

# A POLANYIAN RESPONSE TO “PSYCHOLOGY’S RENAISSANCE”



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## ABSTRACT

*A recent article in the Annual Review of Psychology heralds the arrival of a renaissance in psychology that is improving research practices in the field. The present article evaluates this new epoch in light of Michael Polanyi's thought. While the reforms the renaissance celebrates are invaluable to psychology in its reliance on probabilities for hypothesis testing, they underappreciate the central place of personal judgments in research, portraying them instead and primarily as sources of error that must be curtailed by a narrow range of methods. Valuing the place of personal participation in probability judgments may embolden psychologists to accredit inquiries that more openly rely on discernment to declare truth and are better suited to the I-Thou relations that distinguish human psychology from the study of matter in motion.*

Knowledge that we hold to be true and also vital to us, is made light of, because we cannot account for its acceptance in terms of a critical philosophy. We then feel entitled to continue using that knowledge, even while flattering our sense of intellectual superiority by disparaging it. And we actually go on, firmly relying on this despised knowledge to guide and lend meaning to our more exact enquiries, while pretending that these alone come up to our standards of scientific stringency (*PK*, 354).<sup>1</sup>

The post-critical philosophy of Michael Polanyi resists idealization of rule-governed scientific knowledge to the neglect (and derogation) of personal judgments that tacitly direct our choice of hypotheses, relevant evidence, procedures of observation, etc. Moreover, it counts as misbegotten the notion that truths and falsehoods can be readily distinguished by strict adherence to methods apart from the personal participation of individual researchers. Greater approximation to such rule-bounded science appears to be the objective of what Leif Nelson, Joseph Simmons, and Uri Simonsohn call “Psychology’s Renaissance” (hereafter *PR*) in a recent issue of *Annual Review of Psychology*.<sup>2</sup> The present paper appraises this renaissance in terms of Polanyi’s treatment of probability and his discussion of the I-Thou relation in *PK*. Grounds will be found through this appraisal for wondering about alternatives to the dominant tools in psychologists’ repertoire today, and although these alternatives will only be pointed towards here, one hopes they appear sufficiently congruent with Polanyi’s position on the face of things to justify fuller discussion in the future.

Before proceeding along this path, however, a clarifying note is in order. While psychology’s renaissance is not confined to any particular subfield of the discipline, it is true that social psychology, that specialty concerned with questions of individual motivation, action, and thought in relation to others, has experienced sufficient trouble of late to warrant particular attention. Indeed, Nelson and colleagues identify several “consequential events” (*PR*, 512) that led to psychology’s renaissance, and all of them at least implicate social psychology (*PR*, 513-514). It is for this reason that the present paper will emphasize this domain of psychological science over others and employ the generic label “psychology” throughout to refer to it.

### Is “Psychology’s Renaissance” a Renaissance?

Psychology’s renaissance, according to Nelson and his coauthors, is an awakening and response to dubious yet longstanding research practices in the field that elevate the number of false positives (erroneous claims of statistically significant results) in published studies to unsettling levels. The problem of false positives is fundamentally about the validity of probabilities that psychologists rely on in hypothesis testing—judging, for instance, whether the numerical difference between two conditions of an experiment is the consequence of random fluctuations attributable to sampling procedures or the experimental treatment. An activity the authors call *p*-hacking is the primary cause of the trouble (*PR*, 514-517; the *p* refers to probabilities consulted for decision making in hypothesis testing). When confronted with a dataset that took considerable time and many resources to compile, it is not unusual for researchers to evaluate a hypothesis by taking multiple passes at their data, each time modifying, among other things, the measures they analyze, the observations they include versus

exclude, and the statistical controls they employ to adjust for potential contaminating variables.

The deleterious effects of such practices often go unappreciated even by conscientious researchers, and they were demonstrated powerfully by Simmons, Simonsohn, and Nelson in a 2011 publication.<sup>3</sup> There the authors relied on computer simulations to test the presence of a difference between two randomly selected samples known to come from one-and-the-same population. They examined 15,000 simulations under different conditions of *p*-hacking (e.g., testing two measures separately and then combining them for a third test), and on every occasion a test was statistically significant, they documented the result. Based on the criteria psychologists follow in hypothesis testing, we would expect Simmons and colleagues to have falsely detected a difference between the samples about 5% of the time. But *p*-hacking the analyses in one way or another inflated this percentage considerably (7.7% to 12.6%), and when the researchers combined several distinct forms of *p*-hacking (a not infrequent practice in data analysis), the percentage of false positives soared to 60.7%!<sup>4</sup> This is why *p*-hacking is counted by Nelson and colleagues as “a first-order problem for the validity of psychological research” (*PR*, 514) and “arguably the biggest threat to the validity of published research” (*PR*, 525).

Psychologists are responding to the problem with reforms in research and publication practices that increase researcher accountability. Some, for instance, are making their data publically available, and journal editors are beginning to require full disclosure of study materials, procedures, and analyses from submitting authors (*PR*, 518); other journals are adopting the practice of pre-registration (*PR*, 519). Here all study details are determined as completely as possible in advance of data collection and recorded as public proof of researchers’ intentions, thereby discouraging them from parading free explorations of data as confirmatory tests of *a priori* hypotheses.

Let it be said that the concerns and reforms of psychology’s renaissance are not trivial. A Polanyian perspective does not change this. No doubt, pursuing truth in community requires keeping that community’s house in order, but it is nevertheless reasonable to ask whether the energy behind psychology’s renaissance flows from an epistemology that is disagreeable from a post-critical perspective. Does the renaissance seek impersonal knowledge, knowledge that is strictly formalized and free from reliance on human judgment to discern reality? The ambition sounds outlandish framed in this way, and its exponents certainly do not claim this as their objective. They recognize the inevitability of human participation in science and the complications that follow from this; they see the need for careful reflection on the use of statistics in research and oppose the mindless pursuit of small probabilities that are publishable (*PR*, 529). Nelson et al.’s entire paper, in fact, is predicated on the appreciation that science requires the judgment of scientists, and while this gives the appearance that our

question should be answered in the negative, it neglects what appears, at least to this author, to be the more crucial underlying message: failures in psychological science are primarily attributable to human error, ignorance, or caprice, and successes are realized when researchers commit to getting the science right and conform themselves to proper methods and practices that approach as closely as possible the operations of a purely rational machine. It should be noted, however, that this is not a rebirth; it is the continuation of an aspiration that Wolfgang Kohler judged with some disfavor to characterize American psychology just 70 years ago: “Our main obligation as scientists is that of avoiding mistakes.”<sup>5</sup>

The message comes through, for instance, in a curious statement the authors make concerning the evaluation of failed replications in psychology. They say, “Just as it is impossible to bathe in the same river twice, it is impossible to run the same study twice;” they call this state of affairs an “*unfortunate fact*” (emphasis added; *PR*, 520). Assuming these words were chosen conscientiously by the authors, they warrant notice. The inability to conduct exactly the same study twice could only be called “unfortunate” if there was an alternative that is conceived of as ideal and is wished for. What might it be? A world perhaps where a long line of identically executed studies succeed in revealing the precise conditions under which a range of well-measured outcomes return their predicted values. Such an arrangement would constitute the reliable advance of knowledge that finally stands up on its own and speaks for itself, untarnished by human wiles. Knowing this is not our reality, however, we are left to acknowledge our state of affairs as an “unfortunate fact” and reduce our expectations to an unattainable *ideal* of objective knowledge that survives hard times better (by being unattainable) and encourages our best efforts to come near it with proper techniques—statistical analyses and probabilities among them. But inasmuch as “[m]an has a pathetic need for rest and safety,” we may be tempted again to confuse the ideal with real possibility and to treat statistics and probabilities as the avenue of rescue.<sup>6</sup> Polanyi however shows that even these are reflections of our own ingenuity and reliance on likeminded explorers in pursuing truth, and since the question of probabilities in hypothesis testing is so essential to psychology’s renaissance, considering his evaluation of the subject more carefully is recommended.

### Polanyi on Probability and Order

Polanyi reminds us that “[p]robability statements can never be strictly contradicted by experience” (*PK*, 21). Given a bag labeled to contain 95 white marbles and 5 black, our trust in the accuracy of the label and the theoretical probabilities calculated from it, is not fundamentally challenged when we shake the bag thoroughly and then draw a black marble from it on a single occasion (*PK*, 23). However much we would be surprised by the occurrence, the numerical probability does not rule it out

as impossible. Extending the number of trials offers a better test of the bag's purported contents by bringing the force of accumulated evidence to bear on the question. But even here, Polanyi tells us that the label and associated probability statement may only be "controverted" (i.e., contested), "not contradicted" (*PK*, 22). Should I draw a black marble from the bag five times on 15 attempts (where the selected marble is returned to the bag on each occasion and the contents shaken), it can be demonstrated that the theoretical probability of this event is *astonishingly* small (it should occur only once in approximately 1,400 replications of the 15 trials), but not strictly impossible.<sup>7</sup> However preposterous it may seem to raise the question, we can still ask in this case, "Ought we to count the label as mistaken?" Evaluating the observation against the theoretical probability does not force our hand toward rejecting the label; it leaves the matter to us, the inquirers. But the degree of surprise we feel in relation to the observation and the theoretical probability we calculate as a numerical analog of this feeling offers guidance. "I shall be surprised," Polanyi says, "to a degree corresponding to the reciprocal of this numerical probability. Such is my participation in the event to which a probability statement refers, *and this I regard as the proper meaning of its probability*" (emphasis added; *PK*, 22). And yet, in acknowledging this, Polanyi does not "ascribe subjective meaning to the probability of an event," but "universal validity" (*PK*, 22).

How can this be? A clue to an answer may be found in Polanyi's parenthetical confession, "I am prepared to follow [Sir Ronald Fisher]," the 20<sup>th</sup> century innovator of null hypothesis testing (*PK*, 23). Fisher's method of discerning when an observed result should be counted as genuine or attributable to chance variation is discussed by Polanyi in relation to an experiment of Charles Darwin that compared the heights of self- and cross-fertilized stalks of wheat (*PK*, 22). The average height difference Darwin observed (the cross-fertilized plants were, on average, 20.93 eighths of an inch taller than those that were self-fertilized) was judged by Fisher to be genuine because its probability of occurrence in a distribution assuming no difference at all fell below 5%. It is a question, then, of when we should be struck by an experimental result, and Fisher answers by recommending that when the probability of observed discrepancies are found in a theoretical distribution of mean differences centered on zero (no effect) to be less than .05, we should take notice. Polanyi judges this strategy to put his feelings of surprise in sharper contact with reality, and in so doing he willingly submits to the instruction Fisher provides. His surprise and his interpretation of the probabilities that reflect it, therefore, is not untutored; it is conditioned by Fisher's guidance and also ratified by others who accept Fisher's leadership in this place. This responsible act of following is Polanyi's affiliation with a tradition of inquiry that he believes has commerce with the truth, and in his affiliation, which involves submission to the standards of the guild, his surprise is elevated from the subjective to a personal clue to genuine discovery.

It is in recognition of the personal nature of probability statements that Polanyi goes some length to argue against Gottlob Frege's treatment of language in which declaratory sentences (e.g., "It is raining") are distinguished from statements of assertion ("It is asserted that"). "If language is to denote speech," Polanyi says, "it must reflect the fact that we never say anything that has not a definite impassioned quality" (*PK*, 27). Impersonal assertions of the form, "It is asserted that" are, therefore, no good, and neither are declaratory statements unattached to any human knower ("[It is] no better than an unsigned check; just paper and ink without power and meaning." [*PK*, 29]). Both, for Polanyi, are incomplete symbols—akin, he says, to "a solitary question mark or exclamation mark," (*PK*, 27). Statements of probability are no different. For a person to say in good conscience, "*I believe* [an assertion] the probability of drawing a black marble 5 times out of 15 trials from a bag of 95 white marbles and 5 black is  $7 \times 10^{-4}$  [a probability statement]" is for that person to "set his seal" (*PK*, 29) to the statement; he acts as a responsible human being who has elected to uphold a particular human tradition he believes to have contact with reality. This leads us to conclude that even granting the methodological improvements of psychology's renaissance, a personal commitment remains necessary to make sense of the probabilities it seeks to purify. We must believe them to be true, and once believing, we must decide what to think by their light in a fellowship of likeminded explorers. This entails, as already suggested, receptivity to education by those we accredit as having authority in this domain.

A further observation Polanyi makes in relation to probability and order is also noteworthy. He says that it is "only in view of...orderliness that the question [can] be asked at all whether the orderliness [detected] was accidental or not" (*PK*, 34). When we evaluate the probability of a particular observation, we do so precisely because the outcome in question has struck us, standing out to our eyes against a background of fluctuations that, according to the conception of "events governed by chance," only could have produced what we see "by coincidence" (*PK*, 36). Null hypothesis testing is the method by which we evaluate the observation's likelihood against chance, but the fact of the observation striking us in the first place is a testimony to the trust we place in ourselves—as, it should be added, is the decision to dismiss the observation out-of-hand and not make the test at all. By no means is this to say that our judgments are always right or that they require no scrutiny; it is rather to remind us that behind every test we make of our judgments—even in the context of justification—we *find ourselves* exercising faith in a tradition and deciding in light of it what to test and what to ignore, selecting procedures and the outcomes that count as successes, and the best words/symbols to describe the results. Reality is certainly there for us to investigate—though we should wonder in what sense it ever speaks univocally for itself (*PK*, 265)—and we believe Fisher's strategy offers a valuable guide for adjusting our thoughts about reality in the proper places. But it behooves us to appreciate that with this belief comes the

hazard of making more of the technique than it warrants. So here at the conclusion of this brief review of Polanyi's analysis of probability, let us frankly state what we already know to be true, but rarely say aloud: Null hypothesis testing is a tool with circumscribed usefulness that does not impersonally and precisely distinguish for us truth from falsehood; this distinction we make for ourselves in consultation with a community of others likewise committed to truth whether Fisher's method is adopted or not. No alternative path to knowledge exists.

## Null Hypothesis Testing and Random Sampling

There yet remains a feature of probabilities in psychological research that Nelson, Simmons, and Simohnson appear to pass over completely, and because it provides another occasion for glimpsing the influence of personal judgment in the assessment of probabilities, it is fitting to discuss here. The feature lies slightly upstream from *p*-hacking, but is no less important to the veracity of research. Indeed, for *p*-hacking to pose a danger, it seems that this earlier detail must be addressed first. Consider a classic experiment by Elizabeth Loftus and John Palmer who showed participants footage of a car collision.<sup>8</sup> They asked a subset of participants following the film, "About how fast were the cars going when they *smashed* into each other?" They gave the same question to the remaining participants, but substituted the word "hit" for "smashed." The critical question—pointing to the importance of question wording in interviews—was whether speed estimates reported by participants differed across the two conditions.

In classic null hypothesis testing, this problem is solved by comparing the observed difference between conditions to a theoretical (or null) distribution formed under the assumption of taking all possible random samples from a well-defined population in which no difference in speed estimates exists. Many of the randomly sampled differences will diverge from zero, with some being larger and others smaller than this value, by chance alone. The trick is judging whether the *observed* difference between conditions is sufficiently improbable within the null distribution to reject it as the source of the observed data or, in other words, to rule out chance variability as the reason for the observed difference. Rejecting the null distribution amounts to saying that the difference is not an artifact of random sampling, but is genuine, and Loftus and Palmer reached precisely this conclusion when they reported in their paper that the speed estimates made by participants interrogated with *smashed* as opposed to *hit* were significantly different.

It is, however, well appreciated that psychologists rarely engage in the laborious and expensive enterprise of random sampling (Loftus and Palmer relied on 150 available college students). The interpretation of *p*-values under such circumstances is quite unclear. *What does it mean for a researcher to rule out as an explanation for his results chance fluctuations following from a sampling procedure he did not use?* Educational psychologist

William L. Hays warned students in his critically acclaimed textbook that saw five editions over 40 years in this way: “Inferential methods apply to probability samples, drawn either by simple random sampling or in accordance with some other probability structure. *There is no guarantee of their validity in other circumstances.*” Indeed, Hays goes on to say, “Unless the assumption of random sampling is at least reasonable, the probability results of inferential methods ... *might as well be omitted*” (emphases added).<sup>9</sup> That psychologists report *p*-values despite this, and that psychology’s renaissance is so deeply concerned about their purity even in the absence of probability-based sampling procedures, is a curiosity. One is left to conclude that psychologists dislike the play of personal judgment in their evaluation of evidence any place they find it, except when it is convenient to the work. Treating as inconsequential differences between their available samples and those they might hypothetically have obtained through random sampling seems an example of this. For Polanyi, however, the proper response to the situation is not stricter methods, per se, but to trust the powers of judgment that led to the method in the first place and then continue to direct our steps when the explicit guidance it supplies fails or falls silent. Indeed, I take this to be exactly what psychologists do when they (responsibly) decide to interpret the *p*-values of their research under circumstances where the requisite assumption of random sampling is not satisfied. But granting this brings another possibility to the fore: if null hypothesis testing can be responsibly wielded in the absence of strict random sampling, might it also be possible to responsibly assign this method lesser importance in psychology altogether and for the purpose of encountering whole persons that quantities, experiments, and statistics cannot approach? The significance of answering this question affirmatively grows when the individual psychologists investigate is properly appreciated as a Thou rather than an It, a distinction that Polanyi clarifies in his discussion of logical levels.

### Intimations of a Polanyian Psychology

According to Polanyi, a two-tiered logical structure holds in the scientist’s investigation of inanimate matter: there is the object itself (the first logical level) and the scientist’s knowledge of the object as a sample of quartz, silt or clay, etc. (the second logical level). This is a “*knowledge of things*” (*PK*, 344). It is distinct from our “*reflections on our knowledge of things*” (e.g., “the logic and epistemology of science”), which constitutes a third logical level (*ibid.*). This third level surfaces anytime we think about our thoughts about things *and* when we study living organisms in light of what *they* know—a rat’s mental map of a maze, for instance (*TSOM*, 76).<sup>10</sup> This situation defies the two-tiered logical structure of physical science by involving reflection on *another being’s* knowledge, and it follows from Polanyi’s commitment to personal knowledge and the process of evolution he believes gave rise to the mental powers he recognizes in himself that he accredits to lower organisms primitive manifestations of the same.

Studying the knowledge of these organisms thus entails comprehending what they know, discerning what they intend to do, rendering a judgment about the efficacy of their knowledge, and, perhaps, endowing them with new knowledge through education. It *does not* entail treating them as lifeless objects or mindless machines, as befits firewood and toaster ovens (*PK*, 344-345).

This is a crucial shift for Polanyi, and it grows in importance when inter-human relationships are considered. Here “[t]he I-It situation” characterizing the study of inanimate matter “transform[s] into an I-Thou relation” imbued with an altogether distinct degree of “[m]utuality” that is only prefigured in the study of lower animals (*PK*, 346; *TSOM*, 33). The investigator and the subject of investigation now have the potential for comparable self-understanding and contact with reality. The investigator’s knowledge of the subject in this case “has lost the character of an observation and has become an *encounter* instead” (emphasis added; *TSOM*, 95). This does not mean that the depth of understanding or contact held by the two are always on a par, but it does preserve the potential for this to be so, and also the potential for the subject to surpass the investigator in these areas.

Treating individuals atomistically without any or only the faintest view toward the whole of their mental existence neglects this “mutuality.” Such neglect carries the practical advantage of offering simple facts that can be translated into averages or percentages (e.g., see Loftus and Palmer’s research above), but looked at honestly, such quantities are only *clues to still further investigation*. They lead on to vital shades of meaning in the individual and the circumstances he inhabits. Rather than ends in themselves, they are the earliest beginning of comprehension, and it seems, at least to this author, that a psychology which purports to speak authoritatively about the experiences of persons in their surroundings could do better. Consider, for instance, Stanley Milgram’s famous obedience studies.<sup>11</sup> His participants were fooled into believing they were administering increasingly painful shocks to another innocent person. It is well known that his procedure evoked intense emotional responses from participants. According to one observer, a man who proceeded to administer the strongest shock possible (450 volts) was “rapidly approaching a point of nervous collapse. He constantly pulled on his earlobe, and twisted his hands. At one point he pushed his fist into his forehead and muttered: ‘Oh God, let’s stop it.’” Now is this behavior best interpreted as a simple instance of obedience (which it was)? His acts seem overshadowed by the powerful external signs he presented of internal distress. Suppose, for instance, that years ago the man was the punching bag for an abusive, alcoholic father. The stone-faced appearance of the experimenter churned up memories from the depths, leading him momentarily to revert to the gutted personality of his pre-pubescent self and relive the submissiveness that saved him from his father’s violence. What richness such insight would add to the opaque label “obedient,” but it is avoided. Why?

The psychologist avoids treating his quantities as clues to wholes—as means to more important ends—not just because he wants to use statistics (a misbegotten mark of scientific legitimacy in the human sphere), but also from his fear that bias pervades his personal judgment and the corresponding worry that allowing himself to encounter comprehensive entities who are finally irreducible to their measurable parts would require him to rely on it in research. A question psychologists should grant far greater importance than they do is, “What are the *meanings* of this or that individual’s actions *for him*? And how are the expressions of several or many members of a community properly brought together into a reflection of their shared experiences?”<sup>12</sup> For such questions to be accepted in psychology as worthy of investigation, the answers that follow would have to be trusted as more than merely interpretive or subjective. A full defense of why such trust is warranted, however, would require a recapitulation of *PK* in its entirety, and this, of course, is beyond the scope of the paper.

But it would be mistaken to conclude that what has been argued up to this point is simply that one mode of inquiry can responsibly be substituted for another once the personal coefficient of our knowledge is accepted. This is fundamental, but it does not stop there. In light of the I-Thou relation discussed above, *such substitution is called for* when the goal is genuine understanding of others’ experiences. Accepting this goal as the psychologists’ highest obligation, certain consequences follow, including the repositioning of results acquired by experimentation and quantification to lower rungs of importance as clues (among others) to wholes and the elevation of insights gained by broader inquiries to higher ones. This is what follows from granting the reality of logical levels in psychology, and it seems that accepting this conclusion would lead to important changes in the field. Reliance on experimental methods and inferential statistics would diminish to make room for encounters with the free communications of others. This shift would, in turn, require psychologists’ training to broaden, perhaps to include the case studies of clinical psychology and the ideographic approaches of personality researchers, and when extended to communities or groups, also the ethnographic and participant-observer techniques of anthropologists and journalists. A reoriented psychology would draw upon the insights of these other disciplines and perhaps intermingle with them; and as Sigmund Koch’s piercing observation at the turn of the century makes clear, a reoriented psychology would also have much to gain from the humanities.<sup>13</sup> Just how much is uncertain, but if training in great literature only served to burden psychologists again with the question of human totality, an important service would be done.

Should the above proposal sound too radical, it is worth recalling that Kurt Lewin himself, the accredited founder of experimental social psychology, emphasized individuals’ inner experiences (what he called “the field” or “life space” of the person) as the proper sphere of inquiry for psychologists. “*Objectivity* in psychology,” he said,

“demands representing the field correctly as it exists for the individual in question at that particular time. For this field [the individual’s] friendships, conscious and ‘unconscious’ goals, dreams, ideals, and fears are ... essential.”<sup>14</sup> And, lending credence to Koch’s position above, he even held out Dostoevsky’s work as exemplary, saying that the knowledge he had of his characters is the kind psychologists must strive after with others.<sup>15</sup>

It is also worth considering a proposal Polanyi shared with Carl Rogers in a broadcasted conversation.<sup>16</sup> “If we could only get away from [the word ‘scientific’ for ten years]” he said, “we would see so many possibilities of appreciating knowledge—of appreciating views and explorations” that we might rightly call instead “penetrating, revealing, sensitive, [and] true ... It is quite an obvious way of describing them.” And regarding Rogers’ experience inside therapy (he felt conflicted about its scientific legitimacy), Polanyi expressed “complete confidence in the value of such a pursuit” to the advancement of truth. Whether the experience was quantitative or qualitative, “scientific” or otherwise, did not trouble Polanyi: “It seems to me,” he said, “not a substantial question.” In speaking these words, Polanyi offered to Rogers what the latter observed in therapy with clients: *freedom from tension*. Were such freedom to permeate psychology today, what marvelous possibilities would unfold, and what vibrant inquiries might be permitted to invigorate a genuine rebirth in the field.

## ENDNOTES

<sup>1</sup>Polanyi, M. 1958. *Personal Knowledge* (Chicago: The University of Chicago Press).

<sup>2</sup>Nelson, L., Simmons, J., Simohnson, U. 2018. “Psychology’s Renaissance.” *Annual Review of Psychology*, 69:511–534.

<sup>3</sup>Simmons, J., Nelson, D., Simonsohn, U. 2011. “False-Positive Psychology: Undisclosed Flexibility in Data Collection and Analysis Allows Presenting Anything as Significant.” *Psychological Science*, 22:1359-1366.

<sup>4</sup>See Endnote 3, Table 1 on page 1361.

<sup>5</sup>See page 729 in Kohler, W. 1959. “Gestalt Psychology Today.” *American Psychologist*, 14:727-734.

<sup>6</sup>See page 105 in Kohler, W. 1949 (1992 reissue of 1970 paperback). *Gestalt Psychology* (New York: Liveright Publishing Corporation).

<sup>7</sup> $\binom{N}{r} p^r q^{N-r} = \frac{N!}{r!(N-r)!} p^r q^{N-r}$ , where N = number of trials, r = number of successes, p = probability of success, and q = probability of failure.

<sup>8</sup>Lofthus, E., Palmer, J. 1974. “Reconstruction of Automobile Destruction: An Example of the Interaction Between Language and Memory.” *Journal of Verbal Learning and Verbal Behavior*, 13: 484-489.

<sup>9</sup>See page 227 in Hays, W. 1994. *Statistics*, 5<sup>th</sup> Ed. (Belmont, CA: Wadsworth Group/Thompson Learning).

<sup>10</sup>Polanyi, M. 1959. *The Study of Man* (Chicago: The University of Chicago Press).

<sup>11</sup>Milgram, S. 1963. "Behavioral Study of Obedience." *Journal of Abnormal and Social Psychology*, 67: 371-378.

<sup>12</sup>Is this to throw away the nomothetic tradition of research in psychology? No, but in view of the I-Thou relation, it does make the nomothetic subservient to the ideographic. This is the intended meaning of the phrases "clues to still further investigation" and "clues to wholes" in the main text.

<sup>13</sup>Koch, S. 1993. "Psychology" or "The Psychological Studies." *American Psychologist* 48:902-904.

<sup>14</sup>See page 338 in Lewin, K. 1936. "Behavior and Development at a Function of the Total Situation." In Cartwright, D. (Ed.) 1951. *Field Theory in Social Science: Selected Theoretical Papers*. (Oxford, England: Harpers).

<sup>15</sup>Lewin, K. 1936. *Principles of Topological Psychology* (New York: McGraw-Hill Book Company).

<sup>16</sup>See pages 159 and 175 in Kirschenbaum, H. and Henderson, V. (Eds.) 1989. *Carl Rogers: Dialogues: Conversations with Marin Buber, Paul Tillich, B.F. Skinner, Gregory Bateson, Michael Polanyi, Rollo May, and Others* (Boston: Houghton Mifflin Company). See also Polanyi, M. 1957. Scientific Outlook: Its Sickness and Cure. *Science*, 125:480-484.