**Why does psychology not develop like the natural sciences?**

Sam S. Rakover

Department of Psychology, Haifa University, Haifa, Israel 31905

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Rakover phone number: 972 4 8240963

Email: [rakover@psy.haifa.ac.il](mailto:rakover@psy.haifa.ac.il)

Correspondence should be addressed to Sam S. Rakover, Department of Psychology, Haifa University, Haifa, Israel 31905 (Email: [rakover@psy.haifa.ac.il](mailto:rakover@psy.haifa.ac.il))

**Abstract**

The present article suggests an answer to the following question: why has physics been able to develop unified-theories that provide good and efficient explanations for many natural phenomena, while psychology has failed to develop such theories? The article's answer is based on the following observation: in physics, the units of measurement (UMs) have an expression in theoretical terms that are the equivalent of observational terms (call it "UMs-equivalency"). By 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 a necessary condition. Thus, it does not follow that every physical theory that maintains UMs-equivalency becomes a successful theory (because the theory may not properly represent the processes in reality). The article develops and justifies this idea and suggests that it is hard to imagine a successful psychological unified-theory when UMs-equivalency does not exist.

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

**Why does psychology not develop like the natural sciences?**

**Introduction: an outline of the paper’s main idea .OP 1logy Eng 768/60000/125**

The present article suggests an answer to the following question, which I term the “developmental-gap”: why is there a wide gap between the scientific development of the natural sciences (particularly physics) and the development of psychology? This question is connected directly to the following general problem: can psychology 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 science of physics. Given this, the query that now arises, and that I will discuss later, is as follows: what is the developmental difference between these two disciplines? Briefly, the answer is that while physics has developed general successful unified-theories (e.g., Newton's), psychology has failed in this regard. What is the explanation for this failure?

The 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 (call it "UMs-equivalency"); but in psychology the UMs are expressed only in theoretical terms. (Note 1, 2) For example, in physics the theoretical term “length” has real expression, i.e., the concept of length has an actual procedure of measurement (see a full description below) and measuring devices which use UMs such as millimeter, centimeter, meter, kilometer; by contrast, in psychology, real, empirical, UMs have not been discovered for various concepts such as love, hate, interest, memory, intelligence (see a historic discussion on the subject in Michell, 1999). (Note 3) In physics (and the other natural sciences) a set of international measurement units, the SI (International System of Units) is used, based on the seven fundamental UMs: meter, kilogram, second, ampere, calvin, mol, candela (see SI in references). Of these seven real basic measures new terms of measurement are constructed, such as speed, acceleration, energy. We may add that on these measurement units as a whole rest the theory and practice of physics. The above can be demonstrated in a simple way: it is possible to derive from Galileo’s law of falling bodies a prediction that describes the distance that a body will pass in free fall as a function of the passage of time and to confirm it by conducting the appropriate measurements. In this case it is clear that what is calculated theoretically will be measured empirically, because the theoretical terms of time and distance are equal to these terms in the empirical measurement.

Nothing of its like has yet occurred in psychology, that is, in contrast to physics, in psychology real UMs have not yet been found on which it would be possible to construct psychological unified-theories.

Furthermore, the article suggests that the solution to the problem of the developmental-gap by UMs may also constitute a possible solution to the problem of Wigner (1960), the Nobel laureate for physics in 1963. He ponders how it is possible to understand the enormous success of mathematics in describing/explaining natural phenomena (especially in physics). He writes: "The first point is that the enormous usefulness of mathematics in the natural sciences is something bordering on the mysterious and…there is no rational explanation for it” (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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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 the 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 equal to the UMs in reality (UMs-equivalency) so what is stated theoretically, mathematically, exists also in the observable/measurable physical phenomena. (Note 4) In Figure 1 the broken arrow with the equals sign in the middle indicates two functions. First, the arrow signifies that it is possible to derive from the theory in a certain condition a specific prediction which can be tested by comparing it with the empirical observation (that is, the UMs). Secondly, the equals sign in the middle of the arrow is meant to emphasize the UMs-equivalency, that is, the fact that there is equality between the UMs in theory and the UMs in the actual observation.

By contrast, theories in psychology can be characterized as based on the use of everyday language, where in a number of cases mathematical language is also used. Some of these theories represent the UMs as theoretical terms. For these, there is no real expression in observations: that is, in psychology UMs-equivalency does not exist. (Note 5) For this reason, the non-broken arrow in psychology fulfills only one function: in certain conditions it is possible to derive from psychological theory specific predictions that can be compared to the behavioral observations called “behavioral indexes.” These indexes cover a wide range of behaviors: from responses to the appearance of the stimulus (e.g., reactions, choices, answers to questions or evaluations), speed of response, changes in the electrical resistance of the skin (GSR) and changes in pulse rate, to changes in the blood stream in the brain (fMRI). In some cases it is possible to test empirically also hypotheses about UMs and to see, for example, if the results support the qualities of an interval scale or a ratio scale.

In view of the foregoing it is now possible to present in brief the central argument of the article, which is based on UMs-equivalency, that is, the equality 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 only a necessary condition. From UMs-equivalency it does not follow that any physical theory using UMs turns into a successful theory. In fact, in physics too unsuccessful theories are based on the same building blocks, namely UMs. But 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 to be developed. (In this sense, one may expect that in other disciplines such as sociology unified-theories will not be developed. For example, several researchers in sociology suggested that the weakness in attitude research stemmed from inadequate measurement, e.g., Hauser, 1969; Zeller & Carmines, 1980.)

UMs-equivalency is also the answer to Wigner’s problem: how is it possible to understand the success of mathematics in describing/explaining natural phenomena? (For other solutions to Wigner’s problem see Livio, 2009.) The answer rests on the units of the theoretical computations being the same units of the empirical measurements.

This article attempts to develop and justify the ideas presented above. It is organized in the following way. Section (A) discusses briefly the developmental-gap between psychology and physics, and reaches the conclusion that the gap is generated by the fact that the theoretical situation in psychology can be evaluated as a “failure of developing a unified-theory” (FDUT), i.e., till now psychology has not developed a successful unified theory as those in physics. Section (B) suggests a possible solution to the problem of FDUT, which is centered on the observation that there exists no UMs-equivalency in psychology. Section (C) presents several psychological examples that supports the proposal of UMs-equivalency. Finally, the Discussion focuses on the following issues: supports and justifications of the UMs-equivalency, and its relationships to psycho-reductionism, consciousness, and the generation of interval scales in psychology.

**(A) The developmental-gap between psychology and physics**

The article’s question (and its related queries) has been discussed in the professional literature for many years (see e.g. Lilienfeld, 2010; zittoun, Gillespie & Cornish, 2009). The question has also been discussed in the general literature, and has never left the scientific scene. Suffice it to write on Google the question “Is psychology a science?” to receive many dozens of articles in the professional, popular and blog communications to grasp how relevant and hot the question is at present (see 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 relation between psychology and the position of the general public and the economic policymakers that support psychological research, nor do I deal with outright scientific fraud; I examine briefly only problems deemed to attest that psychology has not developed like the natural sciences. (The discussion is based on the following main studies: Ferguson, 2015; Lilienfeld, 2010, 2012; Pashler & Wagenmakers, 2012; Rakover, 2012; Sanbomastsu & Jonson, 2019; Zittoun, Gillespie & Cornish, 2009.)

*Experimental control:*Due to the enormous complexity of the individual (including animals) the good control that is found in natural science experiments is not possible in psychology. For example, the degree of interest and weariness of the participants in laboratory experiments in psychology may vary greatly. As a result, it is not clear what exactly is being tested in the experiment and what affects the participants’ behavior.

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

*Hidden psychological process*: Most explanations in psychology are based on cognitive-processes which cannot be observed directly. These processes are a kind of theoretical concepts only.

The response to this critique is that even in the natural sciences, in many cases one bases the models on theoretical processes which cannot be observed directly. For example, a way has still not been found to observe directly the force of gravity; and according to Heisenberg’s principle of uncertainty of the sub-atomic level, as the certainty about the place of a particle (e.g., electron) increases, so does the certainty of its momentum decrease, and vice versa.

*Empirical generalization*: Frequently the findings based on a certain sample do not generalize to other samples; moreover, repeated tests are not always able to obtain the same findings even with the same sample of participants. One of the reasons, among others, is that the memory of the first experiment is liable to influence the results of the repeat experiment.

The response to this criticism is that it is possible to resolve this problem by correct planning of the series of experiments, for example, use of groups with different samples (use of a between-subjects design). Furthermore, generalization from sample to sample encounters 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 in various conditions (whether the hypothesis is confirmed or not).

*Confirmation bias*: There is a strong tendency among authors, and among editors of journals, to publish studies whose results are affirmative, supporting the authors’ hypothesis. Results that are not significant usually undergo non-publication (the authors do not submit the articles and the editors eschew reviewing them). The result is that the published literature reflects the interests of the researchers and editors. In addition, complaints are raised that different studies are conducted in a way that leads to significant results statistically (e.g., by enlarging the sample size).

The response to this criticism is the following. First, such a tendency has shown up in physics also. Weinberg (1993), Nobel laureate in physics in 1979, describes in his book *Dreams of a final theory* that the analysis of the results of the empirical test of Einstein’s theory of relativity was influenced by knowledge of the prediction derived from this theory (bending of a light ray that passes near the sun). Secondly, it is possible to overcome this tendency by prior publication of the research’s hypotheses and methods before the research is conducted in practice. (I do not hold that this solution is free of flaws. The chief reason is that the research question requires a large number of preparatory experiments by means of which the researcher constructs and modifies his theoretical/empirical perception, up to the point where he is ready to do the final experiment, clear of all flaws that have been filtered out by the preparatory experiments. The requirement then to publish the hypothesis and methodology in advance may stand in opposition to the natural research process.)

It is to be emphasized that almost all experiments test the researcher’s hypothesis against at least one alternative hypothesis. In other words, the experiment’s results serve to determine among two or more hypotheses. In this respect one may say that to the extent that certain researchers tend to reinforce their favorite hypotheses, others tend to reinforce their favorite alternative hypotheses. It transpires hence that scientific criticism does not lose out.

*The crisis of replication*: In recent years psychology has been beset by the replication crisis. Clearly, the repetition of several studies (especially in social psychology) has not produced 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, which journal will incline to publish an article that shows that hungry rats learn to press on a pedal in order to obtain a food pellet? Or who will wish to submit an article that shows that in her experiment the Muller-Lyer illusion was obtained? Here too, as in the previous problem, it is suggested that the proper treatment is by prior publication of the research’s hypotheses and methods. (However, who will wish to prior-publish the customary methods for obtaining the Muller-Lyer?) Furthermore, certain studies in biology cannot be replicated.

*Complexity*: Sanbonmatsu & Johnson (2019) proposed that in comparison with physics the development of social and behavioral sciences (e.g., psychology) is inferior, because the topics studied by psychology are much more complex than the topics studied by physics.

The response to this criticism is this: although no consensus exists regarding the definition of complexity as applied to science, and although a given area of research becomes comprehensible after a theory has solved successfully most of the problems in that research area, despite all this one can present several simple examples illustrating that physics is a very complex science. First, the concept of mass, which is a basic and essential concept in physics, is highly complicated: its definition in Newton's theory (resistance to applied force) is different from weight, and from mass in relativity theory and in quantum theory. Secondly, the discoveries of many sub-atomic particles and their interactions are so complicated that the Nobel laureate of physics, Weinberg (1993), describes in his book how difficult it is to develop a unified-theory for the sub-atomic area. Finally, to date there is no accepted theory that unifies quantum theory and general relativity theory. Therefore, I may suggest that physics is very complex and its complexity is no less than the complexity of psychology, although their factors of complexity are probably different. While it is very hard for me to indicate the complexity factors in physics (I am not a physicist), I believe that the origin of complexity in psychology stems from the phenomenon of consciousness (e.g., Rakover, 1990, 2018). However, Sanbonmatsu & Johnson (2019) did not discuss this concept.

As may be seen from this short review of criticism of psychology, it is hard to understand why psychology is not considered a science like the natural sciences. First, for all the problems scanned here so far there is a reasonable methodological solution, and secondly, it emerges that to one degree or another even the natural sciences are troubled by similar problems. So is the conclusion that psychology is not like the natural sciences mistaken? I believe it is not.

Compared with physics, from a general historical look at psychology (e.g., Leahey, 2004), from an overview, one may conclude that till now psychology has not developed in any field (e.g., perception, learning, social psychology and developmental psychology) a successful unified-theory similar to the theories in physics: Newton and Einstein’s theories, and quantum theory—theories that have offered acceptable explanations for a host of observations and discoveries. (Note 6). The following two researchers, for example, reached similar conclusions.

In 1986 Paul E. Meehl, clinical psychologist and philosopher of science, wrote an important article that pondered whether there was a connection between basic psychological science and clinical practice. His answer was that there is no integration between these two domains. Also, even an examination of research in basic psychological processes produced difficult problems. In 1973 Allen Newell, a cognitive psychologist and specialist in computer sciences, wrote an article summarizing/criticizing an array of articles presented at a conference on processing visual information. Newell found that every empirical paper presented had the same structure: an interesting phenomenon had been discovered and it was given two contradictory explanations. For example, a distinction between one memory system and a dual; serial or parallel processing; single or multiple coding; decay of memory or interference; innate or learned process; conscious or unconscious process; gradual or one-trial learning; and so on (see Figure 2 in Newell). The problem is that these hypotheses, supported by extremely interesting empirical findings, do not cohere to the point of development of a unified-theory. In other words, Newell suggested that these dual opposing hypotheses do not advance psychology as a science. He maintains that even 30 years hence what we shall obtain is a new collection of articles describing two opposing hypotheses explaining new empirical/cognitive discoveries.

As opposed to the above, some psychologists may propose, for example, that Freud's psychoanalytic theory, Hull's theory of learning or Estes's Stimulus Sampling theory (SST) can be considered psychological unified-theories (e.g., Estes, 1950; Hilgard & Bower, 1966; Marx & Cronan-Hillix, 1987). While these theories were popular in their day, within about two decades they had failed empirically and theoretically. (The great influence of psychoanalysis on literature and on everyday discourse is a different story altogether.) For example, Bower (1994) reviewed SST and found that it encountered many problems when applied to new and complex behaviors, and that currently SST had faded away for several reasons. For example, researchers are interested in new theoretical/empirical questions and the S-R 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 today the cornerstone of the field of animal learning (see Kimble, 1961). Several attempts to propose unified-theories, such as Newell's (1992) SOAR, were developed on the basis of the AI approach to cognition. Although SOAR has made important contributions to understanding cognition, it has received much criticism and has sparked controversies (see e.g. Cooper & Shallice, 1995; Garcia-Marques & Ferreira, 2011; Lewis, 2001). SOAR seems not to have been accepted as a unified-theory for psychology as Newtonian theory has for physics.

On the assumption that the developmental-gap centers on psychology’s inability as yet to develop a successful unified-theory, the question arises as to how may this gap between psychology and the sciences (physics) be explained? The answer suggested 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 equality between theoretical and real UMs (e.g., a ruler to measure distance); in psychology the bridge is based on the use of hypothetical UMs that are indexed by one's behavior. In other words, in physics one develops a theory that uses theoretical UMs that are equal to real UMs; in psychology hypothetical UMs are only part of a theory and are connected to observations by the predictions made from the theory about behavioral indexes.

To clarify this matter, I move on to a very brief description of conducting measurement in physics and in psychology (this summary is based on Coombs, Dawes & Tversky, 1970; Michell, 1999; Tal, 2017). It seems 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. Campbell’s approach holds that 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/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 discussion in Tal, 2017.)

An important point in Campbell's measurement approach, which I wish to emphasize, is that 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 sticks A, B, C:

A |---------------|

B |----------|

C |-----|

To show this, we first define a natural and arbitrary unit of measurement of length by means of the section (-); secondly, we count how many times this unit goes into A (15 times), into B (10 times), and into C (5 times); and finally, we see that indeed 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, because A= B+C.

Given that measurement of the length of a stick or an object by means of a real and arbitrary UM=[(-) or meter] maintains all the mathematical properties of numbers, one may suggest that what is determined by means of numbers is also determined by means of the lengths of the sticks (objects). The same may be said of several 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 the fundamental UMs (length, weight, time) such as speed (distance/time), acceleration, energy etc. (see SI). Some other measurements are based on certain physical laws such as temperature, which is founded on the Ideal Gas Law and on thermal expansion (see Bringmann & Eronen, 2016; Sherry, 2011. (Note 7)

This measurement situation that characterizes physics does not exist in psychology. One cannot find psychological properties (cognitive, mental) that may be measured not by an empirical discovery of the relation between a certain amount of the psychological property and the UM of that property, and not by appealing to an empirical psychological law by means of which it will be possible to measure some mental property. This circumstance caused several researchers and philosophers such as Kant, James and Leibovitch to cast doubt on the possibility of developing a psychological science (see discussion in Algom, 2019b; Marks & Algom, 1998). This is not the position taken by the present article, namely psychology upholds many properties of science but it is not a science like the natural sciences (physics) because of the difference in UMs.

The mathematical psychologist Luce (1972) expressed this 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." (P. 96)

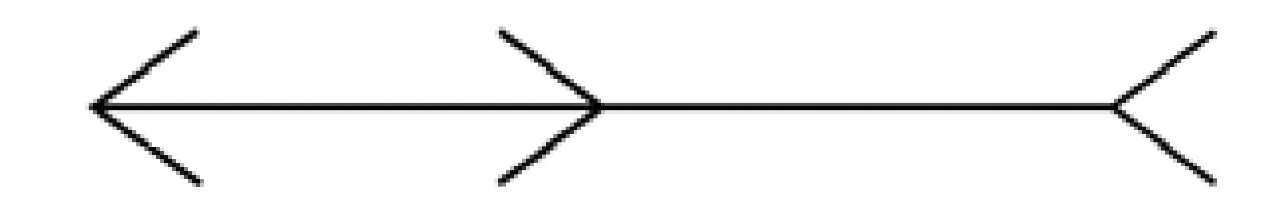
Luce on the one hand agreed that measurement in psychology was not like measurement in physics. On the other hand he suggested a research approach based on the metaphor of perceiving the human being as a measurement tool. By comparison, Stevens (1946), as stated proposed a very broad definition of measurement, based on the attribution of numbers to psychological properties according to a certain rule. This definition was adopted by psychology and opened wide the doorway to the use of numbers and mathematics. The point, as Michell (1999) determined, is that psychologists leapt over or ignored the need to show empirically that the psychological property to which they relate numbers is indeed a quantitative property. That is, a property characterized by an additive structure. He writes:

… 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. (P. 187)

**(C) Examples that substantiate the problem of units of measurement in psychology**

I now move on to demonstrate in several examples the argument that measurement in psychology does not work as it does in physics, and in fact UMs-equivalency that exists in physics does not exist in psychology.

(1) *Illusions*:As an example, consider the famous Müller-Lyer illusion:



The physicist finds that the length of the right-hand line is identical to that of the left-hand line, but everyone else perceives the left-hand line shorter than the right-hand line. The reason is this: the physicist measures the physical properties located outside the cognitive system, but the person who looks at this illusion responds to the right-hand and the left-hand stimuli according to the information processing that is taking place in her perceptual system. (In many respects this is based on the realist approach to measurement: see e.g., Tal, 2017.) 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 and the objective length is considered an index reflecting the size 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, one may propose that with the comparison between the physical and the psychological measurement it is possible to learn that our perceptual system makes mistakes and distorts, which facilitates learning something about the perceptual mechanism.

(2) *Intelligence, IQ*: Over many years, and at enormous expense in empirical and theoretical research, psychology has developed tests for measuring an individual’s intelligence. At the end of the test/measurement process every subject receives a numerical grade attesting to her intelligence level, known as the IQ. Does this grade attest to one's level of intelligence? The following example well substantiates that the IQ numerical grade is extremely problematic.

Consider the following hypothetical example. Assuming that Einstein's intelligence level was very high (IQ = 150), is it 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. One justification for the use of IQ grades is practical: to predict one's success in other tasks (see Coombs, Dawes & Tversky, 1970).

(3) *Consciousness:* Several researchers believe that there is no problem in measuring on an equal-interval scale subjective variables such as attitude, attractiveness, and feelings (e.g., Algom, 2019a,b). I don’t agree. Take for example 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 is broken here; nevertheless, no one will be surprised by this case, just as no one will be astonished by the following results of a soccer game: 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, for conscious experience no unit of measurement has so far been discovered, as it has for length. For example, it seems ludicrous to define a measurement unit of love (UMlove), and say that Jacob loves Rachel 7.5 UMlove 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 UMlove 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 10UMlove while his love for Rachel is 17.5UMlove. Similarly, Michell (1999, p. 88) cites Von Kries: "One cannot explain what it means to say that one pain is exactly 10 times as strong as another." Michell suggested that Von Kries did not realize that the quantity objection is connected to empirical testing (see also Marks & 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, as yet has not succeeded empirically to discover real UMs for conscious behavior. If a complete and satisfactory explanation of consciousness in terms of brain processes were developed, then for consciousness one would probably discover UMs which are based on certain neurophysiological processes. However, as Rakover (2018) has argued, to date there is no accepted theory that explains satisfactorily the relation between mind and body, consciousness and brain (see Discussion).

(4) *The* *"Unit-equivalency” criterion:* This criterion is built on a dimensional analysis. Accordingly, the combination of UMs on one side of the law's or the 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 = 1/2GT2, where S is distance of fall, T is time of fall and G is acceleration caused by Gravitation. Now, since S is measured by the meter unit, the expression GT2 must also be measured by the meter unit. A simple calculation shows that it is: meter = (meter/time2)x(time)2. Does a psychological theory meet this criterion? It does not. 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-equivalency, 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.

(5) *Psychophysics:* It has been suggested that the Just Noticeable Difference (JND) estimated in psychophysical experiments may well be a UM of sensation (e.g., Baird & Noma, 1978; Marks & Algom, 1998; Gescheider, 1997; 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 (ΔI), increased in fixed relation to the intensity of the physical stimulus (I) for a given sensory dimension (an empirical generalization called Weber's law: ΔI/I = Constant). Given Weber's law, Fechner assumed that 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 jnd's, starting at the absolute threshold."

Is JND similar to the UM of length (or weight)? The answer is no for the following reasons: 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, in fact 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 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 paved with assumptions, including the most important one about the subjective JND. Secondly, several empirical results did not accord 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 but changes, an assumption that 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 Libet’s, 1985, famous experiments).

Let us assume 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 ABD was used in a psychophysical experiment. The results of this experiment show 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.

Given these, the following questions can be raised: 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 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., 1inch = 2.54centimeters)

From these five examples (illusion, IQ, consciousness, unit-equivalency and psychophysics) one may reach the following two general conclusions that support the paper's main ideas. First, since psychology has not yet succeeded in discovering real UMs, it is hard to express theoretical concepts such as sensation, perception, intelligence and consciousness in an objective measurable way. All that has been done is development of theories (models, hypotheses) that describe how the cognitive system processes information, and relate 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**

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:*The basic question that the present article seeks to answer is this: why does psychology not develop like a natural science (physics)? The study of this question led to the discovery of the following observation: in psychology unified-theories as in physics have not developed. The explanation of this observation lies in the following distinction: in the natural sciences, such as physics, empirical units of measurement (UMs) were discovered that uphold the requirements, the rules, of the world of numbers on which is founded mathematics, the language of a physical theory. Given this, what is expressed in theory, the theoretical terms based on UMs, is 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 discovered in psychology, that is, UMs-equivalency has not yet been revealed. In psychology, measurement terms are a part of the theory alone, 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.

*Justifications:* In addition to the explanations of psychological measurement, which were discussed above, and 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, 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 of the paper’s questions, which have been discussed above, have been discarded as incongruous except for UMs-equivalency. In addition to these, I examine and disqualify the following two alternatives to UMs-equivalency: psycho-reductionism and consciousness, and the generation of interval scales in psychology.

*Theory efficiency*: One reasonable explanation for the “failure of developing a unified-theory” (FDUT) can be attributed to the fact that psychology has a problem in bridging the theory-observation gap. This gap is bridged in physics by the UMs-equivalency that does not exist in psychology. 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 & 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. This situation, I repeat, does not exist in psychology. Hence, the theory-observation gap in a physical theory is bridged to a much higher degree than in a psychological theory.

Given this, one may raise the following question: why the UMs-equivalency holds in physics but not in psychology? My answer, which will be elaborated later on, is this: given (a) the phenomenon of consciousness yet has not been grasped by the conceptualization of the sciences, and (b) as mentioned above, most of human behavior is saturated with consciousness, it follows that it is very hard 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).

*Reduction*: One may propose that if psychology and particularly 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 yet this research program (reduction) has not been successful (e.g., Rakover, 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 (TR), is reduced to a more basic theory (TB), when TR is derived from TB together with certain ‘bridging laws’, which connect the concepts of these two theories. Usually the bridging laws are perceived 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’ (see Fodor, 1974, 1998). To exemplify this argument, consider the behavioral state of pain. According to the ‘functionalism’ approach [which proposes that a mental state (MS) can be realized by many different material states (e.g., various neurophysiological states (NSs)) that fulfill the MS's causal role in producing behavior], 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 through identity the state of pain and a particular neurophysiological state. If this is the case, a bridging law by means of which it would be possible to reduce psychological theory to neurophysiological theory cannot be found.

A further argument against psycho-neural reduction is based on the requirement of ‘equality of units’ (see Rakover, 2002). I discussed this above and for convenient I will describe it briefly here. It is not possible to discover a psycho-neural law because this law does not fulfill the requirement of equality 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. So 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. Also, response speed is an index that expresses actions of a large number of different cognitive processes (connected to perception, remembering and deciding) which work linearly and in parallel (see discussion in Pachella, 1974).

Furthermore, a meticulous review of the literature regarding the question whether it was 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 deal with a mathematical model from which interval scales can be derived and also be supported empirically: the bisection experiment. In this experiment, a participant hears two tones, (a) high volume, and (b) low volume, and is asked to produce subjectively 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, Dawes & Tversky (1970) 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” (p. 25). The success of building interval scales is of major significance: "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). (The [ … ] is my explanation.)

Relying on the foregoing, the following question can be asked: if it is possible to build in psychology mathematical models that create interval scales theoretically and empirically, what in fact is the difference between psychology and physics? If it transpires that in fact the answer is 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 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 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 riddled with many interpretations – a situation that puts sever 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, but not in psychology: these concepts are interpreted as content, meanings, associations, ways of coding, and hypothetical storage and retrieval (e.g., Palmer & Kimchi, 1986).

Another example concerns the fact that the concepts of psychology are multi-dimensional, and are interpretable from different viewpoints, whereas in physics complex concepts are composed of several one-dimensional components. In contrast to physics, it is very 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 relation is broken: for example, David believes that Ruth is lovelier than Dana because of her green eyes; Dana is lovelier than Alisa because of her raven hair; but he insists that Alisa is lovelier than Ruth because of her long fingers.

*Conclusions and summary*: The paper examined several methodological factors, which were suggested as possible answers to the ‘developmental-gap’ problem and found them unsatisfactory. It was observed that the FDUT is a major factor that differentiates between psychology and physics. One possible explanation of FDUT is UMs-equivalency, which help bridging the theory-observation gap in physics but not in psychology. Analysis of two alternative answers to the paper’s questions: reductionism and consciousness, and the generation of interval scales, did not find them appropriate and good. 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 serves term for term in empirical UMs.

**Notes**

(1) 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 (see Bogen, 2013; Clark & Pavio, 1989; Lambert & Brittan, 1992; Rakover, 1990). Nevertheless, I believe that this distinction is of great importance theoretically and practically, and psychologists use it still today. For example, it is hard to find a psychologist who confuses 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] and suggests that group (I) contains theoretical concepts and group (II) observational concepts; or one who says that there is no difference between the two groups. Clark & Pavio (1989), 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" (p. 510).

(2) I am concentrating here mainly on classical physics because of the following two main reasons. First, from my experience I know that scholars have good knowledge in classical physics but very little in relativity and quantum theories. Furthermore, very few have even mediocre knowledge in neurophysiology and modern research in the brain. Second, the units of measurements, which I 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 here about UMs-equivalency will be exemplified very well by referring to classical physics, which as a matter of fact gave an enormous impetus to research not only to physics but to the natural science at large.

(3) The fact that UMs-equivalency holds for length does not mean that first came the physical theory of length, and then appeared its appropriate measurement. History teaches us that measurements of length (and weight) were made before the mathematical formalization of the measurement theory of length. So what is described in this paper is simply the state of the art of measurement, the approach by which the theoretical term length is equal to the real measurement of this term, the procedure of measuring length (see below).

(4) UMs-equivalency assumes equality between exact theoretical terms (numbers) and inexact empirical terms that depend on procedures of measurement. The discussion of the problems arising from this situation is beyond the purpose of the article (see Sherry, 2011).

(5) One may suggest that psychology too 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, p(r) is estimated empirically by %(r). Nevertheless, attention should be called to the following: the equality between p(r) and %(r) is made only for the dependent variable (the explained variable). There are no UMs for motivation and training, the independent variables, the explainers. For motivation one uses the intuitive indexes of hours of deprivation or incentives, and for training the intuitive indexes are repetitions or reinforcements. By contrast, in physics the equality is between the theoretical, explanatory concepts and the observational, explained concepts, both of which are based on UMs-equivalency.

(6) According to Kuhn's (1970) approach, these three unified-theories have constituted the basis of three different 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 (for a discussion see Rakover, 1990).

(7) Methodologically, Bringmann & Eronen (2016) and Sherry (2011) suggest that it would be worthwhile thinking of the qualitative/quantitative status of the mental terms in analogy with the development of the term temperature: it 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). That is, analogously to temperature, one may expect to transfer a mental term 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 psychology. For example, despite the massive investment in empirical and theoretical research in the term intelligence, one cannot treat measurement of IQ like measurement of length or weight.

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Figure 1: A comparison between physics and psychology in terms of the theory-observation relation.