "that there existed some radical defect in the sanitary condition of the former, or that the physical condition of the one ship's company was far inferior to the other."

The *Dauntless* frigate while on the West India station in 1852-3 lost nearly seventy of her crew, within a few weeks, from yellow fever, which went on attacking the men as long as they remained on board, and only ceased when the whole ship's company were landed at Barbadoes. Throughout the Russian war, she served first in the Baltic and then in the Black Sea. Her sick-rate was high throughout the whole time; and the number of cases of fever, in each year, was very much in excess of what occurred in other vessels of her class.

It is quite true that when a ship has got a bad name on the score of health, more may sometimes be made of any future sickness on board of her than of other vessels to which no black mark may be attached; nor can we wonder at this, as it is scarcely possible for any one sailing in her to divest his mind of her health antecedents, and it is well known that the mere suspicion or dread of a sickness will often go a great way in inducing it, and certainly in aggravating its nature—a strong proof, by the way, of the value of a good repute, and of the necessity of preventing to the utmost any tendency to mental depression or alarm among a ship's company. Nowhere is panic from a pestilence so dreadful as on board ship at sea. But that there was some hurtful physical cause present in the *Dauntless* is, at all events, highly probable, when the history of the *Rosamond*, formerly the *Eclair*, is considered. Besides the disastrous loss of so many of her crew from yellow fever, in 1845, and the reappearance of a similar disease on board a year or so afterwards, when commissioned under a new name, she again proved an extremely sickly ship in 1852 on the West India station, and again in 1854 when employed in the Baltic, being there infested more than any other ship of the fleet with low fever and other zymotic maladies, and her daily sick-rate averaging the enormous proportion of 8 per cent. of her crew, throughout the whole period. The ventilation in her between-decks is stated to have been most imperfect, besides other causes of insalubrity to be afterwards noted.

Causes of Insalubrity in Certain Ships.

It is, obviously, of primary importance to determine the probable causes of the marked unhealthiness of some vessels in a fleet over others, seeing that it must be mainly on a solution of this very point that the question as to the improvability of the health of our Navy turns. In some instances the causes appear sufficiently palpable; in others they can only be surmised; while, not unfrequently, they seem to have hitherto eluded research. In the Navy as in the Army, and in civil life too, the subject of the causation of diseases, and, consequently, the only secure ground for their prevention, has not uniformly been appreciated as its great importance demands. Every year, doubtless, the matter will be more and more attended to, not only by medical men without exception, but also by all executive officers in command of our ships and troops, as well as by municipal and other authorities on shore.

One simple truth may be confidently stated, viz.—that the liability to disease is in general greatly dependent upon, and often is commensurate with, the purity of the air which is breathed throughout the four-and-twenty hours. There is no exception to this law, in any climate or under any condition, at sea or on land. Now there is a two-fold source of impurity in the respired air to be always guarded against. The one source is extrinsic or arising from something external to us, it may be from a marsh, or a foul gutter or drain, or any collection of decomposing matter, which gives off a noisome moist effluvium. The second source is intrinsic, and is unceasingly being supplied by emanations from our own

bodies, independently of any external cause of malaria or miasm. The second of these evils is much the more dangerous of the two; the poison so exhaled is more injurious to health, and its presence is infinitely more common, following us wherever we go, and only avoidable by unceasing vigilance in maintaining the renewal of fresh air around us.*

That both these sources of atmospheric impurity are at times apt to affect mischievously the crews of our ships of war, appears from the following brief instances out of others, cited in the recent official reports. And first as to some of the effects of overcrowding and defective ventilation.

Fever.

Forty cases of fever occurred in the *Eurotas* frigate, while in the Mediterranean, between April and June 1856. Her medical officer "was unable to account for the disease, unless it arose from the extreme lowness and closeness of the deck on which the men were berthed."

In the *Valorous*, an outbreak of fever took place in May 1858, on the passage from Ferrol to Plymouth, where fresh cases continued to occur for some

* "Air contaminated by foul and stagnant exhalations, particularly those from the living body, is the ascertained cause of typhus fever, which has been a more grievous and general source of sickness and mortality in the navy than even the scurvy. * * * The infection of fever is generated by the breath and perspiration of men, crowded for a length of time in confined air, and without the means of personal cleanliness."—SIR G. BLAKE.

The case of the Egyptian frigate, which arrived last year at Liverpool with her crew in such a wretched condition, and occasioned so much alarm at the time in that city, was one out of many instances of the truth of these remarks.

weeks after her arrival. Three proved fatal. "With the exception of a somewhat imperfect ventilation, in consequence of the main deck ports having been caulked in, previous to her departure for Ferrol, no cause (of the sickness) could be discovered" by a board of executive and medical officers appointed to examine the ship.

Thirty-four cases, six fatal, occurred among the crew, 825 in number, of the *Princess Royal*, while conveying a portion of the 11th regiment from Malta to Alexandria, between the 4th and the 17th of January 1858. The first case was on the sixth day after leaving Malta, and the rest within the next fortnight. As to the cause of the outbreak it is stated that, in consequence of the boisterous weather, "it was necessary to keep the ports, both on the main and lower deck, barred in during nearly the whole of the passage." The spread of the disease seems to have been checked by removing the men from the lower to the main deck. Other ships of the line carried a larger number of troops without detriment. Thus the *Majestic* conveyed 800 troops from the Crimea to England in the summer of 1856, and not a single case of fever occurred among her crew. But the previous history of the *Princess Royal* shews that, though a new ship, she was very sickly in 1855 while in the Black Sea. She then lost more men from fever than any other ship in the fleet, or than even the Naval Brigade serving before Sebastopol, notwithstanding all their exposure to wet and cold and their incessant toil in the trenches. In 1857 also, a bad form of fever broke out on board of her at Corfu; 3 out of 17 cases proved fatal. As the *Royal*

Albert, which was lying there at the same time, remained free from any febrile attack whatever, the disease must have been owing rather to some cause connected with the ship herself, than with the locality. Her crew had, moreover, in the early part of the same year, while at Lisbon, suffered much from ulcers, and phlegmonous disease.

It will be afterwards shewn how much the malignant fever of the West Indies, etc., is aggravated by defective ventilation on board ship.

Cholera, &c.

Three or four days after this disease appeared in the *Megara*, at Calcutta in 1858, she put to sea. The next day the number of attacks had increased. Subsequently, the boisterous state of the weather requiring that the main deck ports should be kept closed, all the sick, etc., were put on the upper deck under an awning. From that time, the disease subsided and soon ceased entirely. Within a fortnight, nearly an eighth part of the crew had perished.

About the same time, the *Pylades* was attacked on her way from Calcutta to Trincomalee; "the sick were discharged into the receiving ship (on her arrival there), where, under the influence of better ventilation and a more suitable diet, the disease soon ceased entirely."

The most memorable instance on record of the effects of defective ventilation on the fatal progress of cholera occurred in 1854, in the *Britannia* while in the Black Sea, and just before the sailing of the expedition to the Crimea. Within five days or so, no fewer than 229 out of a crew of 920 were at-

tacked (besides nearly 400 with diarrhoea), and of these 139 died. The ship had put to sea in the hope of getting rid of the disease, which had begun to appear on board of her and some other ships of the fleet at Varna. For a day or two the change appeared to have done good; but the weather became boisterous and all the lower deck ports had to be closed. Thereupon, the pestilence broke out in its full fury in the course of the following night. On returning to Baljik, the whole crew were removed into some empty transports; and there was an end of the disease. The total loss of life from this short outbreak was considerably more than double all the fatal casualties, on board the whole fleet, from the fire of the enemy in the attack against the sea batteries of Sebastopol. None of the officers of the *Britannia* died from cholera. Such a fact is always significant.

The tendency to diarrhoea and dysentery is always observed to be much increased, and their obstinacy and severity to be greatly aggravated by defective ventilation on board.* These are generally the most common and fatal diseases on board crowded troop and emigrant vessels; ships of war also have often suffered most severely from their prevalence. The same thing may be said of another occasionally most troublesome and disabling set of maladies in long voyages, viz., ulcers and erysipelas. Several of the large ships of our Baltic fleet in 1855 suffered seriously from this cause. The surgeon of the *Colossus* considered that "the crowded state of the decks and

* "The dysentery, which stands next in order (to fever) in point of fatality, is also generated and propagated by the want of cleanliness and ventilation."—SIR G. BLANE.

the want of personal cleanliness among the newly-raised men, combined with imperfect ventilation on the orlop deck (where most of the attacks occurred) were the principal causes of the disease."

The *Calcutta*, the admiral's ship, on her voyage out from England to Hong Kong in 1856, had an unusual number of cases of ulceration on board. "I have no hesitation in saying," writes the surgeon, "that the sick berth is manifestly the head quarters of the disease, where large numbers of suppurating sores have been congregated together in a confined and overcrowded place." When a portion of the crew was landed, and better ventilation of the decks established, together with an ampler supply of fresh provisions, the tendency to sloughing ulceration ceased.

The preceding instances of the evil effects of Overcrowding on board ships of war entirely accord with the experience of the army and of civil life, in close unwholesome barracks, workhouses, etc.

Effects of Impure Holds.

The other cause of atmospheric vitiation occasionally on board ship is the admixture of malaria from impurities in the hold, etc., of the vessel, or from her own timbers in a state of decay. The following recent instances from the Navy Returns illustrate this point.

The great sickness of the *Rosamond* in 1854, previously mentioned, was ascribed by her surgeon to the state of her hold. "Several cases" (of typhoid fever) "occurred in men employed in cleaning the bilges; there was a considerable accumulation of filth

under the magazine. Whenever the hatches were kept closed for a time, a foul smell was perceived on opening them." A like state of things was reported of this vessel eight years before.

In 1856, the *Herald*, on the Australian station, suffered much from alvine flux, while cruising among the Feejee Islands. "The greater number of the cases occurred amongst the men berthed on the port side of the lower deck. The gunner's store-room was therefore cleared out, and some very offensive matters were discovered, which produced nausea and faintness in one or two of the men employed on that duty." With the thorough cleansing of the hold, the disease seems to have subsided until the following year.

The *Hecate*, one of the West African squadron in 1858, had sixty-three cases of fever (which in several instances had the character of the less aggravated form of yellow fever) among her crew of a hundred and thirty-two, during the more healthy part of the year between February and May, and chiefly while she was at sea, cruising between the coast and the island of Ascension. Her surgeon was of opinion that the sickness was mainly due to fœtid exhalations, so offensive as to cause nausea and headache, from the hold, which was found to be exceedingly foul from the accumulation of a quantity of rubbish and black mud. "But the most offensive effluvium seemed to come from the wood lining the inside of the hull, which was in a state of decay, and in some places covered with fungi, so that it crumbled away before the common iron scraper."

Another of the African squadron that year, the

Hydra, was similarly affected. Forty-two in a crew of a hundred and twenty-five were attacked, while at sea. Large quantities of black stinking mud were taken out of her hold.

In the course of the same year the *Boscawen*, the admiral's ship on the Cape of Good Hope station, had thirty-six of her crew smitten with fever; "the cause of which appears to have been connected with the state of the hold." Five of the cases were fatal. Besides fever and bowel complaints, boils and ulcers were rife among the crew; out of six hundred and eighty men, eleven died and twelve were invalidated from disease alone in the twelve months.

To what extent the two causes of insalubrity above-mentioned operate in the production of sickness in our ships of war in the present day, it is not possible to say with any degree of certainty. That, as compared with what existed in the Navy sixty or seventy years ago, they are now feeble and infrequent is known to all who are acquainted with the medical history of the service. Even since the close of the great continental war in 1815, a notable reduction in the losses from disease among our seamen has taken place in consequence of various improvements, sanitary and hygienic, on board ship; and still more recently, the death-rate in some stations appears to have been lower than it used to be in previous years.

These general facts suffice to show how much the effective strength of our Navy is dependent on the due observance of the established laws of health, and should serve to encourage us in pursuing a like philanthropic course of carrying into effect, to the utmost, every amelioration which the progress of science may indicate.

Diseases most Disabling and Fatal in the Navy.

I will now briefly analyse the recent mortuary returns of the Navy for the purpose of determining the relative mortality produced by different classes of disease, so that the reader may understand what are the principal causes of the permanent losses sustained by the service from year to year. Out of the 2,125 deaths from disease alone during the three years 1856, '57, and '58 (the total loss from all causes amounted to 2,735), 559 were due to fevers, chiefly of the continued type; 715 were due to diseases of the bowels, chiefly alvine flux and Asiatic cholera; 440 were due to diseases of the respiratory organs, especially to consumption. On each of these three classes I shall make a few remarks; and first of

Fevers.

This class of diseases varies much in frequency and fatality in different years upon all the stations, so that the experience of any one year alone might give a very illusory idea either of their local or of their general prevalence.

Fevers occasion but little of the sickness or mortality in the Home fleet. The annual number of deaths from this cause does not average more than eight or nine. Very many of the attacks are the result of intemperance and exposure to wet and cold, when the men get leave to go on shore. As might be expected, a large proportion of these cases occur in some of the ships at Portsmouth and Devonport. Occasionally, however, the fevers contracted there are of a more serious character, being of a decidedly

typhus or typhoid type. Thus, in 1857, the *Blenheim*, the steam guard-ship at Portsmouth, had nineteen cases of this sort among her crew; the attacks occurred "at the same time that a severe epidemic prevailed among the marines at Fort Monkton and at Forton barracks." The fever in the *Blenheim* was preceded by a prevalence of diarrhoea, in some cases of a choleraic character, on board. In 1858, forty-one cases of fever occurred in the *Victor Emmanuel*. The extensive mud-banks around that harbour are said to be but little, if at all, productive of any febrile malaria, or of other noxious effects upon the health of the crews in the port. The same remark is made, in the official reports, as to the mud-flats along the line of the Southampton water. Very different, however, is the case with the estuary of the Medway, which is decidedly the fever locality on the Home station, and where not only a considerable number of cases of fever, but also much other disabling sickness, annually occur among the crews of our ships of war and the workmen in H.M. dockyard, as well as among the civil population of Sheerness and its neighbourhood, from malarious agency. The fevers are generally more or less periodic or agueish in their type; occasionally they are attended with severe gastric and bilious symptoms, and then they are usually more typhoid in their character. The ships anchored high up the Medway generally suffer more than those off Sheerness, which lies open to the winds from the German Ocean. The extremely insalubrious state of that town and of the country round, as affecting the health of the population both afloat and on shore, was brought before the attention of the Privy

Council in 1859; but whether any improvement has since been effected, I am unable to say. In the Appendix are given some of the details respecting the amount of sickness in the ships of war and among the workmen in the naval dockyard, which seem strongly to call for the attention of the Government.

Fevers are generally three or four times as frequent and as fatal in the Mediterranean as in the Home fleet. The majority of the cases are of a mild continued or remittent type; a fifth part or so are intermittent. The average frequency of febrile attacks of all sorts in our ships of war on that station seems to be about seventy in every thousand men, during the twelve months. The annual death-rate from this cause has averaged about two in the thousand. On this, as on other stations, the fever-sickliness of certain ships, while others remained exempt, was very marked in each of the three years.

I now pass to the West India station, where, of recent years, by far the greatest amount of fatal fever has occurred in our naval force. Fully one-half of all the deaths from fever throughout the service, in the three years under consideration, were caused by that fatal pestilence known as yellow fever. Much instructive evidence may be gathered from the details, although in many respects these are insufficient, given in the official reports respecting its history on ship-board.

In 1856, it prevailed with virulence in four ships of the West India squadron, which numbered thirty-seven vessels in all. In one ship, the *Malacca*, it raged with such violence that no fewer than fifty-six

out of a crew of one hundred and sixty-five perished within six or seven weeks. Many of the vessels of the squadron escaped altogether, while others had only two or three sporadic cases of the disease.

In 1857, out of a squadron of twenty-five vessels, two only suffered to any considerable extent. One, the *Brilliant*, lost thirty-four out of a crew of two hundred and thirty in five or six weeks; and the other, the *Orion*, had fifteen deaths; and the notable fact in the case of this ship was that most of the fatal attacks occurred after leaving the West Indies, and during her voyage to England; three actually occurred in the naval hospital at Plymouth.

In 1858 the squadron suffered much less severely, the total mortality from the disease that year being only eighteen. Seven of the deaths occurred in the *Leopard*.

This very fatal form of fever appears to have been, at times, more destructive in our ships of war within the last fifteen or twenty years than was ever known before. All the vessels which have been smitten most severely have been steamers. No sailing ships in former times seem to have sustained such terrible losses as the *Eclair* in 1845, the *Dauntless* in 1852, the *Malacca* in 1856, and the *Icarus* in 1860. This fact is very suggestive. That the excessive heat on board, without adequately free ventilation, has had much to do with the increased malignancy of the distemper is more than probable. It would seem, too, that the accumulation of dirt and other offensive rubbish under the machinery, etc., has in several instances added not a little to the impurity of the close hot air in the between-decks. Reference is fre-

quently made in the surgeons' reports to both these evils. On some occasions, particular parts of a smitten ship appear to have been the especial focus of the poisonous action. Thus, in the cases of the *Argus* and the *Virago*, it was "about the after part of the lower deck and in the fore-part of the engine-room," and the mortality was greatest among the men berthed near these parts; and, in the *Leopard*, nearly all the attacks occurred among the men living in the steerage, where they had been more exposed than the rest of the crew to "an offensive effluvium which had for some time previously issued from the hold and spirit room." On examination much black mud, mixed with half-rotten chips, which had been accumulating for a long time, was found in the limbers. "The exhalations from that part of the ship, the surgeon believed, were the cause of the yellow fever, as the malarious influences from the shore were the cause of the cases of remitting fever."

The prompt abatement, and often the complete cessation, of the disease when the crew of an infected vessel have been moved out of her into clean, airy quarters on shore or afloat, afford conclusive evidence how much the ship herself is apt to be the chief cause of its malignancy and persistence. One or two instances may be quoted. In the case of the *Argus*, the disease all but ceased at once after the crew, sick and well, had been landed at Bermuda. The surgeon was of opinion that "the fever was contracted from local causes exterior to the ship, but that the morbid poison became localised within her. Not one of the attendants at the hospital on shore

was attacked, whilst nearly all those who attended the sick on board became the subjects of the fever." In the case of the *Dauntless*, fresh cases continued to occur for weeks among the portion of the crew left on board, after the sick had been sent to the military hospital at Bardadoes, where they were placed in the same wards with other patients, without a single instance of the spreading of the disease. The same thing occurred in the case of the *Highflyer*, at Port Royal in 1852, and also in more recent instances.

For almost all practical purposes of prevention and arrest, yellow fever may be regarded in the same light as the typhus fever of our own country. All know how much the virulence and diffusion of the latter are under control by wise sanitary precautions; and nearly the same thing may be said of the former. Both are liable, in a confined impure atmosphere, to spread by contagion from the sick to the well; whereas the risk of such an accident is reduced almost to zero in the airy wards of a clean hospital. The inference from this fact clearly is that, by the simple expedient of securing an ample circulation of pure air at all times, but more especially from sundown to sunrise, to everyone without exception on board a sickly ship, the virulence and extension of the disease would be greatly controlled. The not unfrequent increase of the disease for the first few days after leaving harbour is doubtless owing to the impeded circulation of air in the between-decks, principally at night, from the closing of port-holes, etc. Whenever the sick have been treated in tents or under an awning on the upper deck, instead of in the usual sick bay, and the between-decks have

been kept freely ventilated, salutary effects have been always obtained.

The marked influence of a high temperature in favouring the development and progress of yellow fever, and of a cool climate in mitigating and arresting its course, is a point of much practical interest, as indicating the advantage to be derived by the removal of a sickly ship from the tropics to a higher latitude, when, notwithstanding the adoption of every available precaution, the disease continues on board. By increasing to the utmost the continuous circulation of air throughout the ship, and dispersing the men as much as possible, chiefly on the upper deck or decks, a certain amount of benefit is sure to be obtained. Instances, however, will occur where the only safety is in leaving the West India station as quickly as possible, and moving to the northward. Much fresh sickness, and even considerable mortality, it is however to be remembered, may take place during the passage, and before the vessel is able to reach a sufficiently cool climate, if the crew continue to occupy the close and infected between-decks as hitherto. Thus, the *Malacca* had eleven deaths on the passage (eleven days) from Port Royal to Bermuda, and thirty-eight fresh cases of the fever. The *Firebrand* lost ten men on the passage (twelve days) between Port Royal and Halifax, and had on arrival no fewer than seventy-nine of her crew laid up. The *Spiteful* took seven days in going from Nassau to Halifax; eleven men had died on the passage, and there were forty-six cases of fever on the sick list when she reached that port. Occasionally, from stress of weather, the voyage from the West Indies

to the North is rendered very tedious, occupying as many as twenty days to Halifax; and in a few instances, the sick ship, being unable to reach even Bermuda, has been forced to bear up for England.*

These circumstances serve additionally to shew how much this subject—the outbreak of yellow fever in our vessels of war—calls for a thorough inquiry into all the circumstances which may, on the one hand, favour, and which, on the other hand, may mitigate or arrest its virulence on board ship. The necessity for such inquiry is strongly urged in the last published Statistical Return, that for the year 1858, in consequence of the very disastrous loss of life in several vessels of the squadron during the past year,—so disastrous that Admiral Milne has recently given orders for the more prompt abandonment of the tropics by all infected ships in future.

The West Indies is not the only station which has suffered from yellow fever. The fleet on the east coast of South America sustained serious losses from

* H.M.S. *Barracouta* sailed from Jamaica for Bermuda on the 24th October, 1860. Yellow Fever appeared during the passage; besides her ordinary crew, she had upwards of a hundred supernumeraries on board. On arrival at Bermuda, "the civil authorities, dreading the introduction of the fever into the island, would not permit any communication, or allow the sick to be landed. Medicines, fresh provisions, and other necessities were, however, sent on board." While making for Halifax, she encountered a violent gale which compelled her to shape a course for England, where she arrived on the twentieth day after leaving Bermuda. Of twenty cases of fever on board, six proved fatal.

If the above statement, respecting the conduct of the civil authorities at Bermuda, be correct, this was most reprehensible, and surely calls for official explanation. It may seem strange that the naval authorities did not insist upon the sick being landed, especially as there is a separate naval hospital on one of the islands, and as the *Barracouta* was also very much over-crowded at the time.

this disease in 1856-57-58, as well as in some previous years. Formerly, the Brazilian station was singularly healthy and exempt from all malignant fevers. From 1830 to 1843, the yearly death-rate in the fleet there did not average more than a trifle over seven per thousand of the strength—a ratio less than that upon the Home station. But for the last ten or twelve years it has been very much higher; and this in consequence of the yellow fever, which first appeared in Brazil in 1849-50.

On the Pacific, the Cape of Good Hope and the Australian stations, fevers are comparatively rare and mild. In our West African squadron, there has been a great reduction for a good many years past in the mortality from this cause, the annual death-rate therefrom not having exceeded during the trienniad under notice five per thousand of the strength.

This reduction has been owing in part to the absence of the malignant form of yellow fever from the African coast in that period, and for three or four years previous; in part also, to the salutary changes which have been introduced, mainly on the recommendation of the medical department of the service, into the mode of conducting the duties of the station. "By a wise and humane regulation, the deadly practice of sending boats away on detached service, to watch or intercept slavers, has been interdicted, or, at all events, greatly restricted. Prize crews are no longer turned adrift to wander through the streets of Sierra Leone, where the vessels they navigate from distant parts of the station are delivered up to the authorities of the mixed commission court; the orgies of 'the barn,' which lowered the character of the

white man in the eyes of the black, have long since ceased; and last, though not least, the introduction of quinine wine as a preventive of fever has not only reduced the number of febrile attacks, but has lessened the severity of those which do occur; and thus the mortality has also been reduced to a level which does not materially exceed the death-rate from fever on some of the more healthy stations."

Eruptive Fevers.

This class of diseases is, as might be expected, comparatively rare among seamen. Occasionally, measles and scarlatina appear in particular ships, chiefly on the Home Station. But the only exanthematous fever requiring notice is the small-pox.

The extent to which this disease has of recent years existed in the Navy will be seen from the following facts. In the Black Sea fleet in 1854 and 1855 there were eighty-one cases, of which five proved fatal. In the Baltic fleet during the same years, three hundred and three cases occurred (eighty-five in the *Neptune* line-of-battle ship alone), and of these eighteen were fatal. In the majority of instances, the disease seems to have been caught in Portsmouth or Plymouth before the ships sailed.

In 1856 the total number of cases of small-pox throughout the entire service was thirty-nine. They occurred chiefly in the Home and Mediterranean fleets. Three of the cases ended in death. Whether the patients had been vaccinated or not, is not stated. In 1857 the number of cases in the service amounted to twenty—ten on the Home, and six on the East India station. No case proved fatal. In 1858 the number

of cases was eighty-three—forty-seven on the Home, and thirty-one on the East India station. There were ten deaths.

"By a circular recently issued by the Director-General, the medical officers of the Navy are instructed to vaccinate, on their entry into the service, all persons who do not present marks of variola, or of previous vaccination. This will, no doubt, tend to lessen the virulence of small-pox, and to reduce the mortality consequent upon it." On more than one occasion, our ships of war have introduced the disease into populations on shore. Thus the destructive epidemic at Malta in 1830, when fifteen hundred deaths occurred, was said to have been brought to the island by H.M. ship *Asia*; and it was only last year that two seamen, from H.M. ship *James Watt*, were landed with the disease at Gibraltar, but where from the precautions taken it spread to a limited extent only.

Diseases of the Bowels, chiefly Alvine Flux and Cholera.

There is one station where these diseases prevail with unusual severity in almost all years, causing the principal amount of sickness and mortality among the fleet employed in that part of the world, viz., the Indian and Chinese waters. On all the other stations, they occasion a much smaller comparative amount of the sickness, and are, moreover, far less persistent and destructive. Everywhere at times, more especially during the summer and autumn months, there are occurring more or less severe outbreaks in particular ships, which it is often not easy to explain. Such local epidemics of diarrhoea, etc., are not unfrequent in ships lying in harbour on our

own coast, as well as in the Mediterranean and elsewhere. The diarrhoea is sometimes of a decidedly choleraic character, and occasionally sporadic cases of malignant cholera are met with, usually between the beginning of July and the end of September. Dysenteric affections are, as a general remark, decidedly more frequent and severe in the Mediterranean than in the Home fleet; but the mortality resulting therefrom is seldom considerable. On the West India station they are still more common, more especially in ships which have been long engaged off Greytown and some other ports in the Gulf of Panama. Dysentery is, however, far from being a cause of much fatality on the station generally. It is the East India and China fleet that calls for special notice under this head.

Out of the entire number, 492, of deaths caused by alvine flux (dysentery and diarrhoea) throughout the service in the three years, 425 occurred on this station; in the year 1858 alone, there were 252 deaths. It was on this station also that most of the deaths from cholera took place. Out of the entire number, 160, no fewer than 138 were in the ships engaged in these seas; and of these 110 occurred in 1858.

The excessively high rate of sickness and death on this station, not only during the three years under notice, but on all former occasions when our ships have had to anchor for a length of time in the estuaries of the large rivers, or off the low and swampy coasts of these regions, is a subject of great public importance. The nature of the sickness is almost always the same, viz.: the generally associated diseases

of dysentery and remittent fevers, of which, the former is far the more fatal of the two. The season of the year has much to do with their prevalence. From December to about the middle or end of April they are comparatively infrequent, but from May to November their prevalence and severity are enormously increased. Hence the necessity of keeping this fact steadily in view, in time of peace as well as of war, as far as the exigencies of the service will admit.

In 1841, such was the amount of sickness and mortality which followed the capture of Canton (during the hot season) that "it was almost fatal to the efficiency of the Naval force." In 1857, the city was taken on December 29th, and "notwithstanding the fatigue and exposure of the men, the number of febrile and diarrhoeal attacks which followed was by no means great—owing, it is presumed, to the season of the year being more favourable to the health of Europeans than that chosen in 1841."

The attack on the Peiho forts in North China took place on May 13th, and on the 22nd the force proceeded up the river to Tiensin. The men continued in good health. On the conclusion of the treaty, the force was withdrawn and reembarked in the beginning of July. On the whole, the expedition suffered comparatively little from sickness.

A good deal of the sickness on this station among the men is doubtless owing to their recklessness and wilful neglect of the most obvious precautions; for the officers, generally speaking, suffer much less in proportion, in consequence mainly of their greater attention to food and clothing, and of less intemperance.

The use of the water of the rivers, contaminated as it is with manifold impurities, has been reasonably enough suspected of having not a little to do with the excessive prevalence of alvine flux in the Indian fleet; this idea seems to be confirmed by the extreme frequency of intestinal worms among the sick. But this subject still needs further examination, as it is alleged that the crews of some ships, who have used only water distilled from their engines, have suffered as much as others where that practice was not adopted.

The occasional marked immunity of some ships from the diseases which prove so prostrating to others is highly suggestive, and demands a more searching inquiry than it seems yet to have received; for what is true of one vessel may reasonably be expected of others under like conditions. There is certainly not a more important problem in Naval and Military hygiene than clearly to determine all the circumstances which favour the induction, and aggravate the severity, of alvine flux. The food, the drink, the clothing, the exposure, the nature and amount of fatigue duties, the accommodation of the men, the mooring of ships, the site of barracks, these and many other points all need to be examined into.

That the tendency to diarrhoea and dysentery is greatly increased by the breathing an impure atmosphere is, as already stated, a recognised fact in medical science. They are the most common pest of crowded and ill-found emigrant and troop ships in all climates, and no diseases have been found more wasting to armies whether in the field or in unwholesome quarters and cantonments.

Overcrowding in a ship will, indeed, occasionally

continue for a considerable time without being followed by any distinct disease, such as fever or flux; but that the health of the persons so treated is deteriorated is shewn by the fact of their being more than usually susceptible of all noxious agencies, on landing after a lengthened voyage. This truth was terribly exemplified in the case of some of our regiments—the 98th for example—in 1841, which lost half their force within three months of landing in China. And, as if to make the experiment complete, it was found that the men who had been quartered on the orlop or the lowest and worst aired deck in the *Belleisle*, in which the regiment had been brought out from England, suffered in a higher ratio than the others.

The power of resisting the hurtful influences of a malarious climate depends quite as much upon the maintenance of a vigorous tone of health throughout the voyage up to the moment of landing, as on the vigilant use of precautionary measures after arrival.

As to the influence of Diet on the prevalence and character of dysentery in India and China, it has been observed as the result of long experience that the disease is much less frequent and severe among the natives than in Europeans, and more especially among British sailors; and this difference has been attributed mainly to the use of a less stimulating diet, and to the non-use of spirituous liquors.

On the subject of the water used for drink, it is to be remarked that the Chinese are said to generally boil it, and then add a little alum to it. "It is worth mentioning," adds the Naval Report of the health of the Navy for 1837-43, "that a number of Chinese prisoners who were confined for some time on board

the *Cornwallis*, and who had no opportunity of boiling the water they used, or of adding alum to it, suffered from diarrhœal attacks somewhat in the same ratio as the ship's company."*

With another interesting extract from the same report, I conclude this part of the subject. "The more general prevalence of diarrhœa and dysentery amongst Europeans residing or voyaging on the coast of China, and even to some extent on nearly the entire southern sea-board of the Asiatic continents, than amongst aliens of the same class residing in other countries or regions in the same parallels of latitude, is a most remarkable fact; whether these affections arise from some peculiar condition of the atmosphere, from telluric emanations, peculiarity of food or water, sufficient evidence has not yet been adduced to enable us to form anything like a satisfactory opinion; for it appears to be a fact, established on the testimony of many intelligent medical officers, that diarrhœa has sometimes attacked the crews of vessels on entering the rivers in China before they had any communication with the land, and before they had made use of any article of food or drink belonging to the locality. Hence in these cases it was generally supposed that the disease originated from malaria or marsh exhalations suspended in the atmosphere; but

* The elixir of vitriol, or dilute sulphuric acid, may be advantageously substituted for the alum. A very pleasant and useful beverage, I may remark, in hot climates and seasons, is made by adding a sufficient quantity of it to water, (or, what is still better, to cold tea), to impart a grateful acidity, sweetening to the taste. This is the *sulphuric lemonade* of some French hospitals. I have known it used with decided advantage as the common drink in diarrhœal and choleraic seasons, and can recommend it from personal experience.

then again, at other times, there were ships' companies who entirely escaped these affections until they began to use the river-water. Thus the evidence which appeared to be conclusive as to the influence of malaria on the health of the crew in one ship was found to be wanting in another, while the evidence which appeared to be equally strong as to the influence of the water in a third was, in a manner, negatived by the eruption of the disease in a fourth before the men began to use it."

Diseases of the Respiratory Organs, including Consumption.

The amount of sickness in the Navy generally from these maladies exceeds that from any other single class of diseases, and often equals that from all fevers and alvine fluxes together. To take one year for example—in 1856, a sixth part of the entire sickness throughout the service arose from this cause. The proportion varies, as might be expected, on different stations; but the influence of mere climate is by no means so great as is generally supposed. For although not so high as in the Home station, where atmospheric vicissitudes are more rapid and considerable than elsewhere, there is still an immense amount in the milder and more equable regions of the Mediterranean, and even under the tropical skies of the Old and New World alike. The mere inclemencies of weather at sea, even when combined with exposure and great fatigue, have much less to do with the frequency and severity of these ailments than most people suppose; but it must be acknowledged that the reckless habits of seamen when on shore, and their disregard of

ordinary precautions against wet and cold, contribute not a little to their production. But with chest and throat diseases, as with fevers and diseases of the bowels, we have again to notice the curious fact that there is often a marked difference as to their prevalence in different ships of the same squadron, lying at the same localities, and engaged on the same or similar duties. Whether this difference is mainly owing to the damper state of the between decks in some ships than in others, according as the wet or dry mode of cleaning them be in vogue, or whether some crews have greater facilities of drying their wet clothes and getting warm food or refreshment on going down below after a cold and stormy watch and before turning into their hammocks, or whether from any other cause it is not easy to say; but that such a difference exists is a notorious fact, and one that well merits examination.*

The point which might least have been anticipated in respect of this class of maladies in the Navy is the great frequency of tubercular disease of the lungs, or pulmonary Consumption. Sea-faring life is usually considered as prophylactic of, rather than predisposing

* "Generally speaking," says the Report for 1856, "the comparative frequency of inflammatory affections of the lungs, in the Home Force, is to be ascribed to the exposure of the men to cold and wet, which it is difficult to avoid when there is a necessity for employing them on dockyard duties and in boats, and their being quartered in cold, damp, and windy hulks during the winter months, where they have few opportunities for drying their clothes." As showing the difference in different ships' crews, employed in similar work, it is stated that "in the *Royal Williams*, the *Hawke*, *Formidable*, and *Blenheim*, there did not occur a single case of inflammation of the lungs and pleura, while in other ships they amounted, in some instances, to eight, and even to fifteen or sixteen."

to, the weakened and deteriorated state of the constitution on which the development of this disease depends; and experience has certainly shewn that civilians, with a decided tendency to it, have often got rid of every symptom by making a few lengthened voyages. How comes it then that the malady is so common among our seamen, and this too not on one station only, but in almost every part of the world to which our fleets are sent?

We are not surprised at the amount of mortality from this cause among the crowded tenants of the ill-aired dwellings of our manufacturing towns—ill-fed it may be, and often insufficiently clothed; but that it should be equally or even more frequent among our well cared for sailors, with the pure air of heaven always around them, is certainly what was not to be anticipated. Consumption is the most uniform and persistent cause of the large destruction of life in our Navy. Fevers, cholera, dysentery, and, indeed, every other malady vary in point of frequency; in some seasons they are very prevalent and fatal, in others they are comparatively infrequent and innocuous; but in all years, and nearly alike in all climates, phthisis eats its slow and inevitably fatal course into the strength of our Navy, and causes on the whole a greater amount of permanent loss to the service than any other single malady. During the three years, 339 deaths were caused by consumption, and 111 by other diseases of the respiratory organs, chiefly pneumonia and bronchitis. In the same period, 533 seamen were discharged out of the service on account of consumption (which in the great majority of cases would prove fatal within six months of their dis-

charge), and 171 on account of other confirmed pulmonary diseases.

The ratio of the deaths from consumption alone averaged, for the three years, 2.6 per thousand of strength; and the corresponding ratio of the number of men invalided on account of this disease was 3.9. The amount of the permanent loss from this one disease is thus seen to be excessively great.

One remark only as to the predisposing and favouring causes of this evil—a wide subject if discussed in its various bearings—need be made at present, merely to recall to general attention the well-considered conclusion of the recent Royal Commission on the sanitary state of the Army respecting the cause of the great prevalence of pulmonary consumption among our soldiers generally, and its inordinate frequency among the household troops, viz., the close foul air of the crowded barrack-rooms at night.* Is the atmosphere of the lower between decks of a ship of war, after the men have turned into their hammocks, at all less impure? To use a common phrase, it is so thick that you might cut it. And when it is

* The words of the Commissioners' Report are these: "That the ravages committed in the ranks of the army by pulmonary disease are to be traced, in a great degree, to the vitiated atmosphere generated by overcrowding and defective ventilation, and the absence of proper sewerage in barracks."

The inquiries which have recently been made, to ascertain the causes of excessive mortality from lung diseases in certain districts of England, have led Dr. Greenhow to the conclusion that, "working in ill-ventilated and over crowded rooms," and "residing in dwellings so constructed, that the bedrooms are badly ventilated, and the cubical space per head is inadequate to the preservation of health, powerfully aid the operation of other injurious conditions in favouring the development of these disorders."—*Report of the Medical Officer of the Privy Council, for 1860.*

remembered that the men have suddenly to turn out from this close, damp, and fetid air at night to go suddenly on deck in all weathers, the exceeding frequency of those illnesses, which always serve to accelerate the development of tubercular disease, is only what might be expected. That a large amount of the inflammatory affections of the throat and lungs in the Navy is due to the cause here indicated, will not be questioned by any medical officers in the service; and the experience of the present day is but the repetition of what has been strongly and repeatedly testified to by former observers. Sir G. Blane pointedly alludes to this subject as calling for the attention of the Admiralty in his day, and as one where salutary improvements were manifestly required. In the present day, when most ships of war are provided with steam power, and the heat of the between decks is, therefore, apt to be still greater than it used to be in former times, the necessity for the suggested improvements is the more pressing.

Diseases of the Brain, etc.

There is no class of diseases so fatal, in proportion to the number of attacks, in the Navy as those of the nervous system. By far the greater number of deaths under this head (there were 172) are due more or less directly to intemperance, and are classed under the designation of apoplexy and delirium tremens. The number of men hopelessly ruined in health and discharged as invalids from the service, chiefly from epilepsy and insanity,* swells the above figure to

* In the three years, seventy-six seamen were invalided on the ground of insanity; and that a good many who were discharged on account of

501, representing the total permanent loss in the course of the trienniad. But even this aggregate gives but a very imperfect idea of the entire sacrifice of life and service arising from this national vice; as it is well known that, independently of many febrile and other attacks attributable to intoxication and its consequences, very many of the violent deaths from drowning, suicide, etc., occur, and nine-tenths of all the crimes and offences are committed, while under the influence of liquor. How to diminish this great public evil by the substitution of acceptable and less pernicious beverages, and in other ways, is still a question for further inquiry, although it has more than once come under the consideration of our Naval authorities.

Amount and Causes of Invaliding in the Navy.

The amount of permanent loss to the service by Invaliding from disease alone always largely exceeds that by Death from the same cause. During the three years, the relative proportion was two-fifths greater. The total number in this period invalided was 4221, and of this aggregate 3808 were from disease alone; the deaths during the same time from disease being 2125. Diseases of the lungs were the occasion in 692 cases, and three-fourths of these were consumption. Diseases of the bowels, chiefly dysentery, afforded 585; of this number 324 were from

epilepsy, palsy, and delirium tremens, would eventually lapse into some form of mental weakness can scarcely be doubted.

"The great proportion of maniacs among seamen is chiefly owing to injuries of the head received in a state of intoxication."—SIR G. BLANE.

the East India and China fleet, in 1858 alone. 162 cases were due to the sequelæ of fevers.

Various maladies which add but little to the death-list, annually cause a large amount of invaliding. In the three years,

448	cases were due to	Rheumatic affections.
286	"	" Diseases of the heart.
263	"	" Ulcers and abscesses.
244	"	" Hernia or rupture.
243	"	" Venereal diseases.
235	"	" Debility—complicated generally with some internal organic mischief, often of the lungs.

The great prevalence of rheumatism, with its disabling effects, is mainly due to the same external influences which occasion so much sickness from diseases of the organs of breathing;—viz., the cold damp state of the between decks in some ships; but principally to the abrupt transitions at night from the heated atmosphere below, when the pores of the skin are open, to, perhaps, a cold and wet watch on deck. The combined loss from these two orders of disease alone amounts to between a third and a fourth part of the total number of men, invalided in the course of the year.

Losses by War, Accidents, &c., compared with Losses from Disease.

I have now to compare the losses arising from the accidents, including drowning, to which seamen are specially liable, and also from the casualties of war, with the losses which have been shown to result from disease. For this purpose I have selected the Baltic

and Black Sea fleets in 1854 and 1855, and the East India and China squadron in 1857 and 1858, during the period of hostilities with China and the Indian mutiny.

The total mortality in the fleets during the Russian war, including the naval brigade and marines serving with the army before Sebastopol, was as follows:

From diseases	-	-	-	1574*
" accidental injuries, suicide, and drowning	228			
" wounds received in action	-	-	227	
				2029

In the Baltic fleet, in 1854, only fifteen deaths occurred from wounds in action at Bomarsund, Hango, etc.; and in 1855 there were but eleven, of which five occurred from an attack by the enemy on a boat's crew, while landing some Russian prisoners under a flag of truce.

In the Black Sea fleet, in 1854, the total number of deaths from the casualties of war, afloat and on shore, was ninety-four; of which sixty occurred from the attack by the fleet on the sea-batteries of Sebastopol, and nineteen in the naval brigade (about 1200 strong), between the 2nd October, the date of landing, and the end of the year.

In 1855, there were ninety-eight deaths in the brigade from wounds received in the trenches before

* By far the greatest mortality was from diseases of the bowels, principally malignant cholera; 861 of the deaths were so occasioned. Diseases of the respiratory organs caused 217 deaths; fevers 172 deaths, exclusive of 29 deaths from the exanthemata, chiefly small-pox; and diseases of the brain 58 deaths.

Sebastopol, and six on board ship, either in the Black Sea, or the Sea of Azof.

Among the East India and China squadron, in 1857, there were eighty-seven violent deaths out of a total mortality of three hundred and twenty-seven. Of these eighty-seven, thirty-eight were from wounds received in action: viz., twenty with the Chinese, and eighteen in the naval brigade (520 strong) serving in India, against the revolted sepoys. Twenty-nine deaths resulted from drowning, thirteen from accidents, two from suicide, three from causes not stated.

In 1858, out of a total mortality of seven hundred and six, of which one hundred and twenty were deaths from violence, thirty-five were from wounds in action, twenty-seven in China, and eight in India; forty-nine deaths were caused by drowning, seventeen by accidents on board ship, three by suicide, one by a stroke of lightning, and fifteen from causes not ascertained, but "probably the result of injury or violence while on shore on leave, or in action with the enemy."

In the three years, 1856-7-8, eighty-two sailors and marines lost their lives in action, and eighty-nine by drowning.

The facts now stated show how small is the proportion of deaths in our navy, from the mere casualties of War.*

* Like results occurred during our former protracted hostilities with the Chinese, from 1840 to 1843. "The total deaths from wounds received in action amounted to 24, or in the proportion of about one annually in the thousand of mean strength; while the total loss from febrile and dysenteric complaints alone amounted to 547, or in the ratio of nearly thirty to the same amount of force." Even as com-

Lastly, the proportion of men discharged out of the service in consequence of accidents and wounds, as compared with the number discharged on account of disease, is inconsiderable. During the three years, the number invalided from the former cause amounted to four hundred and eighty-three, out of a total of four thousand two hundred and twenty-one from all causes, or in the proportion of nearly a tenth part of the whole.

And now what are the inferences which seem to be fairly deducible from the foregoing facts and statements?

That a considerable portion of the sickness—the sickness, too, which is apt to be the most disabling and fatal—is owing to agencies which are well ascertained, and which are certainly not inevitable to ship-board life under any exigencies of public duty, will, I think, be generally admitted. The perfect soundness of the observation, already quoted on the title page of this letter, that "It is only by clearly and distinctly pointing out the causes which affect health, that we can hope to avert disease, and reduce the rate of mortality in the naval service," will be accepted by every sanitarian inquirer. Now, no one can well doubt as to the cause or causes of the exceeding and persistent unhealthiness of such ships as the *Conqueror*, the *Dauntless*, and the *Eclair*; or of the typhus fever generated on board the *Princess*

pared with the number of deaths from other kinds of external violence, that from gunshot wounds was but small; for the deaths from ordinary casualties, principally accidents sustained by falling from aloft or into the holds, were forty-one, and no fewer than one hundred and nine were caused by drowning, five of which were suicidal.—*Statistical Report of the Navy (East India Station)*. 1853.

Royal, and the terrible malignancy of the yellow fever in the *Malacca*, and other vessels of the West India squadron in different years; or of the disastrous virulence of the cholera in the *Britannia*, at Varna. That the extreme sickness and loss of life in the above instances were, to a large amount, avoidable, or, in other words, would not have occurred under circumstances less unfavourable to health, seems to me all but certain. And if this be true in regard of these ships, it is only but reasonable to believe that similar causes in other ships have given rise to like effects, though in a less degree, and varying according to the circumstances of each ship and her crew.

With respect to the all important subject of ventilation, it is now, I believe, nearly universally conceded, by executive as well as medical officers, that the accommodation for the men at night, in most ships of war, is insufficient;* and, also, that there is really no adequate reason for continuing the old usage of berthing almost all the crew upon one deck,

* The following description of the berthing of the men at night, and of its consequences, was given in the first *Statistical Report of the Navy*, in 1840. "The usual space between the suspending points (clues) of the hammocks is from seventeen to eighteen inches, so that, when they are extended by the beds, their bodies are in contact. The effect is to bring the bodies of the men into contact in greater or less number, according to the size of the ships. When at sea with a watch on deck, the accumulation and pressure are reduced by a half; but when in secure harbours, five hundred men perhaps sleep on one deck, their bodies touching each other over the whole space laterally, and with very little spare room lengthways. The direct results of elevated temperature and deteriorated air may be conceived; but it is not easy to conceive the amount of the first, nor the depressing and debilitating power of both, as measured by sensation, within the tropics. The tendency of such a state of things must be to subvert health, and lay the subject of it open to attacks of serious disease."

and that, generally, the least well-aired. I have elsewhere expressed my sentiments on this subject (*vide* Appendix), and beg to refer to them.

The between-decks of some ships are, it is known, so hot and close that for several months at a time, within the tropics, the men are compelled to sleep on the upper deck under canvas. The ordinary wind-sails are most imperfect ventilators at best; and often they cannot be used when most wanted. A good system, whereby the purity of the air in the lower decks may be maintained, especially when the ports are closed, is still a desideratum. Whether the Committee of Officers, that has recently been appointed to suggest means for improving the ventilation of war steamers, has had their attention directed to the berths of the crew, as well as to the engine-room, etc., I am not aware. It is a point that calls for special consideration, as of recent years almost all the instances of extraordinary mortality have occurred in steam vessels. Indeed, the whole subject of the relative healthiness of sailing and of steam vessels needs to be looked into. In the Baltic fleet in 1854, there was certainly a lower sick rate in the sailing ships of the line, than in the screw liners; and the former suffered much less from diarrhoea and cholera than the latter. In the Black Sea fleet too, in 1854, and again in 1855, the *Queen* and the *Rodney*, both sailing ships, were the most healthy of the squadron. Probably this is just what might be expected, considering the greater heat and closeness, as well as the large store of coal or other fuel, on board steamers; unless, indeed, extra means are resorted to for maintaining a more thorough circulation

of air—a purpose which the machinery in action may always be made to subserve.

It has been seen from the evidence of many officers that the condition of the holds in some ships is very far from what it is desirable that they should always be; for that the exhalations from the lodgment of offensive matters there, and occasionally from the timbers themselves, are productive of mischief to health and predispose to and greatly aggravate, if they do not directly engender, serious disease, can scarcely be doubted. This conclusion is only in accordance with the daily experience of civil and military life in like circumstances; for a foul hold is only a lengthened drain or gutter without the means for the ready escape of its contents, and these too subjected to constant shaking about from one side to the other. The miasmata from such a hold are sometimes spoken of as if they consisted merely of sulphuretted hydrogen—a gas, which, though certainly most noxious to life, cannot be regarded as ever the generator of fever, flux, or gangrene. The gas is but the accompaniment and the evidence of a more serious effluvium; the real source of the mischief arises from the putrescent decomposition of organic matter. Such miasms are, I believe, only less mischievous than the miasms proceeding from living bodies.

Sometimes, and even of recent years, the hold of a ship has been found to be anything but sweet shortly after she has been put into commission. This may have arisen from the practice of keeping a certain number of ships in an advanced state, *i.e.*, ready for commission, with their ballast on board, it may be for several years. When this is once disturbed by the

motion of the ship, the existence of something faulty may be only then discovered. Hence the desirableness that a thorough sanitary inspection and report be made of every vessel before she goes to sea, as well as upon her return to port at the end of her commission.

That not a little of the pulmonic disease, and also of the rheumatism, among seamen is capable of prevention is more than probable when we consider the very frequent cause of these ailments, *viz.*, the abrupt transition from the overheated berth to the cold outer air, on every change of night watches. Humidity, too, is a great promoter of sickness. The dryer dwellings are, the healthier *ceteris paribus* they are. Medical officers are pretty generally agreed that damp lower decks serve to swell the sick list, and that it would be better for the men if dry cleaning and rubbing with hot sand often took the place of washing and wet holy-stoning. What was said in the first published Health Report, in 1840, on this subject was the result of extensive experience on the part of Dr. Wilson in all parts of the world;—"Evaporation, especially in low decks and low degrees of temperature, goes on slowly and, therefore, long; in hot climates it is, of course, more rapid and sooner completed; but in either case there is strong reason to conclude that the effects on health are injurious, sometimes highly so. Their power to excite catarrhal and rheumatic affections will not be questioned; nor ought there to be much question as to their power of exciting many of the inflammatory affections of the lower extremities, which in some ships give rise to much inconvenience and suffering. They tend to

reduce physical force, and thus co-operate in the induction of diseases of debility, or render the body more susceptible of attacks of violent disease."

It is unnecessary to say that it is always of much importance that the men should have the ready means of quickly drying, in a suitable place, their wet clothes when they go below.

The hurtful effects on health of a humid atmosphere are aggravated by the inevitable absence of sun-light from the lower decks of a ship. This point has not escaped the attention of Dr. Bryson in his suggestive paper on Medical Statistics in the Manual of Scientific Inquiry, prepared for the use of officers in H. M. Navy by authority of the Admiralty.*

On the subject of the food and drink in the Navy I have but little to say, and that little will have reference to the East India and China squadron, which, as we have seen, suffers so disproportionately from unmanageable and very fatal bowel complaints. It must be admitted, I fear, that our multiplied experience in Chinese waters for many years past has not yet enabled us to reduce the amount of sickness and mortality to which our ships of war are liable, whenever they have occasion to be long detained in any of the great estuaries. But it would be unjust

* "The marked difference in the appearance of men employed in the bread room and holds, compared with those who are freely exposed on deck, or in open boats, at all hours of the day, cannot escape the notice of the most superficial inquirer. It is, therefore, of importance to ascertain whether exclusion from the solar rays be not, to a greater extent than is generally believed, one reason why men who have acquired a pale waxy look from confinement below are more susceptible of disease, and less capable of sustaining its shock, than are those whose blood is enriched and strengthened by the free exposure to light, heat, and air."
—P. 240, 3rd edition, 1859.

to sanitary science to believe that no reduction can possibly be effected in such a climate. That the climate has much, very much, to do with the extreme sickliness is beyond dispute; but then what are the circumstances and conditions which predispose the system to be most promptly and injuriously affected by the climate? Experience seems to point first and mainly to the nature of the *ingesta*; and, secondly, although in a minor degree, to exposure, especially at night and early morning, without due precautions. That the avoidance, as much as possible, of salted meats, and the substitution in smaller quantities of fresh meats with a larger allowance of rice, and, also, the more frequent issue of baked bread in lieu of biscuits, are most desirable, more particularly during the six hot months of the year, is the natural inference from what we know to be the pathological condition of the alimentary canal in dysenteric affections. And for the same reason, we are pretty sure that the daily issue to the men of a sound wholesome wine, in place, and to the exclusion, of ardent spirits could not fail to be beneficial. Of course, there may be difficulties in the way of carrying out such changes to the extent to which it may be deemed desirable; but still it is highly important to discover at all events what are the favouring, and what are the aggravating, agencies of so disabling and so destructive a disease to our Navy as dysentery. The influence of the water used on board for cooking, etc., is one of the points which appear to be far from being clearly determined, although it is difficult to believe but that the quantity of organic impurities in the Chinese rivers must have a pernicious effect on health. We

require to have a series of accurate chemical and microscopic examinations of the water in different localities, so as to compare the results, not only with each other, but also with the ingredients in the river water of other similar regions, where bowel complaints are less frequent than in China.

How to guard against the recklessness of the men when they are on shore, in respect of intemperance and other matters bearing on their health, I am incompetent to form an opinion; it is only the experienced officer who is entitled to speak on the subject. It is enough for my purpose merely to point once more to the sad sacrifice of health and life, that is still traceable to intoxication in the Navy upon all stations. Things are certainly not so bad as they were forty years ago, when half a pint of spirits was served out daily to every lad and man, throughout the service, whether they wished for it or not. Still much remains to be done to lessen the amount of intemperance among our seamen.

To effect a salutary change in this as in every other branch of naval hygiene, it only needs, I believe, that the attention of all the officers of the service, executive as well as medical, and of the general educated public also, be continually directed to watch the influence of every circumstance affecting the preservation of health and the occurrence of sickness in ships, and that all encouragement be given by the administrative authorities for the promulgation of sound opinions, and the tentative adoption of reasonable suggestions. Let no one permit himself to believe that any thing like perfection has already been attained. We have seen what extraordinary

success Captain Cook achieved, now ninety years ago, in preserving the health of his crew; and we know that instances of similar and even greater success have since been attained by following a like course of action. For example, to cite only one or two cases. About the beginning of the present century, the *Glutton* made the voyage round the world and returned to England after twelvemonths absence without the loss of a single one of her crew, 170 in number; and this, too, notwithstanding that she had carried out upwards of 400 convicts to Botany Bay, on the passage out (*vide* Appendix). And on the last of Captain Parry's Arctic voyages, the annual mortality of the crews was at the rate of only 0.5 per cent. of the strength.

It would be a fine thing, certainly, for science and humanity if ten per cent., say, could be struck off from the present ratios of sickness and mortality throughout the Navy. The saving of the loss of service, not to speak of the expense, would be of no slight consequence in a national point of view. Just in proportion, too, to every diminution of disease and death, so, we may be assured, will the robust tone of health and the vigorous alacrity of the well men be enhanced; for it should never be forgotten that there may be, and certainly is, a great deal of lowered health in all communities, short of the occurrence of actual illness. The problem of State Hygiene is not only how to prevent sickness in the public service, but how to maintain the highest possible effectiveness of the men. For the working-out of this problem in the Navy, the more that the medical officers of our ships are regarded in the light of preservers of health, and not merely of healers of

disease, the better; and, certainly, in no branch of the profession can abler and more enlightened men be found than in the medical department of the navy. The more, too, that the bearings of sanitary science are studied by the executive officers, the more they will, I am sure, appreciate their importance, and the more efficient aid they will be ever ready to give to all the recommendations of the medical officers. It is only necessary for them to peruse, from year to year, the very instructive Statistical Reports of the service, to fully understand how much the health of a crew depends upon the condition and sanitary arrangements of a ship.

In conclusion, I will only add that every step of practical progress in the hygiene of the Royal Navy is sure to react with prompt benefit on the health and general welfare of our Mercantile Navy,—which, of course, and rightly so, will always take as a standard for their guidance and example in such matters, the experience of our ships of war. And when it is considered that the number of merchant sailors employed in our foreign trade alone, exclusive of the large numbers on board coast steamers and sailing vessels, exceeds 200,000, and that often no small difficulty is found in obtaining the right sort of men for the service of this vast commerce, the magnitude and national importance of the subject are at once apparent. The best and the wisest, as it is the most humane, course is to use our utmost endeavours to prevent, by anticipative precautions, all unnecessary waste of available strength and power. That there is in our mercantile marine much preventible disablement and sickness—sickness too often termina-

ting fatally, either abroad in hospitals, or among our civil population in this country—is well known to every one whose attention has been drawn to the matter.* The hygienics, indeed, of our merchant seamen have not yet attracted that amount of public notice which their great importance demands. If something of the same system of accurate registration and record, which has for some years now been so beneficially carried out by the Board of Trade, in respect not only of the fabric and sea-worthiness of ships, and of the accommodation to be provided on board for crews and passengers, but also of meteorological, hydrographical, and other scientific observations, all for the good and safety of the service, were extended to the no less important topic of the Vital Statistics of the men employed, results of inestimable advantage could not fail to be obtained. This subject has, I am glad to say, been quite recently taken up by my distinguished friend Dr. McWilliam, Medical Officer of Her Majesty's Customs, in a paper read at the last annual meeting of the National Association for the promotion of Social Science; and it is much to be hoped that his

* "The sanitary and hygienic state of merchant shipping is often very faulty; and there is good reason to believe that there is at all times a large amount of sickness, damaged health, and premature disablement among the merchant seamen, which might be easily prevented by simple precautionary measures." * * * "The sanitary condition, too, of most seaport towns, and more especially of those parts near which the shipping is lying, is generally reported to be extremely unwholesome, and calculated, if not to engender, inevitably to aggravate many of the diseases against which quarantine is directed."—*Report on Quarantine, by the National Association, etc., printed by order of the House of Commons, August 1861.*

extended inquiries may eventually lead to the recognition, by the Government, of the extreme desirability of establishing a more full and accurate registration of the sickness and mortality among our merchant sailors than has hitherto existed.

I have the honour to remain,

SIR,

With great respect,

Your most obedient Servant,

GAVIN MILROY.

London: March 1862.

APPENDIX.

Unhealthiness of Sheerness and neighbourhood; influence on the Ships of War, Dockyard, &c.—Vide p. 30.

"Information showed, beyond question, that the population of the entire island of Sheppey is, to an enormous extent, afflicted with the effects of marsh-malaria, generated partly within the island, and partly on other adjacent mud-lands or marsh-lands about the Swale and the Medway; and that the diseases thus engendered occasion an immense amount of physical suffering, a very large interference with industry of all kinds, and a serious detriment to the efficiency of Her Majesty's service in that important locality."

Among the crews of the *Edinburgh*, and of the Steam Ordinary, averaging 1251 in number, 130 cases of malarial fever, 64 cases of rheumatism and neuralgia, and 81 cases of colic and diarrhoea occurred during 1857 and 1858; occasioning a loss of 2132 days service in these two years. A similar amount of the same sort of sickness prevailed among the crews of the *Waterloo* and *Formidable*, while at Sheerness, during part of 1858. The proportion of attacks of fever per thousand of the whole naval force was 106, and the average duration of each attack was very nearly nine days.

The Garrison Artillery, as might be expected, suffered still more than the population afloat. In 1856-57-58, no fewer than 344 cases of fever in an aggregate force, for the three years, of 2086 men. The average length of each attack was the same as in the Navy.

As to the workmen in Her Majesty's Dockyard (whose numbers averaged nearly 1700 during the three years), it appears that "of every 1000 men employed, more than a third part (perhaps more than half) suffer during the year from disorders which more or less decidedly are referred to that cause; viz., that 146 of them suffer fever, and that each of the 146 is thus kept from his work for (on an average) 9 4-5ths days; that 63 of them suffer neuralgia or other like pains, and that each of these 63 is thus kept from his work for (on an average) 10 3-5ths days; that 158 of them suffer from bowel-complaints, which are mainly attributed to the same sort of local influences, and that each of these 158 is thus kept from his work for (on an average) 5 days; besides that a very large amount of unrecorded illness and disqualification prevails among other members of the force."

With so much endemic sickness, it is not wonderful that Sheerness is said to be spoken of, at Chatham hospital, as "the African station of our home service."—*Report of the Medical Officer of the Privy Council, for 1859.*

Yellow Fever in Ships of War; effects of landing the sick.—Vide p. 55.

"The great and most important practical question arises, is it safe to permit the sick persons from such a vessel as the *Highflyer*, to be landed among a healthy community? This question was put to me, and, fortified in the opinion at which I had arrived by the results of many similar experiments in this place, I recommended that there should be no interruption to the *Highflyer's* people communicating with the shore; but intimated that it would be dangerous for strangers to be exposed to the atmosphere of the ship, so long as the disease continued in her. There has been no interruption to the communication of the *Highflyer's* officers and men with the shore; her sick have been in proximity with a considerable number of surgical cases from the other ships in the hospital, and with a large proportion of the crew of the *Persian*, affected with a malarious fever.

"No single instance of any kind of fever followed the landing of the *Highflyer's* sick, or the free intercourse of her officers, stewards, and people with the town, either in the hospital or in the town.

"I have witnessed and watched the progress of so many epidemics in this place, as to feel perfectly satisfied that any contagious powers the disease may possess in crowded ships, is speedily rendered inoperative by moderately good ventilation; and that the best mode of checking the progress of such diseases is to remove the sick to roomy quarters on shore as speedily as possible."—*Report of Mr. Watson, Medical Officer of Port Royal Naval Hospital, printed by order of the House of Commons, May 1853.*

Mr. Watson and Deputy Inspector Dr. Wingate Johnston were at Port Royal when I was in Jamaica, in 1851, and with both these gentlemen I had frequent opportunities of inspecting the ships of war, and the large naval hospital on the station.

In the dispatch of the Governor of Barbadoes to the Right Hon. Sir J. S. Pakington, Secretary of State for the Colonies (*printed by order of the House of Commons, March 1853*), we read:—

"In forwarding a copy of the report of the Health Officer,* on which the sick of the *Dauntless* were removed to the military hospital, it is very satisfactory to find that by observing the precaution recommended, the disorder was not in a single instance

* "The sanitary measure that I advise, is an immediate removal of the sick to the shore, taking proper precautions for an efficient segregation of them."

extended to the troops, affording, as it does, a strong confirmation of the soundness of the views by which the board have been guided."

Captain Halsted, in his letter to the Governor, mentions that "on the ship's first arrival, all communication with her was peremptorily stopped, an exclusion which still continues with regard to all but the *native* labourers and servants employed on board." The omission of this necessary precaution: viz., of employing only the coloured natives to go off from the shore on board a fever-smitten vessel in the tropics—occasioned, it is to be feared, the spreading of the disease to the unacclimated boat's crew of one of Her Majesty's ships, recently at Port Royal, with very disastrous results. The most regrettable point in the history of the *Dauntless* was the not landing the whole, or greater part, of the crew, the unattacked as well as the sick, as soon after arrival as possible. A large number of valuable lives might thus have been saved; as fresh cases, very many of which proved fatal eventually, continued to occur on board for several weeks afterwards.

Defective Ventilation in Ships of War.—Vide p. 55.

"In consequence of the very fatal outbreak of malignant cholera in the fleets at Varna in the preceding year, when general alarm was occasioned in the land and sea forces of the allies, and the expedition to the Crimea was thereby considerably delayed, the Lords of the Admiralty considered it advisable in the summer of 1855 to put the Commission in communication with the Admiral in command and with the principal medical officer of the Black Sea fleet, in the event of epidemic sickness again prevailing in the squadron."

"A minute inspection of the *Royal Albert* was made by Dr. Milroy, in company with Dr. Brien, the principal medical officer, and subsequently of the *Queen*, with Dr. Deas, Inspector-General of the fleet. Especial attention was paid to the arrangements on board ships of war, which are generally believed to have most influence in predisposing the crew to attacks of epidemic diseases, and in rendering these attacks formidable. The foremost of these is the amount of accommodation between decks for the men at night. In line-of-battle ships, the lower gun deck is reserved for this purpose; in two-deckers, the marines as well as the seamen sleep there; but in three-deckers, the marines and boys occupy the fore part of the middle deck. From 600 to 800 men, according to the strength of the crew, are thus usually berthed on the lower or main deck. By the relief of watches every four hours, there may never be more than one-half these numbers in their hammocks at one time; but, nevertheless, the whole have to sleep on that deck from night to morning. * * * When the ports are closed, the only means of admitting fresh air is by the hatchways, with or without the aid of windsails, so that the same openings serve the double purpose of entrance and escape.

"On board the *Royal Albert*, and in other screw line-of-battle ships, most of the officers have their cabins on the orlop deck, where all the midshipmen and mates also sleep. This arrangement, introduced of late years, must serve to render the atmosphere in the main deck still more impure; the heated breathed air from the orlop deck passes into it. The officers' cabins are apt to become quite stifling when the scuttles are shut, more especially when the furnaces are lighted.

"The accommodation for the sick in screw ships, as in the *Royal Albert*, is, in several respects, inferior to that on board sailing ships of the same class. The sick-bay is much smaller, occupying only one side of the upper deck forward instead of its entire breadth. It is not nearly so well ventilated, and does not admit of being so well ventilated. It is, moreover, exposed to contamination of atmosphere from the faulty arrangements of the water-closets, which have been adopted in the new ships.

"That the ventilation of the between-decks, and of the cabins, is capable of being greatly bettered by sufficiently simple means cannot be doubted. Still it is very questionable whether, by any means, it can be made so perfect as to render the air at night as pure as is desirable, or even safe in certain seasons, while the crew continue to be crowded together on one deck, and that deck the lower one. At least one-half of the best sleeping space in a ship of war is, in ordinary circumstances, left unoccupied. It is only made available when there is much sickness on board, and then it is used as part of the sick-bay. There appears to be no other reason for its non-occupation at other times, but the practice hitherto of the service. Free space and pure air are, however, as necessary for the prevention of disease, as for its mitigation and arrest.

"The changes in the accommodation for the well and sick, adopted of late in the new screw line-of-battle ships, have had the effect of diminishing the amount of space, without any concomitant improvement in the ventilation of the decks. This point requires the more notice, as the heat on board a steamer is of course greater than in an ordinary sailing ship, and the atmosphere is liable to become more oppressive. Many of the most fatal outbreaks of pestilence in the navy of recent years have occurred in steamers."

—From "Notes on Ships of War," by Dr. MILROY, in Report of the Sanitary Commission to the Army in the East.

From a letter which Dr. Deas wrote me from the Bosphorus to Balaklava in November, 1855, I extract the following passage:—"The want of ventilating tubes, whether fitted into the ship's sides or otherwise, is common to all the decks, and is, doubtless, the cause of some mischief. And insisting on the lower deck in two-decked and frigate-built ships, and the lower and middle in three-deckers, being the only decks for sleeping the whole of the crew is, I fear, the source of still greater evil. They clearly ought to be distributed over all the decks. * * * There is no attempt at a

ventilating tube, leading either to the upper deck or into the steam funnel, or anywhere else, from the officers' cabins (on the orlop deck); and these cabins must become most destructive to health, especially when the steam is up." Dr. Deas mentions, also, the want of proper accommodation for the reception of the wounded in action in screw liners, and of the bad state of the cock-pit generally in these vessels. The opinion of so experienced and able an officer is, of course, entitled to great weight.

Diet of the sailor in malarial climates.—Vide p. 59.

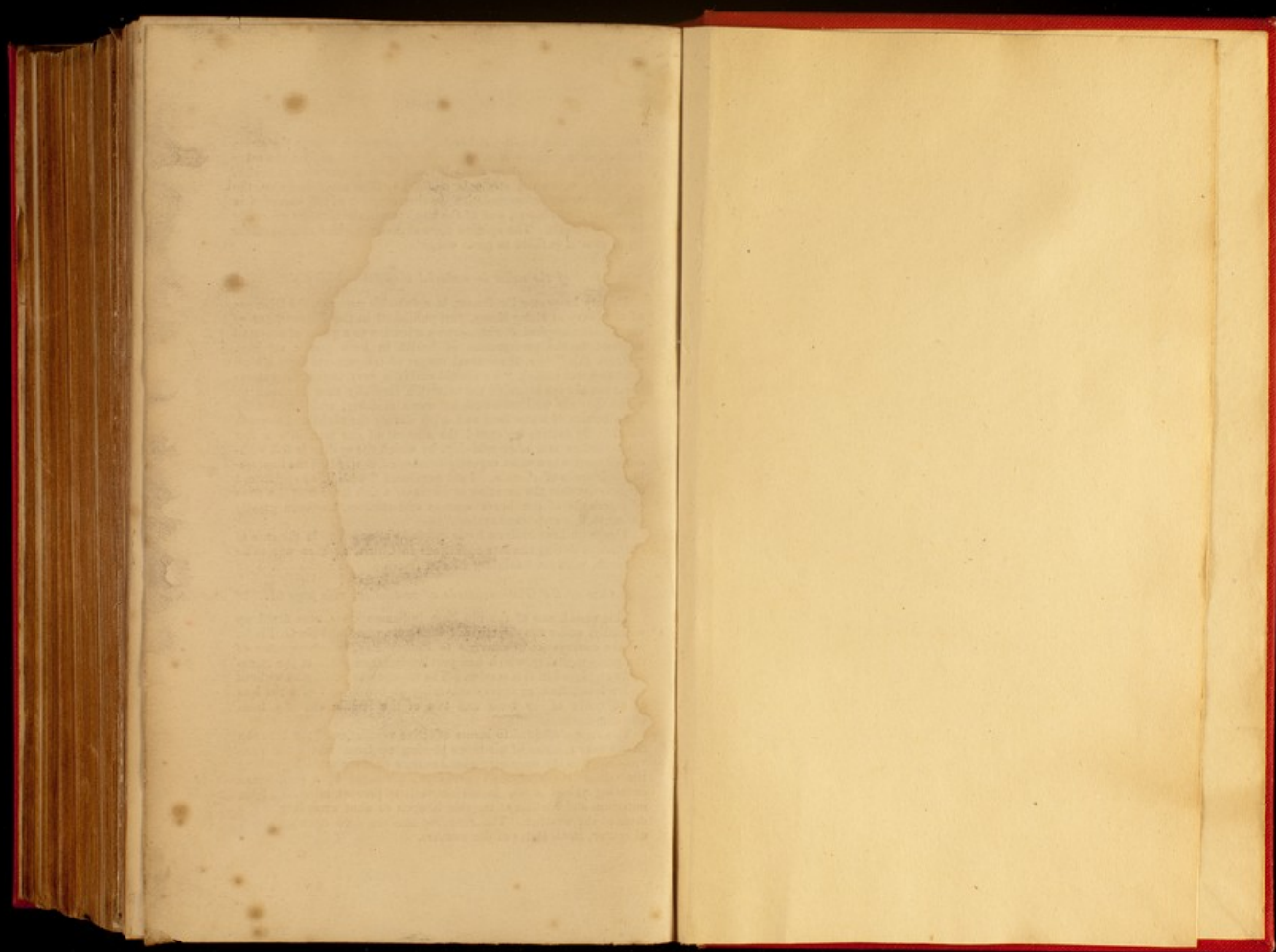
Deputy Inspector Dr. Smart, in a valuable paper on the Diseases of the Navy at Hong Kong, just published in the *Transactions of the Epidemiological Society*, draws attention to a point of no small moment in the preservation of health in both public services. "From this," (i. e. the diurnal ranges of temperature from a high heat to great chills), "we should derive a very important indication, not always attended to, to watch carefully that our men on night duty are well protected by warm clothing, and fortified by the stimulus of warm food and drink during the night and towards dawn. By routine we crowd the aliment of our men into a few hours before and after mid-day, by which the system is left without support when most urgently demanded to ward off the depressing influences of climate. I am convinced," adds this experienced observer, "that the invasion of diseases, which have cost the lives of hundreds of our brave seamen and soldiers, has been greatly owing to this erroneous system."

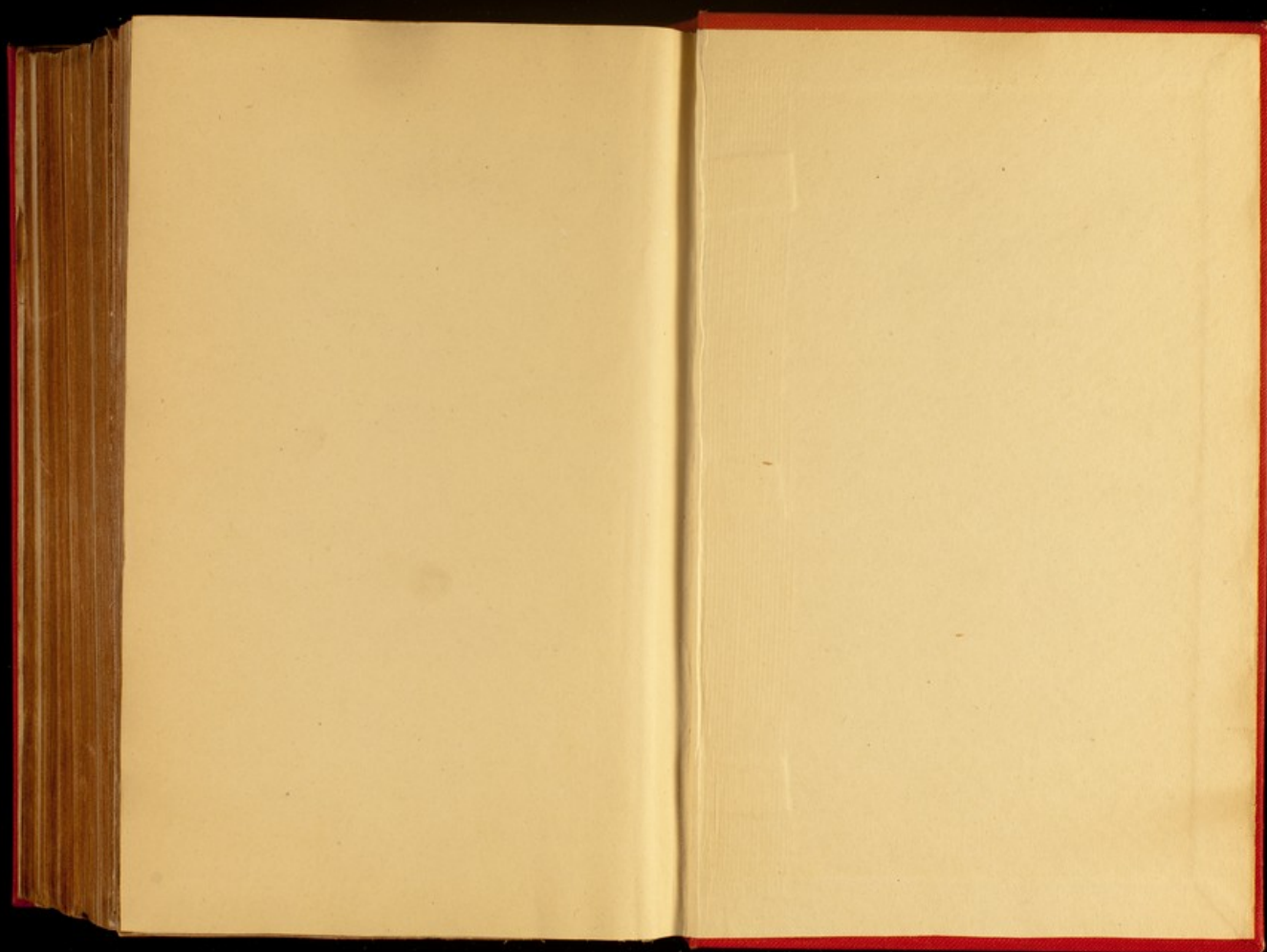
The point here indicated was carefully attended to in the case of our troops during the late operations in China, and, as was to be foreseen, with most salutary effects.

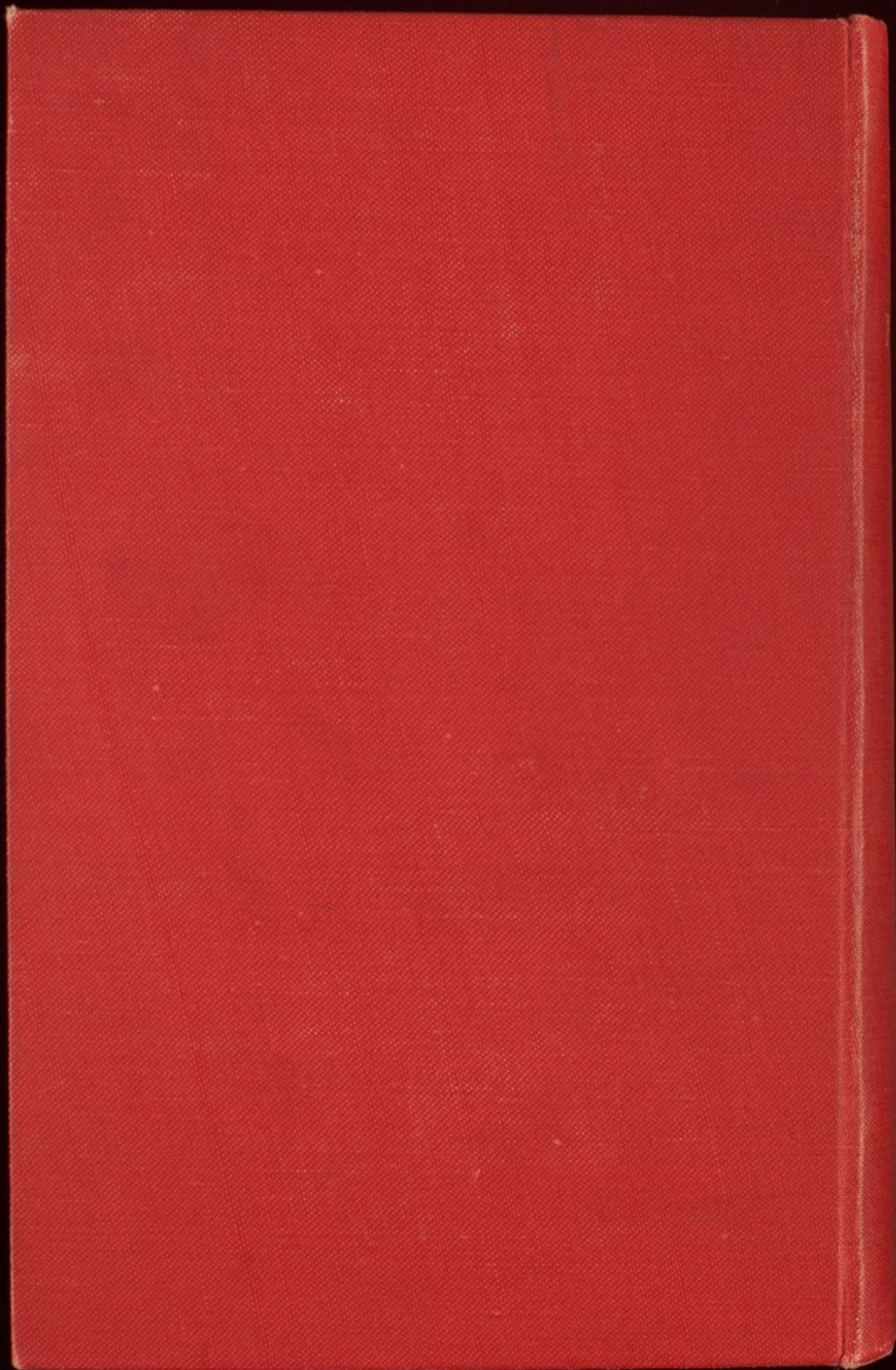
Case of the Glatton; mode of ventilation, vide page 61.

This vessel, one of the old East Indiamen class, was fitted up specially, under the direction of Count Rumford and Sir G. Blane, for the conveyance of convicts to Botany Bay, in consequence of the great mortality which had previously taken place in the transports employed in this service. The voyage was performed without either fever, flux, or scurvy occurring on board, and with the loss of only five of the male and two of the female convicts from chronic diseases.

The means adopted to insure effective ventilation of the between decks were a series of air-tubes passing up from where the convicts slept to the open air; also a narrow opening amidships along the whole length of the upper deck, protected by a pent-house covering raised a few inches above it to prevent rain, etc., from entering, and scuttles at the side to open or shut according to the state of the weather. The air-tubes and the deck opening acted, of course, in all states of the weather.







PAMPHLETS

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