

# Why Has the Field of Psychology Not Developed Like the Natural Sciences?

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*concerning to this article*  
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### Abstract

The present article suggests an answer to the question of why natural sciences such as physics have been able to develop unified theories that provide satisfactory and efficient explanations for many natural phenomena, while psychology has failed to develop unified theories to explain psychological phenomena. The article's answer is based on the observation that in physics, the units of measurement (UMs) have an expression in theoretical terms that are the equivalent of observational terms (UMs-equivalency). In contrast, in psychology, UMs have an expression only in theoretical terms. The UMs-equivalency in physics is not a sufficient condition for constructing successful unified-theories, but it is a necessary condition. Not every physical theory that maintains UMs-equivalency becomes a successful theory, because the theory may not properly represent the processes in reality. This article develops and justifies this idea and suggests that it is difficult to imagine a successful unified theory in psychology because UMs-equivalency does not exist in this field.

*Key words:* Measurement, methodology, scientific development, theoretical concepts, observational concepts

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## Why Does the Field of Psychology Not Develop Like the Natural Sciences?

### Introduction

*Don't use twice in row.*

The present article suggests an answer to the following question: why is there a wide gap between the scientific development of the natural sciences (particularly physics) and the development of psychology? This question regarding what I call the "developmental gap" is connected directly to the general problem of whether psychology can be considered a science like physics. If it transpires that psychology has developed in a direction that differs from physics, one may justifiably argue that psychology does not resemble the natural sciences. Given this, the query arises regarding the developmental difference between these two disciplines. This will be discussed in more detail below, but briefly, the answer is that while physics has developed successful unified theories (e.g., Newton's), psychology has failed in this regard. What is the explanation for this failure?

The present article proposes an answer based on the following observation: in physics, the units of measurement (UMs) are expressed in theoretical terms and in observational terms (called "UMs-equivalency"); whereas in psychology the UMs are

*Complete the "query" thought*

expressed only in theoretical terms.<sup>1</sup> For example, in physics<sup>2</sup> the theoretical term “length” has a real expression. That is, the concept of length has an actual procedure of measurement (see a full description below) and measuring devices that use UMs such as millimeter, centimeter, meter, and kilometer. In contrast, in psychology, there are no real, empirical UMs for concepts such as love, hate, interest, memory, or

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<sup>1</sup> The distinction between theoretical and observational concepts has been subjected to severe criticism. For example, it has been argued that observational concepts are theory-laden (Bogen, 2013; Clark and Pavio, 1989; Lambert and Brittan, 1992; Rakover, 1990). Nevertheless, I believe that this distinction is of great importance theoretically and practically, and psychologists continue to use it. Few psychologists would confuse concept group (I) [reaction time, pressing on a pedal, eye movement, heartbeat and breathing] with concept group (II) [ego, instinct, visual scheme, consciousness, perception and long-term storage]. Similarly, psychologists would not suggest that group (I) contains theoretical concepts or that group (II) contains observational concepts. Further, they would not say the two groups are the same. Clark and Pavio (1989, p. 510), who conducted empirical studies on this issue, have summarized the discussion about the theoretical–observational distinction and propose “that the distinction is generally valid.... [S]cientists do and ought to maintain distinct attitudes toward observational and theoretical terms when thinking or communicating scientific ideas.”

<sup>2</sup> I am concentrating on classical physics for the following two reasons. First, most scholars have adequate knowledge of classical physics but very little knowledge of the theories of relativity and quantum physics. Furthermore, very few have even minimal knowledge in neurophysiology and modern research on the brain. Second, the units of measurements referred to here are used not only in classical and other areas of physics (thermodynamics, electromagnetics) but also in other areas of research such as chemistry and biophysics. Hence, I believe that the theoretical point I would like to make about UMs-equivalency will be exemplified by referring to classical physics, which gave enormous impetus to research in the natural science at large.



## PSYCHOLOGY VS. NATURAL SCIENCES

*are not operational definitions in this direction?*

intelligence (see a historic discussion on the subject in Michell, 1999).<sup>3</sup> Physics and other natural sciences use the International System of Units (SI), based on seven fundamental UMs: meter, kilogram, second, ampere, kelvin, mol, and candela (~~see SI in references~~). From these seven basic measures, new terms of measurement are constructed, such as speed, acceleration, energy. The theory and practice of physics are based on these measurement units. This can be demonstrated in a simple way: it is possible to predict, from Galileo's law of falling bodies, the distance that a body in free fall will travel as a function of the passage of time. This prediction may be confirmed by conducting the appropriate measurements. It is clear that what is calculated theoretically can be measured empirically, because the theoretical terms of time and distance are equivalent to these terms in the empirical measurement. Nothing like this has yet occurred in psychology. No real UMs have yet been found in psychology upon which it would be possible to construct unified theories.

*on intelligence all of these?*

*5*

*has a footnote*

Furthermore, the solution to the problem of the developmental gap in UMs may also constitute a solution to the problem posed by Eugene Paul Wigner, the Nobel laureate for physics in 1963. He pondered how it is possible to understand the enormous success of mathematics in describing and explaining natural phenomena (especially in physics). He writes: "The first point is that the enormous usefulness of

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<sup>3</sup> The fact that UMs-equivalency holds for length does not mean that the measurement theory of length was developed first, and that its appropriate measurement units were developed afterwards. History teaches us that measurements of length and weight were developed before the mathematical formalization of the theory for measuring them. Therefore, what is described in this paper is the state of the art of measurement, the approach by which the theoretical term length is equivalent to the procedure for measuring length.

mathematics in the natural sciences is something bordering on the mysterious and...there is no rational explanation for it" (Wigner, 1960, p. 2).

The essentials of the proposal for a solution to the developmental gap and to Wigner's problem are illustrated in Figure 1.

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Insert Figure 1 here

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*where?*  
*are the initial/initials necessary?*

Figure 1 compares the methodological situation in physics with that in psychology by examining the connection between theory and observation. It is possible to characterize theory in physics as formulated in mathematical language whose concepts are based on UMs. The empirical observations in physics are also based on UMs, that is, on standard and real measurements according to the SI. The UMs in theory are equivalent to the UMs in reality (UMs-equivalency). Therefore, what is stated theoretically and mathematically also exists in the observable and measurable physical phenomena.<sup>4</sup>

In Figure 1, the broken arrow with the equal sign in the middle indicates two functions. The arrow signifies that it is possible to derive from the theory, under certain conditions, a specific prediction that can be tested by comparing it with the empirical observation (UMs). The equal sign emphasizes the UMs-equivalency, that

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<sup>4</sup> UMs-equivalency assumes equivalence between exact theoretical terms (numbers) and inexact empirical terms that depend on procedures of measurement (Sherry, 2011). The discussion of the problems arising from this situation is beyond the scope of the current article.

is, the fact that there is equivalence between the UMs in theory and the UMs in the actual observation.

In contrast, theories in psychology are stated primarily using everyday language, although in many cases mathematical language is also used. Some of these theories represent the UMs as theoretical terms. They have no real expression in observations. That is, in psychology, UMs-equivalency does not exist.<sup>5</sup> For this reason, the solid arrow in the psychology section of Figure 1 fulfills a single function: under certain conditions it is possible to derive from psychological theory specific predictions that can be compared to the behavioral observations called “behavioral indices.” These indices cover a wide range of behaviors: responses to stimuli (e.g., reactions, choices, answers to questions, or evaluations), speed of response, changes in the electrical resistance of the skin (measured by galvanic skin resistance [GSR]), changes in pulse rate, and changes in the blood stream in the brain (measured by fMRI). In some cases, it is possible to empirically test hypotheses about UMs and ascertain if the results support the qualities of an interval scale or a ratio scale.

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<sup>5</sup> One may suggest that psychology also uses a theoretical term: probability of response ( $p(r)$ ), which is equal to an observational term: percent (frequency) of response ( $\%(r)$ ). For example, in the well-known empirical generalization:  $p(r)$  is a learning function of motivation and training, and  $p(r)$  is estimated empirically by  $\%(r)$ . Nevertheless, the equivalence between  $p(r)$  and  $\%(r)$  is only valid for the dependent (explained) variable. There are no UMs for motivation or training, the independent variables, which explain the phenomenon. To measure motivation, one uses the intuitive indices of hours of deprivation or incentives. For training the intuitive indices, repetitions or reinforcements are used. In contrast, in physics there is full equivalence between the theoretical, explanatory concepts and the observational, explained concepts, both of which are based on UMs-equivalency.



In light of all this, it is possible to present the central argument of this article, which is based on UMs-equivalency, that is, the equivalence in physics between the theoretical UMs and the observational UMs (based on the SI). This equivalency is not a sufficient condition for building successful unified theories in physics, but it is a necessary condition. From UMs-equivalency, it does not follow that any physical theory based on UMs will be a successful theory. Unsuccessful theories in physics are based on the same building blocks, namely UMs. However, if a physical theory succeeds in representing processes in reality, then presumably this theory expects great success due to UMs-equivalency. Based on this, one may propose that in psychology, in which UMs-equivalency does not exist, successful unified theories have low chances of being developed. In this sense, one may expect that in other disciplines, such as sociology, unified theories will not be developed either. Indeed, several sociology researchers have suggested that the weakness in attitude research stems from inadequate measurement (e.g., Hauser, 1969; Zeller and Carmines, 1980).

UMs-equivalency is also the answer to Wigner's problem regarding how it is possible to understand the success of mathematics in describing and explaining natural phenomena (for other solutions, see Livio, 2009.) The answer rests on the fact that the units of theoretical computations are the same units of the empirical measurements.

This article develops and justifies the ideas presented above. It is organized in the following way. Section (A) briefly discusses the developmental gap between psychology and physics. It reaches the conclusion that this gap is generated by the status of theoretical discourse in psychology, which may be characterized by a "failure to develop a unified theory" (FDUT). That is, psychology has still not developed a successful, unified theory such as those that exist in the field of physics.

) There is central argument?

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Section (B) suggests a possible solution to the problem of FDUT, centered on the observation that no UMs-equivalency exists in psychology. Section (C) presents several examples from the field of psychology that support the proposal of UMs-equivalency. Finally, the Discussion summarizes and provides further support for UMs-equivalency in psychology and its relationship to psycho-reductionism, consciousness, and the generation of interval scales in this field.

gmt?

Restate the question please

**(A) The Developmental Gap between Psychology and Physics**

The article's question (and related queries) has been discussed in the psychology literature for many years (e.g. Lilienfeld, 2010; Zittoun, Gillespie, and Cornish, 2009). It has also been discussed in the broader literature, and indeed has long been a popular question. A simple Google search for the question "Is psychology a science?" yields dozens of articles in the professional literature, popular press, and blog communications, showing how relevant and hot the question is at present (e.g. Berezow, 2012; Henriques, 2016; Jogalekar, 2013). Most of these articles deal with methodological problems that prevent psychology from being considered a scientific discipline like the natural sciences (especially physics).

In this article, I do not discuss the relationship between psychology and the position of the general public or the economic policymakers who support psychological research, nor do I deal with outright scientific fraud. I examine only issues that appear to uphold the argument that psychology has not developed like the natural sciences. In the following section, some of the main explanations for psychology's FDUT are presented, along with counterarguments demonstrating how these have been addressed in the field. (The discussion is based on the following

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here

Better to get to it

usually we stand clear of what is to come & just get to it.

Too many INITIALISMS

studies: Ferguson, 2015; Lilienfeld, 2010, 2012; Pashler and Wagenmakers, 2012; Rakover, 2012; Sanbonmatsu and Johnston, 2019; Zittoun et al., 2009.)

### Experimental Control

Due to the enormous psychological complexity of individuals (animals as well as human) the control mechanisms that are found in natural science experiments are not possible in psychology. For example, the degree of interest or impatience of participants in laboratory experiments in psychology may vary greatly. As a result, it is not clear precisely what is being tested in the experiment and what affects participants' behavior.

The response to this criticism is that a random sampling of the participants balances the conflicting tendencies (e.g., slight interest among some is compensated by great interest in others) so that the effect of the independent variable on the dependent variable will be obtained across the sample of participants.

### Hidden Psychological Processes

Most explanations in psychology are based on cognitive processes that cannot be observed directly. These processes are, in a way, only theoretical concepts.

~~The response to this critique is that even in the natural sciences, in many cases~~ scientists base models on theoretical processes that cannot be observed directly. For example, there is still no way to directly observe the force of gravity. According to Heisenberg's principle of uncertainty at the sub-atomic level, as the certainty about the location of a particle (e.g., electron) increases, the certainty of its momentum decreases, and vice versa.

### Empirical Generalization

Frequently, the findings based on a certain sample cannot be generalized to other samples. Moreover, repeated tests are not always able to obtain the same findings, even with the same sample of participants. One reason is that the memory of the first experiment is liable to influence the results of the repeated experiment. ↗

The response to this criticism is that this problem can be solved by correctly planning a series of experiments, for example, through use of a between-subjects design. Furthermore, generalization between samples raises the inductive problem (statistics speaks of a relation between the sample and the population from which it was drawn). Science does not deal with generalization of findings from situation to situation, but with whether a certain hypothesis or theory succeeds or fails in explaining the observations under various conditions (whether the hypothesis is confirmed or not).

### Confirmation Bias

There is a strong tendency among authors and journal editors to publish studies with affirmative results that support the authors' hypothesis. Results that are not significant are seldom published, either because the authors do not submit the articles or the editors reject them. The result is that the published literature reflects the interests of the researchers and editors. In addition, criticisms have been raised that some studies are conducted in a way that leads to statistically significant results (e.g., by enlarging the sample size).

There are several responses to this criticism. First, such a tendency also exists in the field of physics. Steven Weinberg, Nobel laureate in physics in 1979 writes in his book *Dreams of a final theory* (Weinberg, 1993) that the analysis of results from the empirical test of Einstein's theory of relativity was influenced by knowledge of a prediction derived from this theory (bending of a light ray that passes near the sun).

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Secondly, it is possible to overcome this tendency by publishing the research hypotheses and methods before the research is conducted. This solution is not free of flaws. Developing the research question requires a large number of preparatory experiments by means of which researchers construct and modify their theoretical and empirical perceptions. Thus, when an experiment is conducted, many flaws have been filtered out by the preparatory experiments. A requirement to publish the hypothesis and methodology in advance may stand in opposition to this natural research process.

It is to be emphasized that almost all experiments test the researcher's hypothesis against at least one alternative hypothesis. The results serve to determine which is correct among two or more hypotheses. In this respect, one may say that while some researchers tend to reinforce their favored hypotheses, others tend to reinforce an alternative hypotheses. It transpires that scientific criticism is not harmed.

### The Crisis of Replication

In recent years, psychology has been beset by a replication crisis. The repetition of multiple studies (especially in social psychology) have not yielded the desired results. Hence, the methodological requirement of replication has not been met.

The response to this criticism is that it is possible to present numerous research works in psychology, from conditioning and learning to cognitive psychology, which are replicable in an almost trivial way. For example, no journal would now publish an article demonstrating that hungry rats can learn to press a pedal in order to obtain a food pellet. Similarly, no author would submit an article about an experiment confirming the classic Müller-Lyer optical illusion (discussed below). Here again, it is suggested that the proper solution would be prior publication of the research's hypotheses and methods. (Although it is unlikely that any journal would

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prior-publish the traditional methods for obtaining the Müller-Lyer illusion.)

Furthermore, certain studies in biology cannot be replicated.

### Complexity

Sanbonmatsu and Johnston (2019) proposed that, in comparison with physics, the development of social and behavioral sciences such as psychology is inferior, because the topics studied in the field of psychology are much more complex than the topics studied in the field of physics. However,

The response to this criticism is that no consensus exists regarding the definition of complexity as applied to science. A given area of research becomes more comprehensible after a theory has successfully solved most of the problems in that research area. However, physics is also an extremely complex science. Even the basic and essential concept of mass is highly complicated. Its definition in Newton's theory (resistance to applied force) is different from weight, and from mass in the theory of relativity and in quantum theory. Moreover, the many sub-atomic particles that have been discovered and the interactions between them are so complicated that Weinberg (1993), the Nobel laureate in physics, describes how difficult it is to develop a unified theory for the area of sub-atomic physics. Finally, to date, there is no accepted theory that unifies quantum theory and general relativity theory. Physics is no less complex than psychology, although their factors of complexity are different. I cannot specify the complexity factors in physics since I am not a physicist, but I can assert that I believe the origin of complexity in psychology stems from the phenomenon of consciousness (e.g., Rakover, 1990, 2018). However, Sanbonmatsu and Johnson (2019) did not address this concept.

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italics only, no bold

paragraphs must be more than one sentence.

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this means →

This short review of the criticism of psychology makes it difficult to understand why psychology is not considered a science like the natural sciences. First, for all the problems summarized here, there is a reasonable methodological solution. Second, it emerges that, to some degree, the natural sciences are troubled by similar problems. Is the conclusion that psychology differs from the natural sciences mistaken? I believe it is not.

Not really  
You added  
material to  
imagine  
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that psychology  
is not a  
science.

A general historical overview of psychology (e.g., Leahey, 2004) indicates that no field of psychology (e.g., perception, learning, social psychology, developmental psychology) has yet developed a successful unified theory. In contrast, in physics, the theories of Newton and Einstein and quantum theory offer acceptable explanations for a host of observations and discoveries.<sup>6</sup>

Other researchers have reached similar conclusions. For example, in 1986 Paul A. Mehl, a clinical psychologist and philosopher of science, wrote an article examining whether there is a connection between basic psychological science and clinical practice. His conclusion was that there is no integration between these two domains. Further, even an examination of research in basic psychological processes produced intractable problems. Previously, in 1973, Allen Newell, a cognitive psychologist and specialist in computer sciences, wrote an article summarizing and criticizing an array of articles presented at a conference on processing visual information. Newell found that every empirical paper presented had the same

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<sup>6</sup> According to Kuhn's (1970) approach, these three unified theories constitute the basis for three scientific paradigms in physics. Kuhn also suggested that psychology was still in the pre-paradigm stage, since a unified theory in psychology has not yet been developed (Rakover, 1990).

which three?



structure: an interesting phenomenon had been discovered and two contradictory explanations were offered. For example, a distinction between a single memory system and dual systems; serial or parallel processing; single or multiple coding; decay of memory or interference; innate or learned processes; conscious or unconscious processes; gradual or one-trial learning; and so on (see Figure 2 in Newell, 1973). The problem is that these hypotheses, supported by interesting empirical findings, do not cohere towards development of a unified theory. In other words, Newell suggested that dual opposing hypotheses do not advance psychology as a science. He maintains that in another 30 years all we shall obtain is a new collection of articles describing two opposing hypotheses to explain new empirical and cognitive discoveries.

In contrast, other psychologists propose that theories such as Freud's psychoanalytic theory, Hull's theory of learning, or Estes's stimulus sampling theory (SST) can be considered unified theories in the field of psychology (e.g., Estes, 1950; Hilgard and Bower, 1966; Marx and Cronan-Hillix, 1987). While these theories were initially well-accepted, within a few decades each had been disconfirmed empirically and theoretically. (The great influence of psychoanalysis on literature and everyday discourse is a different subject altogether.) For example, Bower (1994) reviewed SST and found that it encountered many problems when applied to new and complex behaviors. Currently, SST has fallen out of use for several reasons. Researchers are interested in new theoretical and empirical questions, and the stimulus-response approach that dominated psychology in the 1950s has been replaced by the new information-processing approach. Even the learning theory that Pavlov developed has been refuted, although his experiments are still the cornerstone of the field of research on animal learning (Kimble, 1961).

are there the explanations?

not three or four?  
Always two?

well, 30 years has elapsed

no need to abbreviate

explain or give an example of refutation please?

There have been several attempts to develop unified theories on the basis of the artificial intelligence approach to cognition, such as Newell's (1992) SOAR. Although SOAR made important contributions to understanding cognition, it has received much criticism and has sparked controversies (see e.g. Cooper and Shallice, 1995; Garcia-Marques and Ferreira, 2011; Lewis, 2001). SOAR seems not to have been accepted as a unified theory for psychology, in the way that Newtonian theory has been accepted in the field of physics.

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THE SAME GAP you said didn't exist (on page 14?)

On the assumption that the developmental gap centers on the inability of the field of psychology to develop a successful unified theory, the question arises as to how this gap between psychology and the natural sciences (physics) may be explained. The answer suggested here is based on the fact that psychology has not succeeded in discovering UMs empirically, as physics has. In the following section, I discuss the topic of measurement in psychology.

**(B) A Possible Explanation of FDUT: The Problem of Units of Measurement in Psychology**

Figure 1 highlights a basic difference between physics and psychology: an attempt to bridge the theory-observation gap. In physics, the bridge across the theory-observation gap is based on the equivalence between theoretical and real UMs (e.g., a ruler to measure length). In psychology, the bridge is based on the use of hypothetical UMs that are indexed by individual behavior. In other words, theories developed by physicists use theoretical UMs that are equivalent to real UMs. Researchers in the field of psychology use hypothetical UMs that are connected to observations only through the predictions made from the theory about behavioral indices.

Q

But this is a key issue?

good point - I hope you extend this transition



To clarify this matter, I move on to a brief description of conducting measurement in physics and in psychology (this summary is based on Coombs, Davies, and Tversky, 1970; Michell, 1999; Tal, 2017). It emerges that psychology is influenced by two contrasting approaches to measurement. The first is offered by the physicist and philosopher of science Campbell, and second by the experimental psychologist Stevens. According to Campbell's approach, measurement is based on an empirical discovery of the relation between a certain amount of a quantitative property and the UM of that same property. For example, the relation between the length of stick L and the UM = meter is:  $L/\text{meter} = 3$ , that is, length L is 3 meters. (For a discussion of the terms "quantitative property" and "number" see Michell, 1999.) Stevens' (1946) approach holds that measurement is the ascription of numbers to objects or events according to rules. (On other approaches to scientific measurement, such as the realistic and the representational, see Tal, 2017.)

Give a citation?  
Give first names, I suggest  
expand this disjointed paragraph

Give simple reference here

huh?

found? A key word - but you present it casually

An important point in Campbell's measurement approach, which I wish to emphasize, is that <sup>researchers</sup> scientists found an empirical operation (e.g., counting how many times UM goes into the length of L) that upholds mathematical properties that define the world of numbers on which the mathematical language in a physical theory is based. To illustrate this, let us examine the following two mathematical properties: transitivity and additivity. The transitive relation states, for example, that if  $(A = 15) > (B = 10)$  and  $(B = 10) > (C = 5)$ , then  $(A = 15) > (C = 5)$ ; and the additive relation proposes that  $(C = 5) + (B = 10) = (A = 15)$ . These relations exist in the group of lines A, B, C:

A|-----|

B|-----|

C|-----|

*This means ??*

To show this, we first define a natural and arbitrary unit of measurement of length by means of the section (-). Second, we count how many times this unit goes into A (15 times), B (10 times), and C (5 times). Finally, we see that the lengths of the three sticks uphold the transitive relation, because A is greater than B, B is greater than C, and A is greater than C; and also the additive relation is upheld, because  $A = B + C$ .

Measurement of the length of an object by means of a real and arbitrary UM = [a dash (-) or a meter] maintains all of the mathematical properties of numbers. Therefore, what is determined by means of numbers is also determined by means of the lengths of the measured objects. The same may be said of other quantitative properties such as weight and time. Measurement of weights is based on the use of scales, and measurement of time is based on the use of a periodic phenomenon, such as the earth revolving around the sun. Many other measurements are derived from fundamental UMs (length, weight, time), such as speed (distance/time), acceleration, energy, etc. (see SI). Some other measurements are based on physical laws such as temperature, which is founded on the Ideal Gas Law and on thermal expansion (see Bringmann and Eronen, 2016; Sherry, 2011).<sup>7</sup>

*Why did you not say clocks?*

*A law is not the phenomenon*

<sup>7</sup> Methodologically, Bringmann and Eronen (2016) and Sherry (2011) suggest that it would be worthwhile to think of the qualitative/quantitative status of psychological terms as analogous to the development of the term temperature. The term temperature changed from a qualitative term into a quantitative term as a result of the development of an appropriate physical theory: temperature constitutes a measure of average kinetic energy of microscopic particles. Following this analogy, one may expect a psychological term to be transformed from qualitative to quantitative status by developing a suitable theory. Although this notion is attractive, to the best of my knowledge such a development has not been achieved in the field of psychology. For example, despite the massive investment in empirical and theoretical research on the concept of intelligence, one cannot treat measurement of IQ like measurement of length or weight.



In the field of psychology, on the other hand, this approach to measurement is not found. No psychological properties (cognitive, mental) may be measured not by an empirical discovery of a relation between a certain amount of the psychological property and the UM of that property. Neither can one appeal to an empirical psychological law by means of which it will be possible to measure some mental property. This circumstance caused researchers and philosophers such as Kant, James, and Leibovitch to cast doubt on the possibility of developing a psychological science (see discussion in Algom, 2019a, Marks and Algom, 1998). However, this is not the position taken in the present article. I propose that psychology upholds many properties of science but it is not a science like the natural sciences (physics) because of the difference in UMs.

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The mathematical psychologist Luce (1972, p. 96) expressed the matter thus: on the one hand, "...psychological measurement is not of a character closely analogous to either fundamental or derived physical measurement... In brief, the reason is that psychological measures do not exhibit any fixed relation to physical measures and most likely not to one another when examined over individuals. This is reflected in the absence of any structure to the units of psychophysical measures." On the other hand, Luce proposed a hypothesis whereby "... man — and any other organism — is, among other things, a measurement device, in function not unlike a spring balance or voltmeter, which is capable of transforming many kinds of physical attributes into common measure in the central nervous system. According to this view, the task of psychophysics is to unravel the nature of that device."

Q

We recommend not ending with a long quote.

Luce agreed that measurement in psychology is not like measurement in physics, yet he suggested a research approach based on the metaphor of perceiving the human being as a measurement device. By comparison, Stevens (1946) proposed a broad definition of measurement, based on the attribution of numbers to psychological properties according to a

certain rule. This definition was adopted in the field of psychology, opening the doorway to the use of numbers and mathematics. The point, as Michell (1999) determined, is that psychologists have bypassed or ignored the need to show empirically that the psychological property to which numbers are being applied is indeed a quantifiable property that can be characterized by an additive structure. That is; "... there has been little serious scientific research undertaken to show that the relevant attributes are really quantitative and, therefore, that the relevant attributes are measurable." (Michell, 1999, p. 187).

**(C) Examples that substantiate the Problem of Units of Measurement in Psychology**

intent

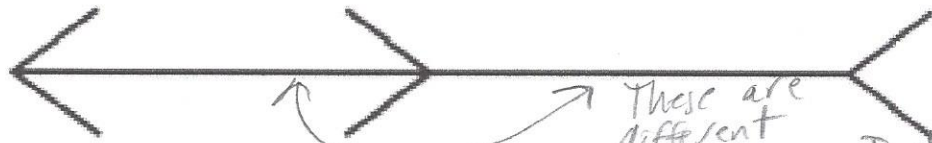
I will now demonstrate via several examples the argument that measurement in psychology does not work as it does in physics, and that the UMs-equivalency in physics does not exist in psychology.

We need more than one sentence per paragraph.

**Illusions**

italics, no bold

Consider the famous Müller-Lyer illusion:



not really

Measurement according to the methods utilized in physics reveals that the length of the right-hand line is identical to that of the left-hand line, although people tend to perceive the left-hand line as shorter. The reason is that the physicist measures the physical properties located outside the cognitive system, while many people who look at this illusion respond to the stimuli according to the information processing taking place in their perceptual systems.

I took a ruler to it.

(In many respects this is based on the realist approach to measurement: see e.g., Tal, 2017.)

Cost as fact note?

One may measure the size of the illusion by moving the right-hand line to the left until it looks the same as the left-hand line. The difference between the subjectively adjusted length

Boy this can be argued & not glossed easily over



and the objective length is considered an index reflecting the degree of the illusion. However, the index is no more than an expression of the information processing taking place in one's perceptual system. In fact, this measurement procedure results in what is called the "point of subjective equality," which is different from the "point of objective equality" measured by a ruler. Given this comparison between the physical and the psychological measurement, it becomes apparent that our perceptual system makes mistakes and distortions, offering new insight about the perceptual mechanism.

*Offering new insight about the perceptual mechanism.*

**Intelligence Quotient (IQ)**

Over many years, and at enormous expense in empirical and theoretical research, psychology has developed tests for measuring intelligence. At the end of the test/measurement process, subjects receive a numerical grade attesting to their intelligence level, known as the Intelligence Quotient (IQ). To what extent does IQ attest to one's level of intelligence? The following example substantiates that the IQ numerical grade is extremely problematic.

As an exercise, let us assume that Einstein's intelligence level was very high, with an IQ of 150. Is it then possible to argue that his intelligence level was equal to the total intelligence level of three imbeciles each of whom had IQ = 50? If one assumes that IQ is a quantitative attribute (i.e., its structure is additive) then the answer is yes! But this answer is utterly ridiculous. Hence, one may propose that in many cases like this, psychology plays the math game correctly but without mirroring the psychological reality. Despite this drawback, one justification for the use of IQ grades is practical: to predict one's success in other tasks (see Coombs et al., 1970).

*not the preferred term*

*imbeciles*

**Consciousness**

*But could say one is 3 times higher than one "imbecile"?*

*Not sure about such an example because many psychologists say that are not interval scales.*

Several researchers argue that there is no problem in measuring, on an interval scale, subjective variables such as attitude, attractiveness, and feelings (e.g., Algom, 2019a, b). I disagree. Take for example the variable of attractiveness and consider the following possibility: Danny is attracted to X more than to Y; Danny is attracted to Y more than to Z; but Danny is attracted to Z more than to X! The additivity relation breaks down; nevertheless, no one will be surprised by this case, just as no one will be astonished by the following results of several soccer games: Team A defeated Team B, Team B defeated Team C, but Team C defeated Team A!

Our responses and actions are not purely motor movements — they are saturated with conscious experiences: sensations, feelings, intentions, wishes, and desires. Nevertheless, no unit of measurement has yet been developed for conscious experience. For example, it seems ludicrous to define a measurement unit of love ( $UM_{love}$ ), and say that Jacob loves Rachel 7.5  $UM_{love}$  more than he loves Leah. By this example, I do not suggest that one may not say that Jacob loves Rachel more than Leah, only that  $UM_{love}$  has not been discovered like the natural unit of length (the meter) and in this sense it is not possible to measure Jacob's love for Leah, and say that it is 10  $UM_{love}$  while his love for Rachel is 17.5  $UM_{love}$ . Similarly, *Michell* (1999, p. 88) *confirms* cites Von Kries: "One cannot explain what it means to say that one pain is exactly 10 times as strong as another." *(cited by Michell, 1999, p. 88).* Michell suggested that Von Kries did not realize that the quantity objection is connected to empirical testing (see also Marks and Algom, 1998).

In effect, I propose that the lack of scientific understanding of consciousness is the main reason why psychology, which adopted the methodology of the natural sciences, has not succeeded empirically in discovering real UMs for conscious behavior. If for consciousness one would discover UMs which are based on certain neurophysiological processes, a complete and satisfactory explanation of consciousness in terms of brain

processes would be developed. However, as I have argued elsewhere (Rakover, 2018), to date there is no accepted theory that explains satisfactorily the relation between mind and body, consciousness and brain (see Discussion)

### The "Unit-equality" Criterion

This criterion is built on a dimensional analysis. Accordingly, the combination of UMs on one side of a law's or theory's equation must be identical to the combination of the UMs on the other side of the equation (e.g., Rakover, 1997, 2002). Consider Galileo's law, namely free fall of bodies:  $S = 1/2GT^2$ , where S is distance of fall, T is time of fall and G is acceleration caused by the force of gravity. If S is measured using the meter as a unit, the expression  $GT^2$  must also be measured by the meter unit. A simple calculation shows that it is:  $\text{meter} = (\text{meter}/\text{time}^2) \times (\text{time})^2$ .

Does any psychological theory meet this criterion? No. Consider an overall structure of theory of psychology: Behavior = f(stimuli, neurophysiological processes, cognitive processes, mental processes). Clearly the criterion is not met. Behavior (number of correct responses) is not identical to the units with which the stimulus is measured (loudness of the noise), to the physical units of the brain processes (differences of electrical potential), to the measurement units of cognitive processes (information processing), or to the measurement of mental processes (consciousness). Actually, here one has a correlation between the dependent variable (left side of the equation) and the independent variables (right side of the equation).

To solve the problem of unit-equality, one may introduce certain constants into the above equation of the theory of psychology, so that their multiplications by the independent variables will result in the required UM of the dependent variables (number of correct responses). Unfortunately, this solution will not work, since in psychology the constants are



not invariable, i.e., they change over participants, time and situations. Therefore, the introduction of these constants is no more than ad hoc.

*indices, no bold*

*WNT*  
*coming*

**Psychophysics**

It has been suggested that the <sup>J</sup>ust <sup>N</sup>oticeable <sup>D</sup>ifference (JND) estimated in psychophysical experiments may well be a UM of sensation (e.g., Baird and Noma, 1978; Gescheider, 1997; Marks and Algom, 1998; Stevens, 1975). In these experiments, one estimates JND by answering the following question: given a certain stimulus (e.g., light, sound, weight), what is the minimal change in this stimulus for a participant in the experiment to sense a difference? Weber was the first researcher to find that the minimal change, the difference threshold ( $\Delta I$ ), increased in fixed relation to the intensity of the physical stimulus ( $I$ ) for a given sensory dimension (an empirical generalization called Weber's law:  $\Delta I/I = \text{Constant}$ ). Given Weber's law, Fechner assumed that an increase in  $I$  matches increase in the number of sensory measurement units of equal size — the subjective JND. This theoretical assumption about the sensory measurement unit led to the development of Fechner's law: sensation equals the product of a certain constant by the logarithm of  $I$ . Gescheider (1997, p. 11) writes: "... once a basic unit is established, one has only to count up units in order to specify the amount of a measured property. Thus, Fechner developed a scale of sensation magnitude by counting JNDs, starting at the absolute threshold."

*referred*  
*an experiment is referenced in papers, this cannot be mentioned as you have*

*Your habit is to quote these down*

Is JND similar to the UM of length (or weight)? The answer is no. The JND is a purely theoretical concept, an assumption proposed by Fechner. There is no parallel real measure of the subjective JND as there is, for example, of the equality of the theoretical/empirical concept of length. This is expressed in several ways. First, Fechner discriminated between two kinds of psychophysics: inner psychophysics that deals with the relation between sensation and brain states, and outer psychophysics that deals with the

*But there is an operationalism of it.*

*theoretical or empirical?*

relation between sensation and the stimulus. Fechner was interested in the unobservable inner psychophysics, and he attempted to infer it from the outer psychophysics. In short, Fechner's approach is filled with assumptions, including the central one about the subjective JND.

Second, several empirical results were not in accordance with the predictions derived from Fechner's psychophysical law. Furthermore, some other suggestions about the UM of sensation differed from Fechner's. For example, Stevens (1975) proposed that the UM of sensation is not fixed. This assumption led him to develop a new law according to which sensation is a power function of I. Stevens also introduced the direct method of magnitude estimation for constructing a ratio scale — a method that Fechner rejected (he believed that sensation should be estimated indirectly).

Finally, it should be emphasized once again that the term JND in Fechner's theory is nothing other than a hypothesis supported by the subject's responses to changes in the stimulus. In fact, this is precisely the empirical basis of psychophysics, and of psychology generally: the subject's response is a function of the stimulus,  $R = f(S)$ ; and if also one takes into account the organism itself, then  $R = f(S, O)$ . That JND (a UM of sensation) depends on the individual's responses may lead to a possible inconsistency. Consider the following hypothetical experiment (inspired by the famous experiments of Libet, 1985).

Let us propose that scientists have invented an advanced brain detector (ABD) that records a special brain signal that appears before a conscious decision is made. Assume further that this ABD was used in a psychophysical experiment, the results of which <sup>showed</sup> ~~showing~~ that (a) there was a big difference between the average JND based on ABD (the unconscious JND) and the average JND based on the participants' reports (the conscious JND); the unconscious JND was much lower than the conscious JND; and (b) no significant



correlation was found between the conscious and unconscious JNDs. Thus, the following questions can be raised: <sup>W</sup> which is the true JND, the conscious or the unconscious? Which should be used as a UM of sensation? Clearly, such questions do not arise with regard to <sup>the</sup> UM of length. It does not matter which arbitrary unit one uses to measure distance as there are simple formulas for transferring one unit to another (e.g., 1 inch = 2.54 centimeters)

From these five examples (illusion, IQ, consciousness, unit-equality and <sup>comment</sup> psychophysics) one may reach two general conclusions <sup>that support the paper's main</sup> ideas. <sup>W</sup> First, since psychology has not yet succeeded in discovering real UMs, it is difficult to express theoretical concepts such as sensation, perception, intelligence, and consciousness in an objectively measurable way. Rather, what has been accomplished is just the development of theories, models, and hypotheses that describe <sup>S</sup> how the cognitive system processes information, and relate <sup>S</sup> it to certain behavioral indexes. Secondly, the use of mathematics in some of these theories is liable to create inconsistencies because the theoretical concepts do not have identical real measurements.

### Discussion

<sup>Holmes no notes</sup> In this section I discuss the main ideas of the paper, justify the UMs-equivalency, and examine two alternative approaches to the paper's problem.

#### Main Ideas — <sup>remove</sup>

The basic question <sup>W</sup> the present article seeks to answer is why psychology does not develop like a natural science (e.g. physics). The study of this question led to the following observation: psychology has not developed unified theories as has happened in the field of physics. The explanation of this observation lies in the following distinction: unlike in the field of psychology, in the natural sciences such as physics, empirical units of measurement



(UMs) were discovered that uphold the rules and requirements of the world of numbers on which mathematics is founded, and thus the language of a physical theory was established.

Given this, what is expressed <sup>as</sup> ~~in~~ theory in physics and the theoretical terms based on UMs are reflected in the empirical terms based on UMs (UMs-equivalency). The empirical terms are established on seven measurement units — the SI, from which it is possible to construct all the empirical measurement terms, and which can be calculated and predicted by the relevant theories. The UMs-equivalency is not a sufficient condition but is a necessary condition for developing successful unified theories (because even if this UMs-equivalency exists, the theory itself may be incorrect).

Such a system of theoretical measurements that are equal to the empirical measurements has not been <sup>evident</sup> ~~discovered~~ in psychology, <sup>that is,</sup> ~~UMs-equivalency has not yet been revealed~~. In psychology, measurement terms are solely part of the theory, where the theoretical term and the empirical term connect essentially through the use of the operational definition. Hence, since in psychology UMs-equivalency does not exist, successful unified-theories have not been developed.

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**Justifications**

Several explanations of psychological measurement were discussed above, including those suggested by Luce (1972), who argued that psychological measures do not exhibit any fixed relation to physical measures, and Michell (1999), who proposed that psychological attributes do not exhibit additive structure. ~~In addition,~~ <sup>TO</sup> I offer the following. First, in comparison to a psychological theory, the efficiency of a physical theory is much greater because of the UMs-equivalency. Second, many explanations discussed above have been discarded as incongruous, except for UMs-equivalency. I ~~examine and~~ <sup>examine</sup> disqualify the

following two alternatives to UMs-equivalency: psycho-reductionism and consciousness, and the generation of interval scales in psychology.

*italicized, no bold* **Theory efficiency.** *write out fully* One reasonable explanation for FDUT can be attributed to the fact that psychology has a problem in bridging the theory-observation gap, *which* ~~This gap~~ is bridged in physics by the UMs-equivalency, ~~which does not exist in~~ ~~psychology.~~ *methodological viewpoint* It is well known from a methodological viewpoint that the efficiency of a theory is reduced *greatly* when the connection between theoretical and observational concepts is unstable (e.g., the values of validity and reliability are *extremely* decreased, e.g., Neal and Liebert, 1986; Rakover, 1990). The UMs-equivalency guarantees that in physics the theory-observation gap will be reduced to the maximum possible, since the theoretical UMs are equivalent to the observational UMs. ~~Hence,~~ *the theory-observation gap in a physical theory is bridged to a much higher degree than in a psychological theory.*

*below* Given this, one may raise the following question: *why* does UMs-equivalency hold true in physics but not in psychology? My answer, which will be elaborated ~~later~~ *on*, is this: given (a) the phenomenon of consciousness has not yet been grasped by the conceptualization of the sciences, and (b) most of human behavior is saturated with consciousness, so it follows that it is difficult to develop an explanatory theory of behavior on the basis of the methodology that is used in the sciences and is adopted by psychology (for a thorough discussion see Rakover, 2018).

*Italicized* **Reduction.** One may propose that if ~~psychology and particularly~~ *the study of* consciousness could be reduced to neurophysiological processes, then psychology would develop like a branch in the natural sciences. However, to the best of my knowledge this research program (reduction) has not yet been successful (Rakover, *But validity issues still exist!*)

1990, 2012, 2018). To clarify this issue, I shall describe briefly the classic methodology for inter-theory reduction, Nagel's (1961) model of reduction. A theory, which is called the reduced theory ( $T_R$ ), is reduced to a more basic theory ( $T_B$ ), when  $T_R$  is derived from  $T_B$  together with certain "bridging laws," which connect the concepts of these two theories. Usually the bridging laws are conceived of as identities. For example, in the case of reducing thermodynamics to statistical mechanics, it was proposed that temperature equals the average kinetic energy. In this case, statistical mechanics also offers an ontological (material) explanation for the macro concept of temperature through the micro concept of kinetic energy.

One of the most powerful arguments against psycho-neural reduction is that of "multiple realizations" (Fodor, 1974, 1998). To exemplify this argument, consider the state of pain. The "functionalism" approach proposes that a mental state (MS) can be realized by many different material states (e.g., various neurophysiological states) that fulfill the MS's causal role in producing behavior. Thus, pain is a MS that can be realized by material processes such as various neurophysiological processes found in a large number of organisms (humans, dogs, cats, fish, reptiles, etc.), and also in highly advanced and elaborate computers. Given this argument, it becomes impossible to reduce a psychological theory to a neurophysiological theory, because it is not possible to find a bridging law that will join the state of pain and a particular neurophysiological state. If this is the case, one cannot find a bridging law by means of which it would be possible to reduce psychological theory to neurophysiological theory.

A further argument against psycho-neural reduction is based on the requirement of "Unit-equality" (Rakover, 2002). I discussed this above and for convenience I will describe it again briefly here. It is not possible to discover a

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psycho-neural law because this law does not fulfill the requirement of equivalence of units, namely identity of the units of measurement on either side of the law's equation [e.g.,  $MS=f(NS)$ ]. Why? Because the measurement units of the psychological concepts are entirely different from the neurophysiological measurement units, and no common measuring standard can be found for them that will unite the psychological with the neurophysiological. While the concepts of the neurophysiological theory are measured by means of electrical, chemical and molecular changes, cognitive or mental theories are expressed in actions measured chiefly by frequency of correct responses and speed of response. Therefore, it is hard to see how a bridging law may be built to connect the concepts of these two theories. The combination of measuring units of the chemical changes is not equal, for example, to the measuring unit of the response-speed index. One reason for this is that this index expresses psychological, not physical, time. Moreover, response speed is an index that expresses actions of a large number of different cognitive processes (connected to perception, memory, and decision-making) which work linearly and in parallel (see discussion in Pachella, 1974).

Furthermore, a meticulous review of the literature regarding the question of whether it is possible to reduce consciousness to brain processes, and to develop a very sophisticated computer that has states similar to human mental states, has resulted in a negative answer (e.g., Rakover, 2018).

**Interval scales.** Given that in psychology the UMs terms appear only in theory, one can test these terms empirically through the derivation of predictions from the theory, that is, by testing whether these predictions are confirmed or refuted. I examine this approach by means of one example which deals with a mathematical model from which interval scales can be derived and also be supported empirically: the bisection experiment. In this

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experiment, a participant hears two tones, one high volume and one low volume, and is asked to produce a tone whose volume is halfway between these two tones. Based on this experimental task, a mathematical model was developed that generated an interval scale on which it was possible to scale the tones produced by the participant. That is, a good match was discovered empirically between the predictions deriving from the model and the participant's behavior. This supported the model and demonstrated that an interval scale can be constructed.

Given the above, Coombs et al. (1970, p. 25) <sup>maintained that</sup> write "The absence of a concatenation (or even bisection) operation in many areas of psychology has led to the development of measurement models of different kind. ~~The success of building interval scales is of major significance (my explanation is added in brackets).~~ Campbell argued that only extensive properties [based on a concatenation operation that corresponds to addition] can be measured on an interval scale, and since psychological attributes are intensive in nature [they are not extensive], no interval scale measurement in psychology is possible. The more recent research in measurement theory has shown, however, that the existence of an empirical concatenation is not necessary for an interval scale measurement, contrary to Campbell's views," (p. 19). ~~Don't close with a long quote - poor transition.~~

Thus, the following question can be asked: <sup>If</sup> if it is possible to build mathematical models in psychology that create interval scales theoretically and empirically, what in fact is the difference between psychology and physics? If it transpires that there is no substantial difference, it may be said that the basic idea of the present article regarding UMs is undermined. However, the answer is that despite the success of certain mathematical models in constructing interval scales, psychology is not equal in this matter to physics <sup>If</sup> ~~for the following reasons:~~

It seems that the creation of interval scales is limited to a small number of certain mathematical models. In psychology, the system of UMs-equivalency does not exist. To the best of my knowledge, even the subjective use of JND is nowhere to be found in other research fields, such as memory, social psychology, and personality.

The interesting developments in psychophysics, such as Stevens's theory and <sup>the</sup> Signal Detection Theory (SDT), were created among other things on the basis of a critique of Fechner's theory (e.g., Gescheider, 1997.)

Furthermore, basic terms in psychology are not properly defined and are <sup>thus have</sup> ~~riddled with~~ many interpretations — a situation that puts ~~severe~~ obstacles in the way of generating interval scales. As an example, consider the fundamental concepts in cognitive psychology: information and information processing. In physics and the computer sciences, these concepts are defined as exact. This is not the case in ~~the field of~~ psychology, where these concepts are interpreted as content, meanings, associations, ways of coding, and hypothetical storage and retrieval (e.g., Palmer and Kimchi, 1986).

Another example concerns the fact that the concepts of psychology are multi-dimensional, and are interpretable from different viewpoints. In contrast, in physics complex concepts are composed of several one-dimensional components. It is hard to break down psychological concepts into their one-dimensional components. Furthermore, in psychology, because the concepts are multi-dimensional, in many cases the transitive relationship is broken (as in the above-mentioned example <sup>concerning</sup> ~~about~~ attraction between individuals).

### Conclusions and Summary

The paper examines several methodological factors, which are suggested as possible answers to the developmental gap between psychology and physics, and it found these answers unsatisfactory. It was observed that the FDUT is a major factor that differentiates



between psychology <sup>from</sup> and physics. One possible explanation of FDUT is UMs-equivalency, which helps bridge the theory-observation gap in physics but not in psychology. Analysis of two alternative answers to the paper's questions, namely reductionism and consciousness, and the generation of interval scales, did not find <sup>these</sup> ~~them~~ appropriate or satisfactory. Hence, UMs-equivalency seems to be the best answer to the paper's inquiries. Furthermore, it seems that UMs-equivalency constitutes the basis for a solution to Wigner's problem. The success of mathematics in describing and explaining nature is based on the fact that what is stated in mathematical language is reflected faithfully in empirical UMs. *to what?*

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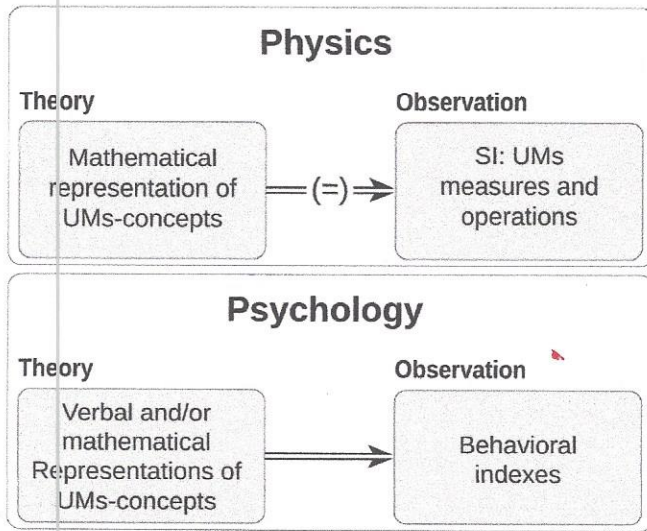
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**Figure 1: Comparison between Physics and Psychology in terms of the Theory-observation Relationship**