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REPLY

Quantification, Conceptual Reduction and Theoretical Underdetermination in Psychological Science

Stan B. Klein

Department of Psychological and Brain Sciences, University of California, Santa Barbara

I argue that academic psychology's quest to achieve scientific respectability by reliance on quantification and objectification is deeply flawed. Specifically, psychological theory typically cannot support prognostication beyond the binary opposition of "effect present/effect absent." Accordingly, the "numbers" assigned to experimental results amount to little more than affixing names (e.g., more than and less than) to the members of an ordered sequence of outcomes. This, in conjunction with the conceptual underspecification characterizing the targets of experimental inquiry, is, I contend, a primary reason why psychologists find it difficult to discriminate between competing, explanations of the effects of mind on behavior. Absent well-specified theory capable of enabling precise and detailed quantitative prediction, inferring underlying mental mechanisms from experimental outcomes becomes a difficult, if not impossible, task.

Keywords: science, theoretical underdetermination, consciousness, objectification, quantification

I thank the commentators for their responses to my short target paper (Klein, 2021). Though I take issue with a number of specific arguments made, there is a bigger picture that I think needs to be addressed. Accordingly, I would like to use the electronic soap-box afforded me by my role as author of this target paper to argue that a scientific approach to the study of consciousness is an investigative and theoretical nonstarter.

The essence of my position is that the notion of "psychological science" is a conceptual oxymoron. Specifically, I critique modern psychology's all-to-frequent attempts to effect an objectification and quantification of personal subjectivity (e.g., Klee, 1997; Klein, 2012, 2015a, 2016; Koch, 1999). My question is "what can we learn about experiential reality from indices that, in the service of scientific objectification, transform the qualitative properties of experience into quantitative proxies?"

The focus of my target paper was the mental construct "consciousness." There were a number of reasons for this choice, not the least of which was the importance of the topic for academic psychology—consciousness is the heart of what makes the psychology of human behavior distinct from the biology of human behavior (e.g., Gallagher & Zahavi, 2008; Klein, 2016; Strawson, 2009). However, as I argue below, a conceptually satisfying explication of mental happenings (consciousness, memory, attitudes, plans, desires, and so forth) will not be forthcoming so long as psychology attempts to position itself securely within the confines of a strict scientific analysis (for related views, see Klein, 2012, 2014a, 2016; Koch, 1999; Robinson, 2008; Velden, 2014).

Sadly, this opinion is not shared by most contemporary practitioners. In fact, with the notable exception of Professor Albertazzi, the commentaries on my target paper either explicitly support or indirectly assume that a scientific inquiry is the obvious default position for

Stan B. Klein https://orcid.org/0000-0002-7754-014X
Correspondence concerning this article should be addressed to Stan B. Klein, Department of Psychological and Brain Sciences, University of California, Santa Barbara, 551 Ucen Road, Santa Barbara, CA 93106, United States. Email: klein@psych.ucsb.edu

psychological research. In contrast, I argue that a science of mental states (including, but not limited to, consciousness) is not a default position but rather a logical fault resulting from a failure to appreciate that an approach to understanding mental states that studies first-person phenomena from a third-person (i.e., scientific) perspective should be treated with a healthy amount of skepticism.

I conclude that psychology, as it currently stands, is not a science (where science is conceived as the type of inquiry one finds in disciplines such as physics and chemistry). To the extent that psychology aspires to be a scientific approach to understanding the mind, it, of quantitative and conceptual necessity, falls short of the mark. Put bluntly, the psychological study of consciousness is a specific example of the more general problem that ensues when psychology attempts to treat mental events as entities capable of being subject to scientific analysis (for extended discussions, see Klein, 2014a, 2014b, 2015a, 2016).

The Problem

What accounts for the maladies afflicting a scientific approach to the psychological investigation (concerning not only consciousness, but virtually all hypothesized mental states, entities, mechanisms, and processes)? To cut to the chase, the oft-cited adage "psychology is a science because it uses the scientific method" is a textbook example of conflating the principle of necessity with that of sufficiency. The scientific method clearly is necessary for scientific inquiry. But it hardly is sufficient. Science is more than collecting data according to a particular set of established standards. It also is the principled attempt to fit that data into a logically coherent and theoretically sanctioned conceptual framework (e.g., Margenau, 1950; Torgerson, 1958; Trusted, 1991).

And therein the problem. What is missing from a psychological inquiry that adheres to the tenets of scientific investigation is the organizing presence (and, as I will argue, the possibility) of an overarching, well-specified theory with which the data can achieve conceptual and quantitative/predictive relevance (for a closely related view on the folly of relying on the "authority of method" for the formulation of theoretically warranted, domain-relevant knowledge, see Koch, 1999).

The Nature of Scientific Theory in Broad Brush Strokes

> There are two outstanding characteristics of any scientific theory. First, the theory *explains* observed regularities by relating them to new entities which it educes as existent facts. Second it must be possible to deduce generalizations from the propositions of the theory which can be used to predict new observable facts. (Trusted, 1979, p. 73; emphasis in original)

Science can be viewed as the systematic exploration of relations between abstract theory and empirical evidence (e.g., Hempel, 1965; Klee, 1997; Margenau, 1950; Nagel, 1961; Trusted, 1979). The theory consists in a hypothetical space occupied by constructs (i.e., explanatory variables not directly observable) and their interrelations. The empirical side consists in observable (either directly or indirectly) data. The two are linked via rules that enable the formulation of (typically mathematical) generalizations to explain relations between abstract constructs and observable data (e.g., Klee, 1997; Klein, 2014b; Ladyman, 2002; Margenau, 1950; Trusted, 1979).

The scientific theory serves a descriptive, interpretive, and predictive function: It identifies the essential conditions that enable one to calculate a range of effects, and affords meaning to those outcomes (e.g., Bird, 1998; Klee, 1996; Klein, 2014b, 2019; Trusted, 1979, 1991). I have dealt at length with the conceptual difficulties attending psychological theory (e.g., the conceptual impasse psychology finds itself in by assuming that—in accord with the scientific dictate that everything from molecules to minerals to minds is wholly physical—experiential aspects of reality (e.g., mental phenomena) can be reduced without remainder to material being (e.g., the mechanical aspects of neural transmission). Put differently, the treatment of first-person occurrences from a third-person perspective is something that should be met with a healthy dose of informed skepticism (for discussions, see Klein, 2012, 2013b, 2014a, 2015b, 2016, 2018, 2020).

Accordingly, in the first part of this article, I concentrate on the quantitative/prognostic problems that accompany attempts to situate the psychological offerings within the family of scientific theory. I touch briefly on the conceptual

lacunae attending much of psychological inquiry in the latter part of the article.

Contemporary scientific empiricism adheres closely to two postulates: (a) reality, in its entirety, is composed of material[1] substances and (b) nature is a reflection of the underlying mathematical order of that reality. The idea that mathematical induction is the only scientifically sanctioned route to the truths of nature has roots in Greek antiquity (i.e., the Pythagoreans; e.g., Koestler, 1959). The first postulate—that reality in its entirety is physical—is of more recent vintage, gaining traction with the ascendency of physics as the sine qua non of science in the 17th century (e.g., Reichenbach, 1951). It widely has been taken (though not on logically or empirically defensible grounds; e.g., Elvee, 1992; Klein, 2012, 2014a, 2014b; Koch, 1999; Koons, & Bealer, 2010; Robinson, 2008) as an endorsement of the stipulation that facts about reality expressed outside the vocabulary of (an ideally true) physics can be re-expressed wholly within that vocabulary.

Both postulates are nicely captured by Galileo's well-known dictum that anything not involving the study of the quantifiable properties of material bodies does not deserve to be called science. In short, a scientific theory requires its postulates express propositions that enable both quantification and objectification. The quantification postulate—that is, that nature's intelligibility is subsumable, in its entirety, under mathematically formulable laws—is the focus of the next few sections: Does this dictum capture the practices of psychological theory?

Science and Psychology

Psychology, struggling to free itself of the shackles of natural philosophy (to which it still firmly was tethered in the early part of the 19th century) took pains to position itself as a scientific approach to the study of the mind. The obvious path to scientific respectability was to model one's methodological commitments on the principles embodied by the hard sciences, in particular physics ("All science aspires to be like physics"; Wolpert, 1992, p. 121). Accordingly, the goal of quantification gradually assumed a place of methodological prominence (save for more clinically oriented practice) in psychological inquiry (e.g., Danziger, 2008).

Unfortunately, human experience does not easily submit to quantification (e.g., Michell, 1999; Klein, 2014b, 2016; Robinson, 2008; Uttal, 2008; Velden, 2014). This often is taken as a tacit admission that experience forfeits its status as part of reality. As Stroud (2000) sees it, a goal of science is to separate "reality as it is independently of us from what is in one way or another dependent on us," a difficulty that often "misleads us to what is really there" (p. 4; see also Sellers, 1963). On this view, objectivity trumps subjectivity in endorsing what is real.[2,3]

This is not to say that all forms of quantification of mental happenings are, in principle, impossible. For more than 150 years, certain areas of research in psychology (e.g., psycho-physics) has attested to the fact that the content of intrasubjective experience—via its behavioral manifestations—can be fitted to quantitative analysis, providing descriptions and conclusions that attain an intersubjective consensus. The content of a mental state need not be arbitrary, ambiguous, or inexpressible. The first-person experience is reportable and thus subject *to some degree* of quantification. But—and this is a cardinal point—attempting to maintain the depth and richness of experience by reducing it to mathematical formalism guarantees that something(s) essential will be lost in the process (e.g., Klein, 2012, 2014a, 2015a, 2015b, 2016).

[1] The terms material and physical both are used to refer to the doctrine that everything that exists, exists wholly as matter. While these designations are not exact synonyms, their family resemblance is sufficiently close to allow one to stand proxy for the other.

[2] The doctrine that "reality" is that which distinguishes what "truly is the case" from that which "only appears to be" (a view with origins in Greek antiquity—e.g., Parmenides, Plato) is seen by many as overly restrictive and lacking firm foundation. Although it is beyond the scope of this article to provide an in-depth discussion of arguments questioning the exclusion of "appearance" from the taxonomy of "what is real," comprehensive treatments are readily available (e.g., Eccles, 1994; Elvee, 1992; Klein, 2014a, 2019; Koons & Bealer, 2010; Margenau, 1984; Papa-Grimadli, 1998; Popper, 1994; Shommers, 1994; Swinburne, 2013; Trusted, 1999; Wallace, 2003).

[3] One problem with this view, however, is that an illusion is an experience, and an experience requires an experiencer (e.g., Klein, 2012 Schwerin, 2012; Strawson, 2011). As Meixner (2008) puts it, "The fictionalization of subjects of experience is incoherent, since it involves the incoherent idea that I, for example, am an illusion of myself" (p. 162). Kant (1998) goes even further, arguing that the self of subjective awareness (his transcendental ego) *must* accompany experience (for related views, see James, 1890; Lund, 2005).

In the next section, I address this second postulate of proper scientific inquiry—quantification (attempts to objectify the subjective will be addressed in later sections of this article). To what extent has psychological theory succeeded in framing theory in terms of mathematical formalisms? My conclusions are (a) psychological quantification too often has come at the expense of the phenomena and (b) the meaning of "number" in psychology is incommensurate with the basic principles of scientific quantification (e.g., Michell, 1999; Uttal, 2008; Velden, 2014).

Psychological Theory and Quantification

A scientific theory is, of necessity, tested by a particular set of empirical tasks. Hopefully, those tasks capture the essential components embodied in the theory (e.g., Brunswik, 1947/1956; Klein, 2014b). Changes in theoretically specified factors should lead to predictable, measurable alterations in the experimental outcome. For example, Newton's famous formula $F = MA$ makes very precise predictions (at least within the limits of measurement) about how modifications in the value of one variable impacts those of the others.

But the predictive sensitivity of theory in psychology appears largely restricted to the binary opposition of "phenomenon present or phenomenon absent." As surveyed in Klein (2014b), changes in both central and peripheral experimental contingencies too often lead to the elimination of the predicted outcome rather than to principled change. And where change does occur, the predictive resolution of quantitative analysis is, at best, ordinal (i.e., more than, less than, or equal to). In essence, the predictive scope of most psychological theory is binary opposition rather than a principled variation.

The binary nature of psychological theory is troubling. It calls into serious question the claim that psychological quantification is the methodological equivalent of mathematical formalisms found in the hard sciences (e.g., Klein, 2014b, 2016). Consider the following thought experiment.

Psychological Theory—The Illusion of Scientific Prediction

Imagine a simple study in which an investigator (henceforth, called E) wants to better understand human memory. In particular, she hypothesizes that a new mnemonic technique will affect the mental construct "memory" in such a manner that its utilization will enhance performance on a test of free recall (I chose to illustrate my points by considering an imaginary experiment on one of the most widely researched topics in psychology—memory. Memory, being a far less contentious psychological happening than consciousness, allows me to focus attention on the key points I wish to make without fear that tangential concerns about the target of inquiry will distract the reader from the thrust of my argument. But nothing of importance pivots on this selection. The reader can substitute any number of psychological phenomena, including consciousness, and my arguments remain in force). She teaches the technique to a randomly selected group of 50 participants (the experimental group: G1). To allow assessment of her technique's effectiveness, the second group of 50 randomly selected participants (the control group; G2) are treated identically to G1 with one important change—they are not taught the mnemonic technique.

The experiment commences. E tells G1 and G2: "A list of 20 words consisting in the names of animals (e.g., bird, dog, ape, goat) will appear, one at a time, on the screen before you. Once the words have been shown, there will be a brief delay, after which you will be asked to remember, in any order, as many of the words as you can." On completion, the results reveal that the members of G1 recall, on average, 17 of the presented words, whereas the mean for participants in G2 is 12.

This minimalist scenario captures the essentials of a great many experimental treatments in psychological research: Stimulus information is presented, and participants (assigned to conditions dictated by theoretical and/or methodological considerations) are requested to respond in a manner relevant to the investigation about information presented during the study (e.g., remember the presented items, circle a number on a Likert scale describing your feelings about property X, and so on).

Various methods can be used to analyze a participant's performance (e.g., summing, timing, and qualitative analysis). But most, dictated by an aspiration to achieve scientific respectability, entail the assignment of numbers to each persons' responses (the rules by which numerals are assigned reflect the properties of what is deemed to be the appropriate scale of measurement;

e.g., Stevens, 1946). This numeric assignment is intended to mirror the scientific act of quantification, facilitating mathematical (most often statistical) analyses and, in conjunction with the hypothesis under scrutiny, help the investigator formulate inferences about the psychological phenomena under investigation (i.e., memory).

So, what can E infer about memory from the results of her experiment? Clearly, she is entitled to draw conclusions concerning the *number* of responses produced by G1 and G2: On average, G1 recalled more words than did G2. Further, let's assume that statistical testing revealed that the mean recall difference between groups attained statistical significance (e.g., $p < .001$). Based on her quantitative analysis of performance, E has solid evidence (*if* the findings are replicable and *if* we uncritically accept the units of measurement as valid indices of the phenomena under investigation; e.g., Klein, (2015a) for the efficacy of her mnemonic technique).

But beyond this somewhat trivial fact, what else do our data permit E to say about participants' memories? The answer is "not much." Having reduced G1 and G2's recollections to numeric values, these become E's sole point of entry into participants' memorial experience. And that point of entry leads to an epistemic dead end (e.g., Klein, 2015a).

There are a number of reasons for this pessimistic conclusion. Some—e.g., the conceptual difficulty of attempting a physical reduction of a subjective state—I have dealt with extensively (those interested are referred to Klein, 2012, 2014b, 2015a, 2015b, 2016, 2019, 2020) and will touch on briefly in later sections of this article. In what follows I focus on the problem of drawing inferences about mental entities from data interpretable only in terms of rank order (rank order, or ordinality, refers to the fact that the *only* mathematically sanctioned statements that can be made about objects occupying a common dimension consist in assertions reflecting their sequential relations).

Suppose E's results had been different. For example, suppose G1 remembered an average of 13 words while G2 recalled 7. Assuming other factors of statistical relevance remained approximately the same (e.g., standard deviation, range, skew), she likely would again obtain a statistically significant difference between G1 and G2. And this scenario can be repeated over and over (G1 remembers 15 items and G2 remembers 10; G1 remembers 7 and G2 remembers 2; etc.). In each case, the numeric value of recall is neither predictable from theory *nor relevant to its assessment*. All that matters is that the numbers obtained experimentally support the statistical outcome "effect present."

The fact is that most psychological theories lack the mathematical sophistication required to support predictions beyond that of rank order. This is troubling. Since rank order exhibits few of the properties required for scientific measurement (e.g., Michell, 1999; Uttal, 2008; Velden, 2014) many of the hypothetical entities accorded causal potency by psychological theory turn out, on reflection, to be little more than reified extrapolations from experimental results derived from a numeric façade "almost as completely non-quantitative as is simple verbal naming" (Uttal, 2008, p. 50). In short, "numbers" attained from rank-ordered outcomes amount to little more than appending names (e.g., more than, less than, or equal to) to members of an ordered sequence.

This lack of quantitative sophistication is *one* reason why psychologists find it so difficult to discriminate between theories: Theories whose empirical reach is limited to the prediction of binary oppositions lack the quantitative resolution needed for reasoned distinctions between alternate hypotheses. Accordingly, the mental entities psychologists posit to account for experimental outcomes amount to little more than the names affixed to underlying mechanisms in virtue of theoretical models that are more descriptive than quantitative. Absent well-specified theory capable of enabling detailed and precise quantitative prediction, inferring underlying mental mechanisms from experimental results becomes a difficult, if not impossible, task (e.g., Klein, 2014b, 2015a, 2016, 2018, 2020; Koch, 1999; Koch & Leary, 1992; Phaf, 2020; Uttal, 2008).

The Attempt to Objectify Subjective Phenomena

A common refrain is that science is not science unless it involves objective as well as quantitative treatment of reality. While many great advances have been made by expressing physical reality in terms of mathematically formulable laws (e.g., Hanson, 1958; Klee, 1997;

Ladyman, 2002; Margenau, 1950; Trusted, 1999), many—perhaps most—of the fundamental issues of facing psychology are experiential, not physical (e.g., Fodor, 1974; Gallagher & Zahavi, 2008; Klein, 2014b, 2015a, 2016; Kohler, 1938; Midgley, 2014; Robinson, 2008).

While a quantitative analysis can be useful when it is related to experience, it must *not* be allowed to stand in place of the experience itself: Measurements and equations are supposed to sharpen thinking. But, particularly when positioned as the evidential basis for understanding the workings of the mind, they have a tendency to become the object of scientific inquiry instead of auxiliary tests of crucial inferences. In the determination of the nature of the mind, experience comes first.

As I argue in the next section of this article, attempted reductions of experiential aspects of reality to measurable, material objects and their relations pose serious problems for a science of psychology. Too often the phenomena being reduced are impoverished to such a degree that they no longer bear a clear resemblance to the way in which they were given in and to experience (e.g., Klein, 2012, 2014a, 2014b, 2015a, 2016, 2018). Unless armed in advance with a conceptually coherent (though, as inductive pessimism makes clear, temporally provisional) sense of what our mental constructs consists in, we have no logically sanctioned way to assess whether the data we collect are commensurate with, antithetical to, or independent of the construct being investigated (e.g., Klein, 2014b, 2015b).

A good deal more can and has been said on the conceptual underspecification in contemporary psychology (for my views, see Klein, 2012, 2014a, 2015a, 2016, 2018). But I think enough has been said to show that the confusion and ambiguity afflicting theory in psychological science is attributable, at least in good measure, to the absence of well-specified theories linking physical observables to well-specified abstract constructs (e.g., Danziger, 1997; Klein, 2014b; Margenau, 1950; Torgerson, 1958). To redress this imbalance, practitioners need to pay greater attention to the conceptual clarity of our constructs *prior to* pushing them into investigative service. To do otherwise is to put the methodological cart before the conceptual horse, thereby placing uncertain limits on the extent to which we can trust the conclusions we draw from our empirical efforts to reflect nature as it exists independent of those efforts.

Continuation of Our Memory Thought Experiment

Consider again our hypothetical memory experiment.[4] Assume that two participants (call them P1 and P2) both remembered exactly 12 words. Putting aside the (serious) problems identified with attempted quantification of behavioral outcomes, let's examine the insights our experimenter is licensed to draw about the memory performance of P1 and P2. Well, she certainly is entitled to draw conclusions concerning the number of responses P1 and P2 produced: Their memorial performance can be described as consisting in a "numerical identity." But beyond this somewhat trivial fact, what else do our data permit her to say about participants' memories?

The answer, once again, is "not much." Numeric identity was obtained by reducing the richness of P1 and P2's memorial experience to values on a scale of measurement (presumably in the service of scientific objectification and quantification). But, by transposing the mental events comprising the experience of memory into a quantitative formalism (in this case via the act of counting), she has stripped the subjective experience of all but its numeric properties (e.g., means and measures of variability). And such quantitative evidence sheds, at best, a dim light on the nonnumeric properties of memory experience.

So, to the question: "What does the demonstration of quantitative equivalence sanction with respect to inferences about memory?" the answer is that "recall performance for P1 and P2 was numerically identical with respect to the property number recalled." But this simply describes a quantitative aspect of reality (which may be of interest to a particular hypothesis)—it does not tell us in what way or ways nonnumeric properties of memory experience were the same (or if indeed they were). It, thus, leaves little room for further inference.

For instance, did P1 and P2 recall the same 12 words? Let's assume they did. Did They report those items in the same order? Again, suppose that to be the case. In what manner were those reports realized in experience: As propositions, images, a combination, something else? Assume both

[4] The following section is based on the introductory and concluding remarks in Klein (2015a). For a more detailed account, the reader is referred to that publication.

participants formed images of some of the animals whose names they remembered. Were those images in color or black-and-white? Were the images formed by P1 more or less clear, more or less detailed, more or less complete than those of P2?

Suppose P1 and P2 both recalled via imagery the word "bird." Was the image on which their report was based a robin, sparrow, canary, eagle, penguin? Was it accompanied by associations (personal or nonpersonal; social or a-social) or feelings (positive or negative)? If so, were these accompanying states intrinsic to the reported content (e.g., not just any bird, but my beloved bird Tommy) or just knowable addenda (e.g., birds are related to dinosaurs)? Did these accompaniments enhance or impede (or have no effect on) the actualization of the image in awareness? In short, in what exactly did the conscious grasp of "bird" consist in? All we can say with assurance is that memory experience occurred, and that this occurrence served as the evidential basis for assigning it a numeric label.

These considerations highlight a basic problem with the psychological reduction of experiential states to quantitative formalisms: How can an investigator wishing to understand a person's mental state (e.g., memory, imagery, thought, inference, desire, judgment, and fear) provide an adequate causal account from behavioral reports if those reports make no reference to such states? To conflate (or just ignore the difference between) two clearly distinguishable ways of treating experience (i.e., in terms of their quantitative or qualitative properties) is conceptually counterproductive.

Some might feel we are entitled to say more because the hypothesis under consideration provides the *conceptual* grounding for the numeric identity. But, this assumes that participants' numeric equivalence can be treated as synonymous—when conjoined with a hypothesis—with experiential (or sub-experiential) properties. And this has not been demonstrated. As I hope to have shown, objective data are not of equal epistemic value for all aspects of reality.

Specifically, while material aspects of reality may be profitably interrogated via numeric reduction, application of this technique to the experiential aspects comes at a considerable cost.

Some Final Thoughts

Put bluntly, psychological theory too often lacks both the quantitative and conceptual sophistication to *scientifically* address questions pertaining to mental process and mechanism (e.g., Danziger, 1997; Klein, 2015a, 2016, 2018; Koch, 1999; Phaf, 2020). The predictive and conceptual resolution of most psychological theory is limited to the binary opposition of "effect present/effect absent" of poorly specified mental activities. In consequence, data from experimental research provide an impoverished base from which to discriminate between hypothesized entities of mind. Not surprisingly, this has resulted in a seemingly endless proliferation of "hypothetical concepts, microtheories, and irreconcilable controversies" (Uttal, 2008, p. 155).

The absence of theories capable of permitting computationally rigorous quantitative predictions about well-specified mental entities is a major obstacle to psychology's aspiration to have its offerings treated as scientifically sanctioned knowledge. Lacking the mathematical and conceptual sophistication necessary to transcend the imprecise verbal descriptions of psychological functions and mental mechanisms, practitioners have no scientifically credible method for distinguishing between competing theories (e.g., Koch, 1999; Uttal, 2008). In psychology, experimental evidence more often is in the nature of demonstration than principled theoretic evaluation. In short, until we fully embrace the need for a more critical attitude toward the validity of our measures and the conceptual relevance of our constructs, "psychology's claim to being a science will remain an assertion in need of defense rather than one of unquestioned acceptance" (Klein, 2018, p. 128).

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