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SPECIAL COMMUNICATION

Chapman's reflexes: Medicine or metaphysics?

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Clinical medicine is a pragmatic discipline. Because of its aim to alleviate pain and suffering as well as to foster well-being, its practitioners can be lulled into thinking that there are times when basic science is irrelevant to the practice of medicine. This attitude is evaluated by comparing a 19th century medical curiosity, the nasogenital reflex, to a set of reflexes, the little used and little known Chapman's reflexes. We must always attempt to substantiate the principles of clinical medicine.

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It is only in the past few years that we have come to understand how aspirin works. Yet, we have been using aspirin successfully ever since its introduction to the pharmacopeia about 90 years ago. The treatment of frostbite changes periodically from immediate heat to immediate cold and back again to immediate application of heat. Yet, at any given time, we seemed reasonably sure that we were doing what was best for those with frostbite. There is a current controversy about the merits of antibiotics for otitis media. What is one to make of all this? Is there any way to know when one is right?

The purpose of this paper is to make some suggestions related to these questions. I will look at the history of medicine and science in general to draw some conclusions about the attitudes physicians ought to take toward what they do and what they believe is true. In a way, nothing suggested will be astounding. It will likely sound commonplace, but sometimes the commonplace ought to be brought to light and supported with argument. All I want to suggest is that physicians and scientists should make every effort to find out what really works and why.

I begin with a brief look at an episode from the history of medicine.

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Reflex theory in general

In the mid- to late-19th century, science and medicine made great strides. Medicine increased its prestige in two related ways. First, it started to get some results, and second, it became scientific, merging (*piggybacking*) is a better word) in a way with biochemistry, physiology, and microbiology. The last word in science in those days was physics, especially Maxwell's equations, which linked electrodynamics and magnetism to just about everything else. None of this was lost on the medical profession, which invented the subspecialty of neurology in the belief that what was good enough to run the world was probably good enough to run organisms with nervous systems.

Technically, this view needed some evolutionary assumptions. After all, if there were no convincing evidence of an evolutionary link between all organisms, the inference that all organisms with nervous systems run on the same principle would be nothing more than

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a hasty generalization. Luckily, Darwin was there to provide what seemed to many an acceptable theory of evolution.

As a result of all this, reflexes became a popular pattern of medical explanation. If you hurt *here*, then the pain was really caused by that over *there* connected by a nervous pathway—a reflex. A little known reflex from the 19th century is the nasogenital reflex. It related migraines, sneezing, and neurasthenia by nervous reflex to too much sex. The evidence in part is that the nasal mucosa is true erectile tissue. Too many erections (vicarious or otherwise) and one got coryza. Thus, one way to cure the problems of depression, whose psychologic basis may be some deeply held views about whom one would like as a sexual partner, was by cauterizing (or removing) parts of the nasal mucosa.

The nasogenital reflex has gotten some recent press. The following is from *The New York Times* Book Review section (January 12, 1992).

> In the second half of the 19th century, another dominant paradigm took hold—"reflex theory," the idea that "every organ in the body could influence any other organ in the body." Women persuaded by reflex theorists sometimes sought relief from migraines by having their ovaries removed. In the grip of the same theory, Freud—suffering various ailments—had his nose cauterized [in keeping with the tenets of the proponent of the nasogenital reflex, W. Fliess.][1]

Precisely what was this nasogenital reflex and who was Willhelm Fliess?

Fliess was for many years the personal physician and close friend of Sigmund Freud. Fliess[2] was a principal proponent of a complex theory that related (primarily female) sexual disturbances to changes in nasal physiology and to cyclic rhythms that governed both physical and psychologic states. He described several intranasal locations as "genital spots," examination of which could reveal hysterical disorders, masturbatory practices, and sexual derangements. Further, Fliess asserted that many of these disorders were treatable by anesthetizing or surgically removing the affected nasal spots.

Belief in the nasogenital reflex must have prompted Fliess to recommend nasal surgery for the treatment of Freud's symptoms. Fliess also advanced the theory that human actions were controlled by "vital periodicities": interweaving cycles of 23- and 28-day lengths. Together, these ideas provided a framework by which hysterical and neurotic disorders could be shown to have an organic origin, a view that Freud was eager to embrace and that he never fully abandoned.

Fliess' ideas concerning the nasogenital reflex and vital periods may seem ridiculous today, especially if the two are considered together as part of one theory. But the two are logically distinct; each can be held independently from the other. One might be true, while the other might be false. Existence of the nasogenital reflex was an hypothesis widely held by Fliess' contemporaries. It received support in the international medical literature for more than 30 years. Fliess' nasogenital theory was simultaneously advanced by an American otolaryngologist, John N. Mackenzie, professor of otolaryngology at Johns Hopkins, who claimed to have independently discovered (c 1883) genital zones in the nose that corresponded to those described by Fliess. Contemporary surgical texts related nosebleeds to "congestion of the sexual apparatus" and presented the nasogenital reflex relationships as accepted fact.

Many texts described "vicarious menstruation," that is, periodic nosebleeds caused by the cyclic changes of the female sexual apparatus. There were many case reports citing successes based on the nasogenital theory. Conferences in both America and Europe often had papers dealing with various aspects of the nasogenital reflex. Numerous authorities published their favorable results in applying nasal surgery to the treatment of feminine sexual disturbances. In 1914, Mayer[3] reported on a series of 93 women cured of their dysmenorrhea by nasal surgery. Mayer claimed an overall success rate of 50% to 75% by treatment of the genital spot in the nose.

Fliess' theory concerning the nasogenital reflex must therefore be viewed in this context as an early version of a widely supported medical theory. It was accepted and confirmed by

many of the leading physicians in America and Germany.[4]

We see then that there was evidence for what appears to us to be a bizarre hypothesis. Of course, many of the clinical successes attributed to the nasogenital reflex theory were a function of how medical conditions were defined, and this in turn was due to a set of beliefs that physicians had about women, who were the primary target of reflex theories.

Present-day students (and some physicians), told about the nasogenital theory, will laugh. How could anyone have believed it? They laugh because most scientific preparation is not really preparation for understanding science in an historical context. Rather, it is preparation for learning present-day doctrine. It is all too easy to think that if *we* have evidence to refute the nasogenital reflex theory, no one should ever have believed it. But this assumes that evidence is not, in part, a function of other beliefs, beliefs that change with time. Indeed, if the story of nasogenital reflex shows anything clearly, it is that what seems reasonable to believe changes against a backdrop of other beliefs.

So, we can say that the nasogenital reflex proponents did all they could with what they had. If they are to be faulted, it is for not being able to see where social values were influencing what they took to be scientific views about women.

Given that preferred treatment modalities change so often; given that new evidence is constantly being gathered, we should expect that we will be laughed at in 100 years by our fellow practitioners. But is there a way to ensure that it will be friendly laughter and not true scorn? If we, today, want to be judged the way I suggested we should judge advocates of the nasogenital reflex theory, what must we do? I will examine this question by looking at Chapman's reflexes.

Chapman's reflexes

What follows is my synopsis of Chapman on Chapman's reflexes:

Chapman's nodes are small individual or groups of gangliform or nodular, superficial masses, found in specific body locations, that quite often are sensitive to palpation. They are related to alterations of visceral function, and they can—in theory—be used in both the diagnosis and treatment of these disturbed organs.

Chapman's nodes are localized changes in the neurolymphatic end structures that produce the palpatory change. The nodes are found where the nerves from the organs pass through fascial layers on their way to the surface. They feel like BBs or large globules of tapioca. The specific pathologic nature of these changes is yet to be identified. But, they are excellent diagnostic tools; for some conditions almost unerring. They are also reliable in treatment. One treats the nodes by exerting pressure against them with the fingers, to make them "melt away." Chapman does not explicitly say so, but if this is treatment of more than the node, that is, treatment of the condition signified by the node, then making the node "melt away" must produce changes in the distant organ affected, just as excising the "genital spots" helped to cure problems of a psychosexual nature according to the nasogenital reflex theory.

One must accept the empiric quality of the nodes; this is the nature of medicine.[5] This last point is put clearly and with clever, tongue-in-cheek manner by Owens[6]:

> As a result of many years experience wrestling with the human body, the writer has found that not being overly bright has its advantages....Without much sense to start with, such an individual occasionally blunders into a way of doing things in a simple, unscientific way that works, which a man of good sense would hesitate to try....The only excuse we have for presenting the simple procedures set forth, herein, is that they work, and frequently have given relief where the more scientific methods evolved by men of intelligence and national reputation in this field have failed.

Owens' comment about himself is clearly self-deprecatory and meant to be humorous. But, his comment about the need for continuing with what works without scientific backing stands for a philosophy of medicine that might well lead to our being seen in 100 years with the scorn I suggested we cannot have for

the adherents of the nasogenital reflex theory. A further look at Chapman's reflex provides an illustration.

Suppose that the nodes *are* actually a part of the medical problem. This is one way to interpret Chapman. Thus, one aspect of having kidney stones is the accompanying set of nodes. But, no matter how obvious the nodes are to the touch, it is not clear how they are (or could be) fully integrated with the rest of the body. Why is this so?

Full integration would require that rubbing away a set of nodes connected with kidney stones will make the kidney stones melt away. Yet, given today's knowledge, one would be hard pressed to come up with a mechanism to explain how this tapioca-like "BB" could, when its excess fluid is pushed aside, cause enough of a change in body chemistry to make this reabsorption happen. In other words, there is no known physiologic mechanism that makes sense out of Chapman's nodes. Put yet another way, to a hard-headed scientifically minded physician, Chapman's nodes just cannot be real things. (One can claim that there is a mechanism, but that it is so complex and takes so long to manifest itself—and then only if other conditions are met—that it is not surprising that we overlook it. The problem with this view is that to some it sounds more like grasping at straws than appealing to science.)

Feeling the node is subjective in precisely the way that physicians, as putative scientists, are taught should be considered untrustworthy. (Notice that what we take to be scientific may itself be a function of who is "politically" in charge of science at any given time.) Evaluating the node, once felt, however, as pointed out before, is not at all subjective. The claim is that the connections are pretty much rule-governed, or they could not work in diagnosis. Certainly, the scientific temperament would ask, How do they work?

Is this, however, the physician's concern? Do we have to understand something before we use it? To some clinicians, Owens, for example, anything that works clinically should be used. So, one answer is that for all its science, medicine is so very pragmatic that its practitioners might not even have to ask for the underlying explanation. Again, the previously cited quotation from Owens is an example. Here, we have come to a well-known tension in contemporary medicine between the need for science and the need to get a job done, to some extent no matter what. Put another way, to some, the only appropriate aim of medicine is cure, or at least alleviation of symptoms. Why? becomes a metaphysical and not a scientific question. So long as the nodes are reliable diagnostic tools, so long as they lead to successful treatment, one should not ask for more.

Is this really enough for medicine?

Not good enough

If the nodes are known from experience to be trustworthy diagnostic signs, then there must be some underlying explanation. Indeed, to take a line from Still, there must be some underlying mechanical-engineering–like principle. There must be a real reflex.

The reason for the search has two parts. First, if it works and therefore will benefit patients; and if the only way to widen use of the reflex is to find its underlying mechanism, to make it scientifically respectable, then we are obligated to do so. Second, if it is just a mistaken idea, used only by a group of osteopathic medical curmudgeons, then we ought to find that out, if only to save their patients.

Remember that the nasogenital reflex theory was supported by clinical successes. One moral to that story is that the notion of what counts as a clinical success is "up for grabs." With this in mind, it is easy to see that there should be no tension between clinical and scientific medicine. If the two cannot work together, then, to take a line from Benjamin Franklin, they will assuredly fail together.

Conclusion

The pragmatic concerns of medicine allow for different criteria of acceptance for certain principles of practice. Full understanding can take a backseat to clinical success in some circumstances, but not as a general rule.

The notion of clinical success, however, is relative and fragile enough to give one pause. What I and a patient take to be a success may

well be due, in large part, to what we already believe about what works. Thus, it behooves medicine (as much as science) to make every effort to substantiate the principles of clinical practice.

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