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ON THE

TRANSMISSION OF DISEASES

BETWEEN

MAN AND THE LOWER ANIMALS.

BY

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FROM THE

Edinburgh Veterinary Review and Annals of Comparative Pathology.

JULY 1858.

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When, in 1854, I announced—as the result of a series of experiments to which I had been led by an inductive process of reasoning, founded on a study of cholera in man, of the diseases of the lower animals, and of the habits and structure of these animals—that I had succeeded in transmitting cholera artificially from man to the dog and cat, the statement was scoffed at as the "baseless fabric of a vision." Such a thing "had never been done," and "had never been heard of," and therefore could not be! The announcement, indeed, only met with the reception common to all innovations in science or art at the hands of the sceptical, the prejudiced, and the ignorant. I ventured further, in opposition to the dicta of "constituted authority," the general belief of the profession, and the "*vox populi*," to state my conviction that full investigation would not only bear out all the results arrived at by me, but would establish beyond a doubt, by numerous instances, the fact that certain diseases of the human subject are propagable in, or transmissible to, certain of the lower animals; and that diseases, so produced in the latter, might in them be studied with such effect and facility as to throw much light on certain obscure human diseases. I pointed out the important results likely to be attained by experimentation on the lower animals as to the transmissibility of disease both to and from man, and showed that this branch of comparative pathology will probably prove a necessary key to a correct knowledge of certain epidemic diseases of man. I propounded a full scheme of investigation, to be undertaken partly by professional, partly by non-professional persons, the details whereof may be found in the under-noted journals.[1]

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[1] 1. "Suggestions for Observations on the Influence of Cholera and other Epidemic Poisons on the Lower Animals," *Edinburgh Medical Journal*, July 1857. P. 33.

2. "Experiments on the Communicability of Cholera to the Lower Animals," *Edinburgh Med. and Surgical Journal*, April and Oct. 1854; *Gazette Hebdom. de Méd. et de Chirurgie*, Oct. 13, and Nov. 24, 1854.

3. "Natural Influence of Cholera on Plants and the Lower Animals," *Association Med. Journal*, Dec. 15, 1854.

4. "Facts for Contagionists, relative to the Artificial and Accidental Communicability of Cholera," *Ibid.*, Sept. 15, 1854.

5. "The Cattle Murrain in some of its aspects," *Lancet*, May 16, 1857. P. 496.

In answer, and in opposition, to my statements, it was argued, that the transmissibility of human diseases to the lower animals, being contrary to analogy, is beyond the bounds of probability; that cholera is essentially a human disease; and that, though cramps, purging, vomiting, and other symptoms might have existed in the animals which died under, and as the result of, my experiments, such symptoms were insufficient singly, or in the aggregate, to constitute *cholera*. Many, indeed, were the ingenious theories and objections raised to explain away the *facts* of my experiments. But it held good in this, as in every other, instance, that

> "Facts are chields that winna ding,
> And daurna be disputed."

After the lapse of four years, the facts remain unscathed by the storm of adverse criticism to which they were exposed. Nay, the views and results at which I arrived, in 1854, have been fully borne out by the history of comparative pathology subsequently to that period—chiefly by the ingenious and painstaking researches of various French and German experimentalists; and they are still daily being corroborated and supported. My observations and conclusions have now, therefore, stood the test of theoretical criticism and practical research for four years, and the facts I brought forward have now, I think, been tacitly admitted as established. The veteran physiologist and pathologist, Professor Alison, remarks, "That the cholera can be communicated to animals by inoculation with this matter of the peculiar rice-water stools, has been sufficiently proved by the experiments of Dr Lindsay in this country."[1] The worthy professor has, however, erred as to the mode in which, and the material wherewith, I succeeded in transmitting human cholera to the dog and cat; and, as the publication of this inadvertent error may have given rise, and may still give rise, to misapprehension, I think it right here to correct it. Among other modes and materials of experiment, I certainly used extensively the "rice-water" evacuations of human cholera patients. But though I thus succeeded in producing disease in the animals experimented upon, the disease was not the specific affection —cholera. The latter was produced by exposing the animals to the cutaneous exhalations from the bodies of cholera patients, as contained in fomites. And this, it appears to me, is a most important distinction to be borne in mind, as throwing light, or, perhaps, in some cases, darkness, on various theories that have from time to time been advanced of the essential nature or the *materies morbi* of cholera, and of the mode of its propagation. A reviewer in the *British and Foreign Medico-Chirurgical Review*, referring to my experiments, says: "It would seem probable that this gentleman has *solved the question of the contagion of cholera* by communicating the disease to animals."[2] And lastly, the editor of the *Gazette Heb-*

[1] "Application of Statistics to Questions in Medical Science," *Edinburgh Med. Journal*, Nov. 1855.

[2] July 1854: Review of Works on Cholera.

domadaire remarks:—"C'est par des récherches de ce genre qu'on pourrait espérer d'acquérir quelques notions vraiment neuves et elévées sur l'étiologie et la pathologie du choléra."[1] Had it not been that glanders and hydrophobia are familiar examples to the contrary, and beyond all disproof, there is little doubt that the same individuals or parties, who denounced the transmissibility of disease from man to the inferior animals as a doctrine absurd and illogical, and as a fact impossible and untrue, would also have denied the transmissibility of disease from the lower animals to man!

I did not advance theories unsupported by facts; nor did I denominate facts what were mere suppositions or speculations. I laid before the profession fully, and I trust frankly, the whole history of my experiments—their results and the conclusions to which they had led me—having neither bias nor prejudice, but actuated solely by the desire that the truth should prevail. Nor did I trust to my own experiments nor my own studies alone; I was too inexperienced as a comparative zoologist and pathologist to do this. I had the advantage of the opinion of experts in human pathology, who saw the appearances on dissection in the animals experimented upon, and who, I think, agreed with me unreservedly as to the interpretation to be put upon the symptoms and pathological appearances. And further, in order that I might not be led into error by mere resemblances, I requested the assistance of one of the most distinguished and accurate veterinarians and comparative pathologists then in Scotland—the late Professor Barlow of the Edinburgh Veterinary College—who expressed no doubt as to the conclusion to which we were shut up—from a consideration of the whole circumstances of the experiments conducted by me—viz., that the cause of the death of the animals experimented on was cholera, and that that cholera had been transmitted to them from the human subject. For a long period I stood alone and unsupported in my views on the transmission of disease between man and the lower animals—views which, since the date of my original experiments, I have repeatedly brought before the public in various ways, in order to stimulate a spirit of inquiry. Gradually opposition has awakened investigation; sympathy has been evinced, and co-operation secured. The cause has been not a little strengthened by the assistance of such men as Dr Richardson, the accomplished editor of the *Sanitary Review*, and one of the foremost of our living experimental physiologists, who has recently laid before the Epidemiological Society of London, a most suggestive paper "On the Investigation of Epidemics by Experiment."[2] A certain amount of impetus has been given to the study of this, and of kindred subjects, by the institution, some years ago, of the *Sanitary Review*, originally the *Quarterly Journal of Public Health*, the prospectus

[1] In a Note introductory to a paper by me, entitled "Transmission du Choléra aux Animaux; nouveaux détails," Novem. 24, 1854. P. 1044.

[2] Read March 1, 1858: Abstract given in *Medical Times*, Mar. 20, 1858. P. 805.

of which contains the following very catholic announcement:—"As there is evidently a connexion between certain unhealthy conditions of the human family and unhealthy conditions in inferior animals and plants, care will be taken to investigate and record the spreading diseases of these last-named divisions of the living creation, and to point out the best measures which science may suggest for the prevention or removal of such diseases." And a still further impetus, I trust, will now be given by the establishment of the *Edinburgh Veterinary Review and Annals of Comparative Pathology.* Such encouragement is very opportune; for even yet there are few bold enough to advocate the doctrines I have from time to time enunciated regarding the relations of disease in the various tribes of animals. The study of the habits and structure of the lower animals is too much left to the zoologist or comparative anatomist; comparative pathology is handed over almost exclusively to the province of the veterinarian; the placing of veterinary medicine at all on a par with human medicine is regarded as unjustifiable presumption; and the diseases of the lower animals are conceived to be peculiar and not comparable with those of man. It is full time that all such absurd ideas were exploded, and replaced by views more in the spirit of modern philosophy and enlightenment—more consonant with the achievements of modern science—more worthy of the age we live in. Now, this can only be done, I think, by members of the medical and veterinary professions zealously co-operating toward one great end—the discovery of truth and the promotion of science. It has ever, probably, been granted, that veterinary science is deeply indebted to medical science—to human anatomy and surgery, our materia medica, &c.; but it has yet, in great measure, to be proved—though I have no doubt of the fact, as I shall immediately attempt to show—that human medicine may become largely indebted to veterinary medicine. Undoubtedly, the studies of the diseases of man and the lower animals borrow and reflect light mutually on each other. I am bound in justice to avow, however, that veterinary practitioners are not, generally speaking, sufficiently scientific; they have not hitherto, as a body, in this country, done that service to human medicine—to comparative pathology—for which their opportunities so well qualify them; and the main object of the present remarks is to show veterinarians how they may so avail themselves of their opportunities as to render true service to science. My paper, however, is more suggestive than informational; and I propose, therefore, only to give a few illustrations to indicate the kind of harvest that may be gleaned by willing and qualified labourers. For details, as well as for bibliographical notes, which may assist the student, I beg to refer to my various publications in comparative pathology, which will be found enumerated at the foot of page 3 of this Brochure. These publications contain, *inter alia*, 1. Notes of the best authenticated instances—selected from Foreign and British journals—of cholera-epizootics in different parts of the world; 2. Illustrations of the transmission of cholera from

man to the inferior animals; and 3. Illustrations of the influence of a cholera-atmosphere on animal life in different countries.

The discussion stirred up by my views, arrived at inductively, and by the results of experiments to which these views led, has induced me to study, during the last four years, the whole subject of the relations of diseases of the lower animals to those of man. Though the points which at first mainly arrested my attention were, 1. The transmission of human diseases to the lower animals; and, 2. The communication of the diseases of those animals to man, with the laws that regulate such transmission; yet the subject gradually expanded itself into a vast field of inquiry or investigation, embracing a great variety of departments of research—each apparently distinct, yet all more or less intimately related. As a result of my inquiry, I am prepared to lay down the following fundamental propositions; and I select this mode primarily of explaining my views, in order that I may place the subject of the relation of disease in man and the inferior animals, in its chief bearings, clearly before my readers:—

1. That certain human diseases occur, under certain modifications—which are, however, often comparatively insignificant—in various of the lower animals, both wild and domesticated, Such diseases are cholera, yellow-fever, typhus and typhoid fevers, plague, small-pox, measles, croup, pleuro-pneumonia, influenza.

2. That certain "epidemic constitutions" of the atmosphere affect equally, or, at least, in marked degree, man and the lower animals; or, in other words, that a relation frequently subsists between coincident epidemics and epizootics, or epizootics preceding or following epidemics and such epidemics.

3. That certain human diseases are transmissible, by contagion or otherwise, to various of the lower animals.

4. That certain diseases of the lower animals are transmissible to different individuals, species, and genera of these animals.

5. That certain diseases of the lower animals are transmissible, by contagion or otherwise, to man.

6. That certain diseases, communicated to man from the lower animals, are propagable from man to man in the same manner as ordinary contagious human diseases are.

7. That there are certain modifications in type, character, and degree in transmitted diseases, depending on the habits and structure of the animals affected; and that such modifications must be carefully studied, in order to a correct appreciation of comparative pathology.

As subsidiary or collateral propositions, I would further state my conviction:—

8. That the study of the diseases of the lower animals is calculated to throw much light on our knowledge of various obscure human diseases, and specially such as possess an epidemic character.

9. That the same study has an important bearing on the health of our flocks and herds, the integrity of our agricultural interests at

home and in the colonies, and on the use of the flesh of the domestic animals as food for man.

10. That the pathology, especially, of the diseases of the lower animals, requires an extended and more accurate study in this country.

11. That experimentation on the lower animals, as to the transmissibility of diseases to or from them and man, is one of the most promising means of arriving at sound conclusions as to the nature of various wide-spread diseases, both in them and in man.

12. That, with a view to the proper conducting of experiments, it is necessary to make a selection of the animals to be operated upon, according to the nature of such experiments, or of the diseases to be studied.

I have illustrated, as fully as the literature of the various subjects will allow, most, or all, of the above propositions in my published papers already referred to: I shall not, therefore, occupy space by going over trodden ground. The few additional citations I have to offer here are chiefly such as have been published subsequently to the date of my last paper.[1] Few though they be in amount, and insignificant in value, they may still stimulate some active and liberal minds to inquiry; and should they do so, the aim of my present remarks shall have been fully gained.

Proposition 1. Dr M'Donald describes a "virulent attack of cholera amongst pigs and cattle," in the neighbourhood of Glasgow, in the spring of 1857.[2] He mentions one of the affected pigs as "lying on its side in a state of collapse, eyes dull, ears hanging back, vagina hanging out, pale and flabby. . . . The belly was of a beautiful pink colour, shaded with blue; on some parts of the body the blue colour approached to a black. . . . At intervals it was much cramped, and the soft fæces were jerked to a little distance. It had also vomited, and its mouth was filled with a grumous fluid." It died after six hours' illness. The *post-mortem* appearances were, a flaccidity of body; surface a dark blue, with a pinkish tint on belly; stomach moderately distended with half-digested food, surface studded with patches of inflammation, varying in size from that of a shilling to that of a half-crown; intervals natural in colour; small intestines, at intervals of about eight to twelve inches, inflamed, the length of the inflamed patches being about one inch; large intestines healthy; liver and spleen natural; heart natural. The lungs alone presented features which specially arrested the eye: the surface, and every part cut into, had an intense scarlet colour. One gentleman lost forty pigs from this disease within a short time. At the onset of the epidemic, the disease had the characters of influenza or intestinal fever; there were present prostration, constipation, and loss of power in the limbs, and the animals lingered for a long time. Latterly the disease became more

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[1] *Lancet*, May 16, 1857. P. 496. *Ol. citat.*

[2] "Cholera amongst Cattle." By Wm. M'Donald, M.R.C.S., Medical Officer for Cadder Parish, Chryston, near Glasgow. *Lancet*, June 20, 1857.

acute in its character and short in its duration, the animals dying after a few hours' illness. With the supposed causes and the suggested remedy for this disease, as well as with its bearings on the quality of our food-supply, we have, under this proposition, strictly nothing to do. But I shall nevertheless venture to add, in regard to the character of the homes of the affected pigs and cattle, that "the long ranges of building are badly ventilated and not drained. I felt suffocated," says Dr M'Donald, "while walking through them, and there was a depth of six inches of putrid water under the plank on which I walked." He also mentions that he saw two cattle dead of choleroid disease, marked by cramp, after a few hours' illness; and he winds up by asserting—greatly to the comfort of the Glasgow public, no doubt—that the meat of all animals, dying as above described, finds its way regularly into market!

It would appear, from the researches chiefly of Continental pathologists and veterinarians, that that heterogeneous disease known under the general or collective term of Murrain, is chiefly one or other of cholera, contagious typhus, bronchitis, or pleuro-pneumonia. Dr Radcliffe of Bromley, near Leeds, says, that a murrain which he saw in Asia Minor, while serving under Omer Pacha in the autumn and winter of 1855, "was characterized by an intestinal affection. It was a choleraic murrain, and it was regarded by the natives as being intensely contagious." This disease was very fatal. The same gentleman describes a pulmonary murrain or disease of cattle, as common in Yorkshire and other parts of England—which is also very deadly and contagious—the pathological appearances wherein are congested and friable lungs, and distension of veins at the root of the heart, with fibrinous clots.[1] The cattle murrain in Germany "is thought to be a typhoid fever, spreading perhaps by contagion, perhaps by atmospherical causes; and in fact more resembling the march of cholera over the world, than an ordinary epidemic engendered by local causes, and propagated by contagion."[2] The *Times'* Vienna correspondent, again, says that there are three forms of the "Vieh-Seuche," or cattle-plague of the continent: 1. A catarrhal affection of lungs; 2. A pulmonary complaint, with typhoid symptoms; and 3. A highly contagious typhus; the last being the real cattle-plague—the "Löser-Dürre"—of Austria. Mr Jos. S. Gamgee has also distinctly stated that the cattle-murrain of Moravia and Poland, the importation of which was so much dreaded in this country in the beginning of last year, is contagious typhus,—which opinion is also that of the most distinguished veterinary pathologists on the continent, including Professor Spinola in Poland, Professor Bochdalek in Bohemia, and MM. Rénault, Yvart, and Imlin, who were members of a commission instituted by the French Emperor, with a view to investigate the murrain question. The chief pathological lesion is ulceration of the

[1] Letter on subject of "Cattle Murrain," in *Times*, April 2, 1857.
[2] *Spectator*, April 1857.

intestinal glands, the lungs being mostly exempt from disease. The same gentleman shows that it is rather the pleuro-pneumonia of cattle which we have to dread in this country—a disease which has repeatedly committed sad ravages among our flocks and herds, and which would appear to be always more or less prevalent in our midst.[1]

There are now many instances on record of cutaneous eruptions, either identical with, or similar to, those of man, occurring in the lower animals.[2] Alibert has found *Herpes circinatus* in horses.[3] Additional illustrations under this proposition are the occurrence of croup, small-pox, measles, and plague, in the pig, as mentioned by Dr Richardson; of cholera in fowls in the Crimea, by Dr Blenkins; of cholera in cattle, pigs, the horse, dog, cat, racoon, camel, zebra, monkey, &c.: of typhoid fever in hares about Paris; of small-pox in the monkey; of influenza in the lower animals in Jamaica, as mentioned by Dr Milroy; of the grouse disease; and of the hog distemper in America. Many of these illustrations refer equally to the second proposition. Special exception would appear to be taken to cholera as a disease of the lower animals, by those who maintain that it is essentially human. But if animals are subject to such diseases as typhus and typhoid fevers, pleuro-pneumonia, croup, variola and rubeola, what *à priori* reasoning can there be hostile to the idea, that they may be also subject to cholera? The above diseases have been attended by the same symptoms and the same pathological appearances; have even, in many cases at least, apparently been produced by the same causes as in man; and the evidence on which their occurrence, in a variety of the lower animals, has been established, is abundantly trustworthy.

Prop. 2 is sufficiently proved by the published histories of epizootics in this and other countries; by the admirable Report to the Faculty of Medicine of Vienna, on the influence of the epidemic constitution of 1832 on the lower animals—including all tribes thereof—in Austria; and by Dr La Roche's account of the yellow fever of 1853 in New Orleans,—where also all kinds of animals were affected.[4]

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[1] Letter in *Times* on subject of "Cattle Murrain," April 3, 1857.

[2] Instances will be found in:—

Hering's *Repertorium der Thier-Heilkunde.* Band. 1, 1840.

Gurlt and Hertwig's *Magazin für die Gesammte Thier-Heilkunde.* Band. 7, 1841.

Letteneur, *Réflexions sur l'Herpes Tonsurant*, 1852.

[3] "On the Existence of Herpes in Domestic Animals and its Communication to Man." By Dr Von Bärensprung. *Annalen des Charité Krankenhauses.* 8. *Jahrgang Heft.* 1: or quoted in *Brit. and For. Medico-Chirurg. Review*, July 1857. P. 263.

[4] For the information of the student I beg to offer a few bibliographical citations on Epizootics:

Census of Ireland for 1851, vol. v. p. 358-9. There is here given an analysis of Epizootics from 1837, in which all tribes and kinds of animals seem to have been affected.—Quoted in Letter by Dr Radcliffe, in *Times*, April 7, 1857.

It does not follow, if we may judge from analogy, that every atmospheric poison which is deleterious to man should be equally so to all or any of the lower animals. This is not likely, when we consider that many mineral and vegetable substances, which are poisonous to the human subject, are altogether, or comparatively, inoperative in certain of the lower animals. I found, several years ago, while experimenting in the lower animals on the antidotes to certain poisons, that the dog is extremely insusceptible of the poisonous action of opium, while it is peculiarly susceptible of that of strychnia. I have given twenty grains of muriate of morphia, and large quantities of opium and belladonna, to dogs, without poisonous effects.[1] Such results demonstrate the inutility or fallacy of employing the dog in experiments on these and certain other poisons; or, in other words, it shows the necessity of selecting the animal according to the nature of the particular experiment, or series of experiments. Other poisons, again, are inoperative, or nearly so, in the rabbit. We know that some animals eat with impunity plants, such as *Œnanthe crocata*, which are poisonous to man; and we know, further, that what is poisonous to one animal is not necessarily so to another.

Prop. 3. My transmission of human cholera to the dog and cat is an instance in point. Experiments of a similar kind, and with similar results, are detailed by Marshall of London, Thiersch and Meyer in Germany, Namias of Venice, Bassani, Feschi, Novati, Calderini, Semmola, and others.[2] Most of these experimenters have

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Ramazzini, *Constit. epid. Hassiac*, ann. 1691; *Hist. Feb. Catarrh*, ann. 1730; *Tract. de Contagione Plenciz: Sager Libellus de Aphthis Pecorinis*, ann. 1764; *Ens. Disquisitio Anat. Pathol. de Morbo Boum*; also *Schrveck, Herder, Valentinus, Gohrliep, Bechrens, Rayger, Stegman, Schelhamer, Hoyer, Grobezius*, in *De Re Rustica*, vol. ii. p. 182. Layard's *Essay on ye Nature, Causes, and Cure of the Contagious Dist. among ye Horned Cattle*, 8vo. 1757; His *Letter* also in *Phil. Trans.* vol. lxx., pt. ii., p. 536; *Dossie Mem. of Av.*, vol. ii. p. 364.—Quoted in Letter by W. St Croix, in *Times*, April 7, 1857.

Prof. Sauvage's *Account of the Epizootic of* 1730 *and* 1731 *in France.* Quoted in *Illustrated Times*, April 11, 1857.

Reports on the relation of Food and Disease. No. I.: "On the Connexions and Relations of Epidemic Diseases in Man and the Lower Animals," *Med. Times*, April 18, 1857.

Letter on the "Rinder-pest" or Steppe Murrain, from Dr Greenhow, Lecturer on Public Health at St Thomas' Hospital, to the President of the Board of Health, dated 11th April 1857.

Kreutzer's *Gesammte Veterinär. Medizin für Ärzte.*

Hering's *Pathologie und Therapie für Thierärzte.*

Spinola, *Handbuch der Speciellen Pathologie und Therapie für Thierärzte.*

Lancisi, *Dissertatio Historica de bovilla peste a* 1711 *et* 1768.

Ramazzini, *De Contagione Boum Epidemica.* Ed. Lond. 1717.

Haupt, *Mag. für die Gesammte Thierheilkunde*, 1854.

Renault, *Recueil de Médecine Véterinaire*, 1855.

[1] "On the Non-Susceptibility of the Dog to the Action of certain Poisons," in a paper on "Belladonna," *Assoc. Med. Journal*, June 9, 1854. P. 509.

[2] See an admirable *resumé* of these experiments in a Paper by Mr Marshall in *Brit. and For. Medico-Chirurgical Review*, April 1853.

used either the blood or evacuations of cholera patients, which they have either introduced into the alimentary canal of the animals, or inoculated in the cellular tissue of different parts of the body. The symptoms and *post-mortem* appearances, in many cases, were identical with those of human cholera; in others very similar. Marshall states that Meyer, "with whom," he says, "our own observations would lead us so far to coincide," found that cholera stools given to dogs produce certain symptoms, which "are sometimes followed by death with asphyxia, and with *post-mortem* appearances very much like those observed in the cold stage of cholera among men." Meyer also describes the case of a dog, "in which the symptoms and *post-mortem* appearances were highly characteristic of true cholera"—the disease having been produced by its licking the evacuations of cholera patients. In reference to the presence of a *peculiar* or specific poison in cholera evacuations, Meyer, "perhaps too sanguinely," says Marshall, "maintains, that from the entire evidence this is probably the case."

"In some experiments I have made," says Dr Fletcher, "I have transmitted the human *small-pox* through the horse to the cow, and so to the child in the form of cow-pox."[1]

Prop. 4 has been proved by experiments and observations made by the professors of the French Veterinary Schools of Alfort and Lyons—schools which have gained and deserve a proud pre-eminence by and for their achievements in veterinary pathology.

Prop. 5. Various memoirs have of late years been published in Germany, and especially France, to prove the transmission of the *Herpes* of animals to man. One of the most recent of these, is the Report by M. Devergie to the Academy of Medicine of Paris, in regard to *Herpes tonsurans.* It has long been *suspected* that transmissibility by contagion existed; but it has been *proved* only recently. He seems to proceed on the experiments and observations of M. Reynal, which go to prove that the Herpes of horses and oxen, at least, is transmissible to man. M. Devergie believes that the *Herpes tonsurans* of the lower animals may give rise either to the same form of *Herpes* in man, or to the *Herpes circinatus;* and he ascribes both these species of herpes to the growth of a parasitic fungus—the Tricophyton.[2] Dr Von Bärensprung essentially agrees with M. Devergie. He thinks that the herpes of animals resembles most closely, if it is not, the *Herpes tonsurans* of man; and he ascribes it to a *confervoid (?)* growth. He and other observers probably use the term *confervoid* in a popular sense, and withal an erroneous one. The *confervæ* belong to the *algæ*—salt or fresh water plants mostly; while growths of this character are always *fungoid.* He rubbed some of the scales of this herpes from one of the lower

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[1] "Letter on Vaccination," by J. S. Fletcher, Surgeon to the Bromsgrove Union House and District. *Lancet*, Oct. 31, 1857.

[2] Quoted in *Medical Times*, Feb. 13, 1858. P. 176.

animals, containing necessarily abundance of the sporules and mycelium of this fungus—Tricophyton, or by whatever other name we may call it—on his left fore-arm. No effect was produced for several days; but after a longer interval considerable itching caused his attention to be directed to the spot, which he found occupied by a well-formed patch of *Herpes circinatus*, about the size of a sixpence. This patch increased in three weeks to the extent of the size of a crown-piece.[1] Again, Dr Fehr has noticed, in Switzerland, a peculiar herpetic eruption in certain of the lower animals, which is communicable to man. Similar observations have frequently been made on the Continent in regard to itch.[2] A further illustration is the disease in France called *Anders*, which appears recently to have been described for the first time. M. Lemaistre gives an account of it as occurring among horned cattle, in the provinces of Limousin and Auvergne; in the former province it goes under the name of *endai*, and in the latter under that of *anders*. It is transmissible to man; but the characters of the disease are not quite the same in man as in animals. This is what we should *à priori* be prepared to expect. In cattle, it is a cutaneous eruption, beginning as pimples, which discharge a reddish-yellow fluid, and harden ultimately into crusts or scabs like Impetigo. These pimples grow in the form of ring-like patches. It is popularly believed to be contagious among cattle. In man, it is more local and circumscribed than in animals, being confined chiefly to the face, fore-arms, and back of hands. In some cases the disease in man was traced to contact with affected animals; while in other cases it appeared to have been communicated from man to man.[3]

The latter circumstance, which does not appear to be clearly proved, illustrates also Proposition 6, which, it must be confessed, is as yet but meagerly supported by substantial evidence. It is note-worthy, that in the cases of herpes, scabies, and anders, the material or medium of communicating the disease is something tangible and visible—something more substantial and satisfactory than an undefined volatile poison, as in cholera. In herpes we have the sporules of a fungus, in scabies the ova of an insect, and in anders, probably, as in glanders and hydrophobia, contact with diseased secretions or excretions—the pus of the pimples or pustules, which, in a sense, constitute the disease. This class of cases is, therefore, distinctly separable from such affections as cholera, where the germ of the disease is intangible, invisible, inappreciable, except in its effects.

Prop. 7 is in a great measure self-evident; and I have in former

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[1] *Brit. and For. Medico-Chir. Review*, July 1857. P. 263. *Ol. cit.*

[2] *On the Animalcules of the Itch of Man and Animals, and on the Transmission of Itch from Animals to Man*, by M. Bourguignon and Delafond, laid before the Academy of Medicine in Paris, in Nov. or Decem. 1857.

[3] "Anders—a Disease communicable from the Bovine Species to Man, and probably from one Human Being to another," by M. Lemaistre of Limoges. *L'Union Médicale*, 26th Jan. 1858; and *Brit. and For. Medico-Chir. Review*, April 1858. P. 536.

publications sufficiently illustrated it. When we consider to what an extent in man the same disease varies in individuals according to circumstances of idiosyncrasy and habit, sex and age, &c., we shall cease to be surprised that the same disease should present many and varied modifications in animals, differing so much from man, and from each other, as cattle, horses, pigs, dogs, rabbits, fowls, and fishes.

Prop. 8 remains, in great measure, to be proved. The light to which the proposition refers is as yet only glimmering in the far distance; still it is a distinct glimmering, and it is for the veterinarians of our age to enlarge this glimmer into a brilliant light. It seems to me that the veterinary pathologists of this country are, as a body, behind their French and German confrères; and that, to place themselves on an equal footing, veterinary science must be studied more philosophically in this country than hitherto. It is a great pity that the veterinary practitioner should, in too many cases, degenerate into the mere horse or cow doctor, whose ignorance leads him to be frequently regarded as a quack, or an "old wife." Why should he, with all the facilities of modern education, not be as fully informed of chemistry, histology, pathology, zoology, and botany, as his brethren of the medical profession? Why should he throw away his glorious opportunities of contributing, in his own way, and in his own sphere, to the progress of knowledge—to the advancement of his science and art? Never, in the history of veterinary medicine, in this or any other country, was the young veterinary student in a more favourable position for entering upon the philosophic study of the etiology, symptomatology, and pathology of the diseases of the lower animals. Veterinary colleges abound, whose chairs are occupied by distinguished professors; there are ample opportunities for the study of all the sciences bearing upon veterinary medicine; there are not a few magazines devoted to veterinary science; and the scientific veterinarian would here unquestionably secure the sympathy and co-operation of the medical profession. The veterinary student should aim at higher education; his talents cannot be too highly cultivated. It is quite a mistake to suppose that any sort of education is good enough for a "horse doctor!" Undoubtedly, the student who, in addition to a knowledge of veterinary science, possesses some acquirements in human medicine, will be better qualified for advancing his profession than his less accomplished companion. But let not these remarks dishearten nor discourage any observer or student, however humble his qualifications. I have taken pains elsewhere to show that all persons concerned in any way with the management or use of the lower animals may contribute towards the elucidation and proof of such propositions as I have stated above. The essential qualifications of the observer are shrewd common sense, patience and perseverance, and a willingness to labour in behalf of science.

Prop. 9 is, again, in great measure, self-evident. Its bearings are amply illustrated by the recent efforts of Messrs Gamgee, Gant,

and others, in regard to the better supplying of our markets with healthy butcher meat. These gentlemen have shown that the flesh of animals, which have died of all manner of diseases, finds its way regularly into our markets—partly, no doubt, from ignorance of veterinary pathology on the part of the inspectors of these markets; and Mr Gant has pointed out how fatty degeneration, *inter alia*, results from the absurd prize fattening system.

Prop. 10. The department of pathology is, perhaps, that in which our veterinary practitioners are most deficient. The records of many epizootics and rare sporadic diseases have frequently been rendered worthless by the absence of pathological details. The Registrar-General of Deaths, &c., in a quarterly report about the beginning of 1857, remarks: "The pathology of domestic animals is exceedingly imperfect; their diseases are badly characterised; and the effect of epizootic causes on the human race is little understood."[1] This is a most true and most suggestive sentence. There are few more important studies to the young veterinarian than this; few likely to be more pregnant of advantage to science; few more certain to gather laurels for himself.

Prop. 11. I am daily more and more convinced of the important results likely to accrue from experimentation on the lower animals, in regard to our knowledge of such diseases as cholera, syphilis, small-pox, erysipelas, dysentery, &c.; and it is not a little encouraging to find my views shared in by Dr Richardson of London, who has recently earned for himself an early, but solid and enviable fame, by his admirable series of experiments on the "Cause of the Coagulation of the Blood." In his paper at the Epidemiological Society, he specially recommends experiments in scarlatina, small-pox, and typhus; he narrates experiments of his own, in which he succeeded in producing all the characters of typhus by introducing alkalies into the blood; and he shows also that a typhus state may be produced by the injection into the system of putrid animal matters. He deplores, as deeply as I do, the present unsatisfactory and unscientific character of the study of epidemiology; and he believes experimentation on the lower animals to be one of the most likely means of improving our knowledge of epidemics. He suggests, as subjects for investigation, the following points, which he puts in the form of propositions:—

1. "That, by experiment, it might be ascertained in what excreta the poisons of certain of the epidemic diseases are located."

2. "By what surfaces of the body such poisons may be absorbed, so as to produce their specific effects."

3. "Whether the virus of disease, in reproducing its disease in a healthy body, acts in the development of the phenomena by which the disease is typified, primarily or secondarily, *i. e.*, by its own re-

[1] Quoted in a Letter from Mr Joseph S. Gamgee on "Quarantine and the Contagious Typhus of Cattle," in *Lancet*, May 16, 1857.

production and presence, or by the evolution of another principle or product."

4. "Whether climate, season, or other external influences, modify the course of epidemics, by producing modification of the epidemic poisons, or modifications in the system of persons exposed to the poisons."[1]

Every one versant either with human or veterinary medicine is fully aware of the importance of experimentation on the lower animals in questions of physiology or toxicology; but it has not been resorted to, to the extent that is desirable, in questions of comparative pathology, or of epidemiology.

Prop. 12 is only consonant with our knowledge of the variations in structure and habits of the lower animals, and of the phenomena of disease as modified by such variations. Dr Richardson remarks, the pig is specially suitable in experiments on small-pox, scarlatina, and typhus, inasmuch as it appears to be more susceptible of the influence of these diseases than any other of the lower animals. Again, I selected the dog and cat in my experiments, as having a structure and habits closely resembling those of man; while other experimentalists have chosen the rabbit, mice, fowls, &c.

In conclusion, I would earnestly call upon veterinary practitioners throughout Scotland to investigate, so far as lies in their power, some of the propositions given above, or any of the collateral subjects or points which they may suggest; and further, to avail themselves of the opportunity which the *Edinburgh Veterinary Review* affords of recording their observations, however isolated and apparently insignificant. I think I can venture to assure the reader of the favourable reception of all reliable observations in comparative pathology by the editor of this journal, whose efforts to place veterinary science more on a par with human medicine deserve to be crowned with success. The reader will find a vast field of inquiry embraced in the propositions I have laid down, and in my various papers on comparative pathology: he may select for himself a path of research suitable to his tastes or opportunities. Due advantage has certainly not, I think, been taken of that section of the *Sanitary Review* which is devoted to a record of epizootics and their relation to epidemics. But let us hope for better things now, when both the medical and veterinary professions have journals opening their pages for the reception of this species of information, and inviting, nay urging, contributors to avail themselves thereof!

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[1] *Medical Times*, Mar. 20, 1858. P. 306. *Ol. cit.*

PRINTED BY ANDREW JACK, CLYDE STREET, EDINBURGH.

Die Magenerweichung der Säuglinge.

Von

Dr. C. L. Elsässer.

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