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Using Medical History to Study Disease Concepts in the Present: Lessons from Georges Canguilhem

Nicholas Binney

RESUMEN

Aunque en la actualidad los médicos puedan pensar que la patología clínica se deriva de la fisiología normal, defiendo aquí que eso no es necesariamente el caso. Históricamente, la fisiología puede haberse derivado de la patología clínica. Después de derivar el conocimiento fisiológico de este modo, los médicos pueden revertir la prioridad conceptual para simular que el conocimiento fisiológico está en la raíz de la práctica médica. Esto implica que el supuesto conocimiento fisiológico objetivo puede estar influenciado por los juicios evaluativos que se hacen para definir los conceptos prácticos de la patología clínica. Argumento a favor de este punto de vista usando la historia de la enfermedad del fallo cardíaco y para ello me baso en un argumento que se debe a Georges Canguilhem.

PALABRAS CLAVE: *Georges Canguilhem; conceptos de enfermedad; historia del fallo cardíaco; teoría bioestadística de la enfermedad; relativismo científico.*

ABSTRACT

Even though medics in the present day may think that clinical pathology is derived from normal physiology, I argue here that this is not necessarily the case. Historically, physiology may have been derived from clinical pathology. After deriving physiological knowledge like this, medics can reverse the conceptual priority, to make believe that physiological knowledge is at the foundation of medical practice. This implies that supposedly objective physiological knowledge can be influenced by the evaluative judgements made to define practical concepts of clinical pathology. I argue for this view using the history of the disease heart failure, drawing on an argument made by Georges Canguilhem.

KEYWORDS: *Georges Canguilhem*, *Concepts of Disease, History of Heart Failure; Biostatistical Theory of Disease; Scientific Relativism.*

I. INTRODUCTION

Philosophers of medicine tend to focus on present-day medical practice to inform their views on contemporary medical knowledge. For

example, Christopher Boorse (1987), has advocated using contemporary reference works (such as the American Medical Association's *Standard Nomenclature of Diseases and Operations*) to learn about different concepts of health and disease, how they stand in conceptual relationship to one another, and whether or not they are objective. This makes intuitive sense, as in order to learn about the present, it seems sensible to study the present, and not the past. I suggest that, in contrast to this, in order to learn about present-day disease concepts, it is also important to study their past.

With regard to the conceptual priority of disease concepts, naturalists, such as Boorse (1977); (2014), argue that physiology has conceptual priority over pathology, which has conceptual priority over practical concepts of clinical disease. On this view, physiologists define what normal physiology is, and use this to define what pathology is, and pathology is used to define clinical concepts of disease. Naturalists hold that physiology and pathology are objective concepts, insulated from the changes to human culture and evaluative judgements that influence the practical concepts of clinical disease. Many normativists, such as Lennart Nordenfelt (2001), take a reverse view, in which clinical concepts of disease have conceptual priority over pathology. On this view, value laden clinical concepts of disease are used to define pathology. However, physiology (understood as the description of biological operations of the body) remains objective and is also used to define pathology as disvalued physiology.

In contrast to both of these views, I argue that Georges Canguilhem (1991) [1966], thought that the relationship between pathology and physiology was rather more complex[1]. Canguilhem is a much neglected philosopher of medicine [Méthot (2013); Peña-Guzmán (2018)], who argued that studying the historical development of concepts was crucial to understand their character. Contemporary empirically oriented philosophers of medicine study the concepts of health and disease as they are used in the present [Lemoine (2015); De Vreese (2017); Fuller (2018); Tresker (2020)].[2] Following Canguilhem, I argue that medical history is of central importance to medical philosophy. I take up the challenge set by David M. Peña-Guzmán (2018), who, referring to Canguilhem, argued that:

> One of the most drastic implications of this historicist position is that philosophers who fail to engage the history of the sciences simply fail to engage the concepts that animate scientific rationality and, consequently, fail to do epistemology. To avoid this failure, philosophers must develop a historical

> conscience and come to see the philosophy of science not as a department of logic, as A. J. Ayer wished, but as a department of genealogy" [Peña-Guzmán (2018), p. 42].

Canguilhem argued that in the present medics do define clinical concepts of disease and pathology using physiology, but that in the past the physiology we use today may have been defined using pathology and clinical concepts of disease. Crucially, this means that neither pathology nor physiology need be insulated from the evaluative judgements used to define practical concepts of clinical disease. I argue that contemporary naturalists and normativists have been too quick to accept that the biological operations of the human body can be objectively described. I use some medical history to argue that present day descriptions of what is happening to patients in heart failure are influenced by past ways of understanding the disease, which were themselves influenced by value judgements made long ago. I begin by outlining different views in the philosophy of medicine regarding the priority of different concepts related to disease (section II). I then present a little history of heart failure, discussing how left ventricular ejection fraction came to be an important index of heart disease (section III). I close by using this historical material to argue in favour of my reading of Canguilhem's view (section IV). My aim is not to prove that this view is universally correct, as this is not possible to do with a brief historical treatment. I only aim to make Canguilhem's position plausible, whilst preserving the distinction between truth and wishful thinking. I argue that medical history may be necessary to investigate the conceptual relationship between pathology and physiology.

II. Priority of Pathology or Physiology?

Contemporary debates about the concepts of health and disease are framed by naturalist and normativist schools of thought. Naturalists hold that concepts of disease, or of pathological conditions, are objective. According to naturalists, what diseases are is fixed by the way the natural world is and is uninfluenced by changes to human culture and evaluative judgements. Normativists, by contrast, hold that there are no such objective concepts of disease, and that all concepts of health and disease can change as human culture and evaluative judgements change.

Naturalists, such as Christopher Boorse (1977); (2014), make biological function central to their account of health and disease. Boorse argues that biological function is also an objective concept, and he defines health and disease in terms of the biological function of an organism's parts and processes.[3] To be healthy is to have typically functioning organs and physiological processes. To be diseased, or to have a pathological condition, is to have atypically reduced functioning of an organ or physiological process. Boorse's theory is referred to as the biostatistical theory of health and disease (the BST).

So, according to Boorse, physiologists study the human body to discover the function of organs and physiological processes. Atypically diminished functioning of an organ or process is deemed pathological. Thus, pathology is subordinate to physiology, as the former is defined in terms of the latter. The pathological can only be known once the physiological is known. Both pathology and physiology, however, are taken to be value free and objective. They are value free because they can be defined without reference to value laden language, and objective because they are not influenced by changes to human culture.

Boorse (1975); (1977); (1997), is also careful to distinguish *theoretical* and *practical* concepts of health and disease. Theoretical concepts are those of the pathologist and physiologist. They are used to investigate how the human body objectively functions and malfunctions, without concern for human culture or evaluative judgements. Boorse distinguishes these objective concepts from the practical concepts used by medical practitioners to care for the sick. Boorse recognises that, in clinical medicine, evaluative judgements influence which pathological conditions require medical attention. Nevertheless, the practical concepts are a subset of the theoretical concepts — those theoretical concepts that are judged worthy of medical attention. Boorse recognises that this judgement could be made for a variety of reasons, including that a pathological condition has produced symptoms that are undesirable. Even so, Boorse maintains that it is possible to have malfunctioning organs, and therefore diseases, that never produce undesirable symptoms, and are never considered as diseases from a practical point of view.

So, the practical concept of disease is subordinate to the theoretical concept of disease. Clinical disease, and clinical pathology, are those objective pathological conditions that are disvalued for some reason, making them worthy of medical attention. As objective pathology is itself subordinate to physiology, clinical pathology is also subordinate to physiology. According to Boorse, medics determine what clinical diseases are

by first learning about physiology, then determining what physiology is atypically reduced and therefore pathological, and then determining what pathological conditions are disvalued and therefore clinically diseased.

As clinical pathology is subordinate to physiology on this view, changes to the content of physiology will influence the content of clinical pathology. The reverse, however, is not true. Here, physiology is not defined in terms of clinical pathology. Changing the values that define clinical pathology does not influence the content of physiology. So, according to the BST, physiology is *independent* of clinical pathology, but clinical pathology is *dependent* upon physiology.

Normativists, such as Lennart Nordenfelt (2001), p. 53, argue for the reverse of this view. They argue that medics do not determine what clinical pathology is by first determining what objective pathology is, and then determining whether this is disvalued. First, says the normativist, medics determine that a person is in a state that they disvalue, that the person is suffering with an illness. They then study this person's physiology, and if they find that the physiology of the person contributes to the person's suffering, then this part of the person's physiology is pathological. Thus, pathology is a form of disvalued physiology.[4] Therefore, for the normativist there is no objective pathology. As human culture changes, evaluative judgments may change, and this in turn may change that part of physiology which is deemed pathological.

Notice that on this view physiology, understood as the description of the operation of the human body, remains objective, as does the physiological explanation of disease. Once the evaluative judgement about the presence of illness is made, then physiology is studied objectively, uninfluenced by those evaluative judgements.

> As soon as this happens, the sciences of disease start living their own life. It then becomes possible to consider the biological or psychological processes in themselves and temporarily disregard their relation to human illness. This is the reason, Fulford and I argue, why medical scientists can regard their enterprise as an exclusively objective scientific affair [Nordenfelt (2001), p. 53].

For pathology to be determined, *both* that which is disvalued *and* physiology must first be determined; and this physiology remains free of cultural influence, and free of value laden terminology. On this view, pathology is caught in a pincer movement between human evaluation and objective physiology, and is conceptually subordinate to both.[5]

This view concerns naturalists. Although physiology may be objective, and free of cultural influence, pathology is not. Naturalists fear that

this would allow any aspect of physiology to be considered pathological, so long as someone disvalued it. Naturalists fear that, on this view, claims that a condition was pathological could not be true or false, as they reduce to mere expressions of preference.

Canguilhem (1991) is credited by Nordenfelt (2007), along with Bill Fulford (1989), as an advocate of the reverse view of health and disease. Canguilhem rejects Claud Bernard's view that normal healthy people are studied objectively, in a way that is disconnected from human culture, interests and values, to produce knowledge of an ideal physiological state known as health [Scholl (2015), p. 406; Tiles (1993), p. 735]. This physiological knowledge, according to Bernard, is then used to derive equally objective pathological knowledge as deviation from this ideal. Canguilhem rejects this by arguing that such a disinterested investigation could never dictate what this ideal should be [Canguilhem (1991), p. 84; Tiles (1993), p. 735], and because the qualitative difference between the healthy and the pathological are not found in the quantitative differences between physiological states [Canguilhem (1991), p. 57]. Instead, Canguilhem argues that physiological study begins with the recognition of disease, and it is through the recognition of disease that normal physiological functions are understood. "Disease reveals normal functions to us at the precise moment when it deprives us of their exercise…Health is organic innocence. It must be lost, like all innocence, so that knowledge may be possible" [Canguilhem (1991), p. 101].

Canguilhem argues that both pathological abnormality and physiological normality require norms supplied by the practice of medicine. For example, he considers the distinction between benign and malignant tumors, arguing that such a distinction need not be drawn by disinterested investigators. As it is drawn by pathologists, this reveals an interest in directing surgical treatment that they take from the clinic. "Let's think about a surgeon. What would he say if a pathologist, after performing a biopsy of a tumor, were to answer in sending him his findings, that whether a tumor is malignant or benign is a question for philosophy, not pathology?" [Canguilhem (1991), p. 219]. Continuing this line of thought, we can consider why the boundary between cancerous tissue and normal tissue exists. Although we may consider cancerous tissue distinct from the rest of the patient's body, would it be so unreasonable to see the tumor as a part of the patient's body, as normal tissue? The boundaries between the cancerous and normal, and between the benign and malignant, arise in the clinic along with the interest in treating the patient. Following René

Leriche, Canguilhem argues that physiological knowledge is derived from pathological knowledge, not the other way around.

> Leriche himself thinks that we progress more often in fact – and should always in theory – from medical and surgical technology prompted by the pathological state to physiological knowledge. Knowledge of the physiological state is obtained by retrospective abstraction from the clinical and therapeutic experience [Canguilhem (1991), p. 98].

All this can be read as in-keeping with the reverse view put forward by Nordenfelt. On this view, the physiology of how a human body is operating can be objectively described, but that part of these operations that are pathological cannot without appealing to culturally influenced norms. Canguilhem even says as much. "Structures or behaviors can be objectively described but they cannot be called "pathological" on the strength of some purely objective criterion" [Canguilhem (1991), p. 226].

However, Canguilhem also suggests that the contingencies of human culture shape those very descriptions as well. For example, again quoting Leriche, Canguilhem suggests that the problems posed in the clinic influence the way in which physiology is explored.

> By contrast, Leriche thinks that physiology is the collection of solutions to problems posed by sick men through their illnesses. This is indeed one of the most profound insights on the problem of the pathological: "At every moment there lie within us many more physiological possibilities than physiology would tell us about. But it takes disease to reveal them to us". Physiology is the science of the functions and ways of life, but it is life which suggests to the physiologist the ways to explore, for which he codifies the laws" [Canguilhem (1991), p. 100].

This suggests that how physiological laws themselves are codified is influenced by how pathology is understood in the clinic. Physiological knowledge is a solution to a problem presented by pathological knowledge. The problem is to explain the difference between the healthy and the diseased, to answer the question "what happened to make this person diseased?". The answer to this question will depend on the contrast between the healthy and the diseased, and this contrast itself depends on cultural norms. According to Canguilhem "One can carry out objectively, that is impartially, research whose object cannot be conceived and constructed without being related to a positive and negative qualification, whose object is not so much a fact as a value" [Canguilhem

(1991), p. 229]. The methods of the physiologist may be objective, in the sense of being impartial. Even so, if the objects studied (the healthy and the diseased) are not objective, in the sense of being uninfluenced by human culture, then the explanation of the difference between them will not be objective either. "Summarizing the hypotheses, we proposed in the course of examining Leriche's ideas, we can say that in biology it is the *pathos* that conditions the *logos* because it gives it its name" [Canguilhem (1991), pp. 208-209]. This raises the question of how the conscious experience of problems (pathos) conditions the rigorous study (logos) of biology. Some have read Canguilhem as arguing that concepts like 'pathology' are "historical schemas that condition what epistemic agents can perceive, say, think, and know" [Peña-Guzmán (2018), p. 42]. On this view, the vast sea of physiological possibilities is made tractable by the pathological experience in the clinic. Pathology's role may not simply be to select from an objectively given physiological descriptions of the human body. Rather, pathology may be *constitutive of those physiological descriptions*.[6]

Canguilhem argues that the influence of pathology on physiology is obscured in the present. He claims that medics do see physiology as a purely objective discipline that serves as the foundation of the clinical concepts of disease [(1991), p. 123]. Even physiologists are not aware of the influence the clinic has had on their discipline [Canguilhem (1991), p. 123]. Even though physiological research begins in the clinic with pathology, eventually medics reverse the logical priority that pathology enjoyed over physiology and come to conceive of clinical pathology as logically derived from physiology. "The abnormal, as ab-normal, comes after the definition of the normal, it is its logical negation. However, it is the historical anteriority of the future abnormal which gives rise to a normative intention" [Canguilhem (1991), p. 243]. "In pathology the first word historically speaking and the last word logically speaking comes back to clinical practice" [Canguilhem (1991), p. 226]. Even so, as medics make use of physiological knowledge, Canguilhem suggests that they smuggle the norms that arose in the clinic back into medical practice. "And it is a question of whether medicine, in doing this, wouldn't take back from physiology what it itself had given" [Canguilhem (1991), p. 123]. Although medics today may conceive of their highest quality pathological knowledge as objectively derived from an objective physiology, this physiology is itself derived from an earlier (and perhaps less successful) pathology, which was laden with the subjective norms and value judgements of the clinic. Canguilhem observed that once this physiology has been produced from pathology, medics will often reverse the con-

ceptual priority, to think of pathology as if it was produced from physiology, and kid themselves that this makes pathology objective.

> Today an objective pathology proceeds from physiology but yesterday physiology proceeded from a pathology which must be called subjective and thereby certainly imprudent, but certainly bold, and thereby progressive. All pathology is subjective with regard to tomorrow" [Canguilhem (1991), p. 212].

So, according to Canguilhem, even though medics in the present may talk about physiology as if it was independent of human culture and objective, this is not the case. He "cannot admit that physiology can be constituted before and independently of pathology in order to establish it objectively" [Canguilhem (1991), p. 211]. Even if medics conceive of pathology as subordinate to physiology, if this physiology was itself subordinate to a pathology that was full of normative, evaluative commitments, then this physiology must itself be dependent upon these commitments, no matter what they tell themselves in the present. As Elselijn Kingma (2012) has suggested, this would produce pathological and physiological knowledge that is value laden despite there being no trace of this value ladenness in how medics speak of these subjects. Canguilhem invites us to look at the historical development of medical knowledge, in order to see how norms and values may influence what appears to be purely objective knowledge in the present, as Kingma (2012) has also recently suggested. This provides an opportunity to explore the character of physiological knowledge, as well as its relationship with pathological knowledge, as Maël Lemoine (2015) has recently called for; but to do so from a historical perspective.

The challenge to the objectivity of the biological description of the physiology of the human body should concern naturalists even more. If no aspect of medical knowledge, not of disease, nor of function, nor of the biological description what happens in the body is objective, then what is left of the notions of truth and falsity?[7] What is there to prevent medical knowledge being equivalent to anything a person believes to be true? As Boorse[8] puts it, "if we abstract from all questions of truth and falsity, then cows jump over the moon" [Boorse (1997), p. 77].

I take this problem very seriously and accept that it has been a persistent problem in the philosophy of medicine. H. Tristram Engelhardt (1974) has also championed an account of medical knowledge that does away with objective truth and falsehood. Engelhardt argues that "The world in which we live is not furnitured by uninterpreted facts. We see

the things around us in terms of social and theoretical expectations" [Engelhardt (1996), p. 190]. Engelhardt illustrates this effect with the example of being diagnosed with a heart condition, in which mere symptoms are transformed into a disease by such expectations.

> Consider the transformation of experienced reality accomplished by the diagnosis of heart disease. A slight shortness of breath or the swelling of the ankles after a long day of work becomes a sign of disease. Sleeping on two pillows is no longer an innocent occurrence, but a possible stigma of a deadly disease. The individual's view of life is changed by a set of expectations regarding the dangerousness of heart disease and the possibility of an early death [Engelhardt [1996], pp. 189-190].

The trouble with this view is that the transformative experiences are the same regardless of whether the expectation are true or false. The medical literature on overdiagnosis is replete with examples of patients who have been terrified by test results that do not carry the significance they think they do. Misdiagnoses shape expectations just as correct diagnoses do. Engelhardt has also been criticised for failing "to account for the common view that there is more to the term 'disease' than just a statement of our values" [Ereshefsky (2009), p. 224; see also Lennox (1997)].[9] To distinguish between truth and falsehood we need something more than beliefs, desires and expectations. We need something that resists expectations, that is a certain way regardless of what people expect or desire. This is why naturalists, quite reasonably, cling to some form of culture independent objectivity.

Drawing on my reading of Canguilhem, my challenge is to show how physiological descriptions of how the human body operates are not objective, in the sense of being independent of the contingencies of human culture. Rather these physiological descriptions are dependent upon clinical pathology, which is itself dependent upon changing human cultures and evaluative judgements. I need to show that even though today's clinical pathology appears to be subordinate to today's objective physiology, today's physiology was once subordinate to yesterday's clinical pathology. I need to show how physiology and pathology are not insulated from changes to human culture and evaluative judgement, whilst preserving the distinction between truth and falsehood, wishful thinking and propaganda. To do this I need to present some history of medicine, focusing on the disease heart failure. This history shows how the acceptance left ventricular ejection fraction as an important index of cardiac function was influ-

enced by evaluative judgements made long ago and are thus not independent of them.

III. A Little History of Heart Failure

In the early nineteenth century, people studied sick patients in the Parisian hospital system [Hope (1833)]. They found that some people came the hospital short of breath and oedematous — with body fluid accumulating in their arms, legs, and in other places. At post-mortem examination they found that many of these people had lesions on the valves of their hearts. They produced a theory to explain how these lesions could lead to breathlessness and fluid accumulation. The lesion was believed to cause an obstruction to the flow of blood, causing a build-up of pressure in the circulation, causing fluid to be pushed out of the blood vessels into the lungs and other bodily tissues. The fluid in the lungs produced the breathlessness, and the fluid in the rest of the body resulted in the edema [Hope (1833), p. 195-200].

In the early nineteenth century the stethoscope was invented by René Laennec [Duffin (1998)]. This allowed medics to hear the murmurs produced by these lesions, and to identify lesions in patients before they died. Such patients were advised to avoid strenuous physical exertion, so that the build-up of pressure behind the heart could be avoided. It was said that the heart could change in size and shape to "compensate" [Broadbent (1897), pp. 90-91] for the incompetent valves, and this state of compensation could be maintained so long as the heart was not given too much work to do. By keeping the circulation of these patients "tranquil", medics hoped to prevent patients developing breathlessness and fluid accumulation [Hope (1833), p. 326]. They hoped to protect their patients' hearts from being damaged by the obstruction by limiting their physical exertion.

This advice spread from hospital practice into general practice. In hospital practice, patients presented to doctors because they were very sick. In general practice, family doctors had the opportunity to examine people who were not very sick, and even people who were not sick at all. When such medics listened to their patients' hearts, they found many patients who had murmurs. Many of these patients were not wealthy and had no choice but to do strenuous work in order to make a living. Many of them also engaged in other physically strenuous activities, such as playing sports or giving birth. Despite their murmurs, the great majority

of such patients never developed any breathlessness or fluid accumulation [Mackenzie (1913), p. 341]. Against medical expectation, patients' hearts did not seem to decompensate following exertion.

This put medics in a difficult situation. The research program on which their knowledge of heart disease was based was focused on the detection of lesions, like narrowed and incompetent valves, and the correlation of these lesions with the symptoms and signs shown by patients. In this research program, lesions were diseases. The finding that so many patients with valvular disease of the heart never became ill led medics to doubt the clinical significance of valvular disease. This doubt, in turn, lead many medics to doubt that lesions were diseases at all.

In the early twentieth century, the influential British physician James Mackenzie galvanised opinion about how to cope with this problem. He advised that doctors should not be satisfied with detecting lesions [Mackenzie (1913), p. 8]. He argued that a doctor's role was to serve the needs of their patients. In his experience, his patients did not simply want to know whether they had a lesion, they wanted to know how the presence of a lesion affected their future [Mackenzie (1916), pp. 21, 37]. They wanted to know if it was safe for them to do strenuous work, play exhausting games, become pregnant, or join the military [Mackenzie (1916), p. 22]. To do their job, Mackenzie argued that doctors needed to be able to offer their patients an accurate prognosis. As focusing on detecting lesions did not allow doctors to do this, Mackenzie argued that medics should not define disease in this way.

Instead, Mackenzie argued that doctors should pay attention to the breathlessness and fluid accumulation itself when diagnosing disease [Mackenzie (1916), p. 45]. When patients with valvular disease developed these symptoms, their hearts were said to have decompensated. Around 1900, medics referred to this symptomatic state as "heart failure" [Fleming [1997], p. 145]. Mackenzie argued that the best guide to the patient's prognosis was to be vigilant for the earliest signs of the development of this syndrome. Furthermore, Mackenzie argued that the diagnosis of heart failure should be applied to patients with this syndrome even if they did not have any apparent pathology of their heart [Mackenzie (1913), pp. 7-8].

As the syndrome of heart failure could occur in patients without apparent pathology, Mackenzie argued that the obstruction caused by the valve lesion played no important role in the generating the syndrome of heart failure. He proposed that the true cause of this syndrome was the reduced cardiac output [Mackenzie (1913), pp. 10-11]. However, he had

no means of measuring cardiac output. In the 1930s, after Mackenzie's death, the American doctor Tinsley Randolph Harrison [(1935)] took up this question and measured cardiac output in patients in heart failure. This research determined, perhaps surprisingly, that many patients who were thought to be in heart failure because of their symptomology did not have a low cardiac output. Furthermore, improving cardiac output did not improve the patients' symptoms [Harrison (1935), p. 52-69].

Consequently, medical researchers were faced with another difficult situation. They expected that patients with heart failure could be identified using the symptoms of breathlessness and fluid accumulation, and that the physiological explanation for these symptoms was a reduced cardiac output. And yet, they found that many patients with the symptomology of heart failure did not have a reduced cardiac output. What should they conclude from these results? Should they conclude that the symptomology of heart failure was as they expected, but that their physiological explanation was incorrect? Or should they conclude that their physiological explanation was correct, but that the symptomology of heart failure was not as they expected? At least these two options were reasonable interpretations of these results.

The latter interpretation would certainly have been impractical. In the 1930s, cardiac output was very difficult to measure, requiring complex inert gas rebreathing procedures [Harrison (1935)]. Even when right heart catheterization became more widely available in the 1940s, the measurement of cardiac output would still have involved passing a catheter into a patient's heart, which is quite an invasive procedure. So, there were certainly practical barriers to defining heart failure this way in this period. However, it does not even seem to have occurred to researchers like Harrison that some of these patients might not have had heart failure. They inherited the view that heart failure could be recognised from its clinical presentation, and just assumed that this was the case. Consequently, Harrison [(1935), p. 69] argued that low cardiac output was not the physiology of heart failure, and that something else had to be. Harrison made the former interpretation.

Harrison (1935), drawing on the early twentieth century physiological work of Ernst Starling and colleagues [Patterson, Piper and Starling (1914)], suggested an alternative physiological explanation for heart failure. Starling had shown that the work done by a heart as it emptied during contraction was influenced by the degree to which the heart chambers were stretched during filling. A heart that was subjected to higher filling pressures stretched more as it filled, and worked harder during contrac-

tion, ejecting a greater volume of blood. Harrison (1935), pp. 246-247, suggested that a failing heart was not one which could not maintain a cardiac output (as Mackenzie had), but rather was one which could only maintain a cardiac output if the filling pressures were raised. These raised filling pressures, he suggested, were what caused an increase of pressure in the lungs and in the systemic circulation, leading to fluid being pressed from the vessels in these tissues, leading to fluid accumulation in the lungs and in other parts of the body.

This understanding of the physiology of heart failure is related to the acceptance of left ventricular ejection fraction (LVEF) as an important index of cardiac function in the present day. LVEF is the ratio of the amount blood ejected from the left ventricle during the ejection phase to the maximum amount of blood contained in the left ventricle at the end of the filling phase. So, having a LVEF of 50% means that half of the blood in the left ventricle at the end of filling is ejected during emptying. On Harrison's (1935), pp. 246-247, view a failing heart is one which cannot maintain the volume of blood ejected without increasing the volume of blood at the end of filling. In such a situation, the ratio of the ejected volume to the maximum volume, the LVEF, will be low.

Notice that in Harrison's research conceptual priority is given to the clinical syndrome of heart failure, and not to the physiological measurements associated with the disease. The low cardiac output theory was rejected because patients with heart failure did not all have a low cardiac output, and patients with heart failure were known by their clinical syndrome. The low ejection fraction theory was proposed as an explanation of the clinical syndrome in these heart failure patients. Here, heart failure is defined clinically, and this clinical definition is used to recognise the physiology of heart failure. *Harrison gave conceptual priority to the clinical presentation over physiological explanation.*

Even though the clinical syndrome took priority over physiology in this instance, this is not always the case. LVEF is also difficult to measure. LVEF can be measured using radionuclide ventriculography and echocardiography, but these techniques did not become sufficiently refined and available to study the LVEF of heart failure patients until the 1980s [Dougherty et al (1984); Soufer et al (1985); Gadsbøll et al (1989)]. Their results, yet again, placed medical researchers in a difficult situation. They expected that patients with heart failure could be identified using the symptoms of breathlessness and fluid accumulation, and that the physiological explanation for these symptoms was a reduced LVEF. And yet, they found that many patients with the symptomology of heart fail-

ure did not have a reduced LVEF. What should they conclude from these results? Should they conclude that the symptomology of heart failure was as they expected, but that their physiological explanation was incorrect? Or should they conclude that their physiological explanation was correct, but that the symptomology of heart failure was not as they expected? At least some researchers now made the *latter* interpretation [Gadsbøll et al (1989), p. 1017]. They argued that as many patients with the clinical syndrome of heart failure did not have a reduced LVEF, this syndrome could not be a reliable guide to who did and who did not have heart failure. *They gave conceptual priority to physiological explanation over clinical presentation.*[10]

Today, a person with a low LVEF is said to have cardiac dysfunction, and only said to be in heart failure if they have clinical symptoms and signs of heart failure as well. The European Society of Cardiology require the presence of both the symptoms and signs of heart failure and "objective" evidence of cardiac dysfunction for a diagnosis of heart failure to be made [Ponikowski (2016)].

IV. Pathology in the Past Influences Physiology in the Present

If philosophers were to go and talk to a cardiologist today, they may well be told something that appears to conform to the naturalistic view of disease. To have a normally functioning heart is to have a typical LVEF. A reduced LVEF is understood as an "objective" sign of cardiac dysfunction. A typical LVEF may be understood as being healthy and having a reduced LVEF as having a disease. Having a reduced LVEF without breathlessness and fluid accumulation is described as asymptomatic cardiac dysfunction, as asymptomatic disease. Having cardiac dysfunction may be considered a disease even if this condition is not disvalued in and of itself. This looks like the objective, theoretical disease concept described by the BST. As developing symptoms is disvalued, symptomatic cardiac dysfunction is given a special designation in clinical medicine - heart failure. This looks like the evaluative, practical concept of clinical disease acknowledged by the BST. The BST's concept of clinical disease is objective dysfunction that is disvalued for some reason, and heart failure is objective cardiac dysfunction that is disvalued because of the presence of symptoms.

So, from the present day, it looks as though the measurement of cardiac function and dysfunction is objective and non-evaluative. It looks as though the clinical concept of heart failure is defined using the objective concept of cardiac function. As cardiac dysfunction is the theoretical disease concept of the BST, and heart failure is the practical disease concept of the BST, "today" (as Canguilhem put it) the practical concept of disease appears to be subordinate to and dependent upon the theoretical concept of disease. But appearances can be deceiving.

Recall, the reason the BST needs practical concepts of clinical disease to be defined using theoretical, non-evaluative and objective concepts of disease, is to insulate these theoretical concepts from the contingencies of human evaluative judgements. The BST requires that theoretical disease concepts do not change as evaluative judgements change clinical disease concepts. The BST requires that theoretical disease concepts are independent of evaluative judgements, and as practical concepts of clinical disease are dependent on evaluative judgements, theoretical disease concepts must be independent of practical concepts of clinical disease. Whilst it is true that LVEF can be defined without appealing explicitly to evaluative judgements, without the use of value-laden terminology, that does not mean that the definition of cardiac function in terms of LVEF is independent either of practical concepts of clinical disease or of human evaluative judgements.

Harrison rejected low cardiac output as the physiology of heart failure because patients with the clinical syndrome of heart failure did not have a low cardiac output. He proposed something like a reduced LVEF as the physiology of heart failure, as a better explanation for this clinical syndrome. Even the most basic descriptions of what was happening to these patients to cause their symptoms was influenced by the clinical category into which they were placed. If the clinical syndrome accepted as heart failure was different, cardiac output might have been accepted as the physiology of cardiac dysfunction, and LVEF may not have been. Harrison's work shows that LVEF need not be a form of cardiac dysfunction unless the clinical concept of heart failure was as it was in 1930. Therefore, cardiac dysfunction is what it is because the clinical concept of heart failure was what it was. Cardiac dysfunction is dependent upon the clinical concept of heart failure. As cardiac dysfunction is the theoretical disease concept of the BST, and heart failure is the practical disease concept of the BST, historically (or as Canguilhem put it "yesterday") the theoretical concept of disease was dependent upon the practical concept of disease.

Conceptions of normal physiology are also affected by this. As many patients with the clinical disease heart failure had a normal cardiac output, having a normal cardiac output was not deemed sufficient for a patient to be physiologically normal. Normal physiology is not insulated from the clinical disease concepts. Harrison's work focused attention on things like ejection fraction, in the hope that all patients with the clinical syndrome of heart failure would have a reduced LVEF – in the hope that all people without a reduced LVEF would be physiologically normal with respect to the function of their hearts. Here, cardiac dysfunction is not conceived of as not having normal cardiac function. Instead, having normal cardiac function is conceived of as not having cardiac dysfunction. Medics determined what is physiologically normal by studying pathology, instead of determining what pathology is by studying the physiologically normal.

As normal physiology is influenced by pathology, and pathology is influenced by practical concepts of clinical disease, and practical concepts of clinical disease are influenced by changes to human culture and evaluative judgement, normal physiology can be influenced by these changes. Mackenzie's work shows that the clinical concepts of heart failure would not have been as they were if it were not for certain human evaluative judgements. Patients valued prognostication. They wanted to know if it was safe for them to do a strenuous job, or to become pregnant, or to join the military. Mackenzie valued prognostication as well, so much so that he saw it as the main role of the family physician. Human value judgements about the importance of prognostication, and the societal role of professionals, played a central role in fixing the clinical syndrome of heart failure as a practical concept of clinical disease. As did the tradition in which Mackenzie was working, which made the connection between heart disease and a syndrome of breathlessness and fluid accumulation. Mackenzie took this inheritance and modified it in the light of his values, solidifying heart failure as a clinical disease. Harrison inherited this clinical disease from Mackenzie, and present-day medics inherit disease concepts from Harrison (and others). This is how present-day concepts of heart failure and cardiac dysfunction can be influenced by past concepts of heart failure and cardiac dysfunction, and by evaluative judgements made in the past.

If this view is correct, then no aspect of medical knowledge, not clinical disease, nor pathology, nor physiology, is insulated from changes to human culture and evaluative judgement. No aspect of medical knowledge is objective. However, this does not mean that medical knowledge reduces

to what people expect or desire to be the case. Mackenzie and Harrison may have expected that patients with the clinical syndrome of heart failure would all have a reduced cardiac output. Despite these desires and expectations, this was not the case. Claims to the contrary would have been and remain wrong. Today, patients in heart failure with a reduced LVEF live longer when treated with angiotensin converting enzyme inhibitors. Claims to the contrary are wrong. There are facts about these disease entities, even if these facts do not exist independently of the evaluative elements that make them possible. Traditional, cultural, ethical and evaluative elements may be necessary to produce medical knowledge, but they are not sufficient determine what this is. We can deny that there are any objective, or culture independent, medical truths and falsehoods to be had, without affirming that cows jump over the moon.

CONCLUSION

Even though medics today may speak of clinical concepts of disease as if they are dependent upon an objective pathology, which is itself dependent upon an objective physiology, this need not actually be the case. Historically, the conceptual priority between physiology and pathology may well be reversed, with physiology being dependent upon pathology, which is itself dependent upon the clinical concepts of disease. Following Canguilhem, I have used some history of medicine to argue that present day concepts of cardiac function are indeed dependent upon historical concepts of cardiac dysfunction and heart failure.

This account is in tension with both the views of normativists and naturalists in the present. Naturalists, such as Christopher Boorse, hold that physiology and pathology are objective, and used to define practical concepts of clinical disease as disvalued pathology. Normativists, such a Lennart Nordenfelt, recognise the influence of changes to human culture and evaluative judgements on practical concepts of clinical disease, and on the definition of pathology itself. An objective (culture independent) physiological description of the biological operations of the human body, however, is required by these normativists to define pathology as a form of disvalued physiology. In contrast to this, Canguilhem argued (on my reading of him) that even these physiological descriptions are influenced by changing human culture and evaluative judgements. Following Canguilhem, I have used history to argue that no aspect of medical knowledge, not disease, nor function, nor the biological description what happens in

the body, is insulated from human culture, whilst emphasizing that this does not reduce medical knowledge to belief, expectation or desire. I suggest medical history can be used to explore the conceptual priority between pathology and physiology, to produce a more complete picture of contemporary concepts of health and disease.

Department of Medical Ethics, Philosophy
and History of Medicine, Erasmus MC
Room Na 24-15, Erasmus MC,
PO Box 2040, 3000 CA Rotterdam, The Netherlands.
E-mail: n.binney@erasmusmc.nl

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NOTES

[1] For a comparison of Canguilhem with naturalists such as Boorse, see Scholl and De Block (2015). I argue that Canguilhem also differs from normativists such as Nordenfelt as well, as I read him as problematizing descriptions of the biological operation of the human body in addition to concepts like disease and function. I recognise that Canguilhem can be read in other ways.

[2] Ljiljana Radenović (2017) has argued that philosophers of medicine have been insufficiently empirically oriented when studying health and disease and calls for more empirical investigation of medical knowledge in the present. I also call for more empirical investigation into the historical development of medical knowledge.

[3] Philosophical positions that take some form of biological function as objective and value free are popular in contemporary philosophy of medicine. Peter Schwartz (2007), Daniel Hausman (2012), Jacob Stegenga (2018) and Steven Tresker (2020) all use or modify Boorse's goal directed account of biological function. Others argue for different accounts of biological function. Jerome Wakefield (1992), Benjamin Smart (2016) and Paul Griffiths and John Matthewson (2018) argue for etiological or selected effect accounts of function. Sander Werkhoven (2019) argues for a dispositional account of function. In each case, biological function is taken to be objective and value free and are used to define disease/disorder/pathological condition.

[4] Note that for Nordenfelt (1995), p. 90, disease is not any form of disvalued physiology. Disease is physiology that is disvalued because it inhibits a person achieving their vital goals.

[5] In addition to Nordenfelt and Bill Fulford (1989), Marc Ereshefsky (2009) and Leen De Vreese (2017) have also argued that the biological operation of the human body can be described in an objective and value neutral way, and used to define disease, even if biological function cannot.

[6] This reading of Canguilhem is in line with Mauro Nervi's [(2010)] argument that objective description of biological mechanisms is not possible. "[T]here is no objective description of any mechanism, rather biological sequences are depicted by the biologist according to the pragmatic need of explaining functions" [Nervi (2010), p. 217]. It is debatable whether Canguilhem supported this view [Scholl (2015); Méthot (2013)]. Even so, I take Canguilhem's work as inspiration to explore this possibility.

[7] Historians of medicine also argue about the philosophical status of medical knowledge and disease categories [Wilson (2000); Cunningham (2002); Muramoto (2014)]. Some historians adopt a naturalist/realist view of diseases, treating them as real and independent of human culture, allowing them to apply contemporary medical knowledge to the past. Others adopt a historicist/conceptualist view, where diseases are fully determined by what historical actors believed about diseases, and where retrospective diagnosis is an illegitimate historical practice [Wilson (2000)]. Some historicist/conceptualist historians adopt the most extreme relativist position, where knowledge is reduced to whatever people believe to be true. "Hence it remained and remains true, that in all these periods the people died of what their doctor (or their bystander) said they died of. And that's that" [Cunningham (2002), p. 34].

[8] Boorse is not responding to Canguilhem here, but rather to Engelhardt (1974), who he reads as ignoring the difference between what is true and what is not.

[9] I am sympathetic to Engelhardt's (1996), pp. 227-228, neo-Kantian aspirations, but also agree that these criticisms of his work are reasonable.

[10] Other researchers did not dismiss the clinical syndrome as inaccurate, arguing that there must be another type of cardiac dysfunction, diastolic heart failure, to explain the discrepancy between the clinical syndrome and a reduced LVEF [Dougherty et al (1984); Soufer et al (1985)]. The historical story I am telling here is obviously incomplete. Even so, researchers did lose confidence in the ability to detect heart failure from a patient's symptoms, as I highlight.

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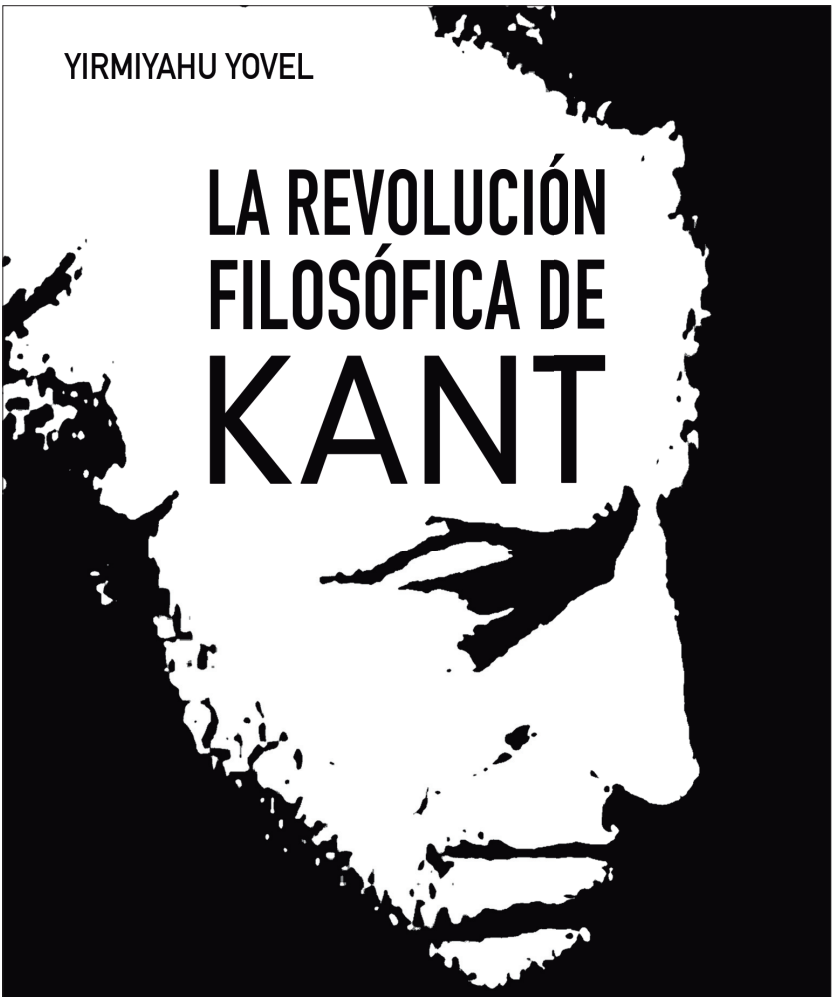

UNA BREVE GUÍA DE LA *CRÍTICA DE LA RAZÓN PURA*

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