In D. Faust (1984) *The limits of scientific reasoning*
(pp. *xi–xxiv*). Minneapolis: University of Minnesota Press.

FOREWORD

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I found this a fascinating, wide ranging, provocative and deeply questioning book which I should think will interest scholars in a variety of domains including scientifically oriented clinical practitioners, cognitive psychologists, psychometricians, statisticians, philosophers and historians of science, and last—but surely not least—academic, government, and foundation administrators concerned with policy questions in the funding of scientific research. I admit that seems a somewhat unlikely heterogeneous readership, but I think all these groups owe themselves an intellectual favor, and the taxpayer due consideration, by reading this book. It also has the effect of refurbishing my somewhat battered image of the "scientist-practitioner" as a training model in the field of clinical psychology, because Dr. Faust writes from the standpoint of a scientifically sophisticated, theoretically oriented *seasoned clinical practitioner*. One may ask whether the author's clinical experience was strictly necessary for his being able to write a book such as this, and I, of course, am not prepared to prove any such counterfactual thesis. But my conversations with him (plus, of course, the point of take off for his reasoning in the book itself, see below) lead me to think that this part of his life experience played a major role, and in any case the empirical fact is that it *was* a practicing clinician interested in cognitive processes rather than a pure academic theoretician who did produce the book.

The chief reason (other than my interest in matters methodological) that I was invited by Dr. Faust and the University Press to write an introduction to his book is that the starting point for his methodological reflections is the controversy concerning the relative efficiency of clinical and statistical prediction. This controversy—which goes back over 40 years of the social-science literature (e.g., Sarbin, Lundberg) and was pushed into prominence, leading to a large number of research studies, by my *Clinical versus Statistical Prediction* (1954)—has suffered the fate of so many controversies in psychology and other social sciences, to wit, that one group of persons considers it to have been settled, a second group continues to resist the massive empirical evidence and think it still controversial, and a sizable number of people don't even seem to know that an issue exists, or that it is of any practical importance. It is safe to say, as Dr. Faust summarizes early on in the book, that the mass and qualitative variety of investigations of the predictive efficiency of subjective impressionistic human judgment, such as that exercised by the individual clinician or case conference or psychiatric team, versus that of even a crude non-optimized mechanical prediction function (equation, monograph, actuarial table) is about as clearly decided in favor of the latter predictive mode as we can ever expect to get in the social sciences. *I am unaware of any other controversial matter in psychology for which the evidence is now so massive and almost 100% consistent in pointing in the same direction*. Psychoclinicians' arguments by analogy from traditional medicine are badly blunted by the finding that diagnosticians in "organic" specialties (e.g., radiology, pathology, internal medicine) show similar cognitive deficiencies in interjudge reliability, weight inconsistency, inferiority to more "objective" data-combination and inference-making methods. That this body of data has had so little effect upon clinical practice reflects on the scientific mental habits of practitioners and the defects of training programs. It is, alas, not unique because it can be paralleled with other examples, such as the continued reliance upon costly skill-demanding projective tests which have been repeatedly shown to have low *absolute* validity and negligible *incremental* validity for purposes of making practical decisions that matter to the welfare of the patient and use of the taxpayer's dollar.

Starting from this somewhat unlikely jump-off point, Dr. Faust generalizes the finding concerning the inefficiency of clinicians' inferential habits and uses of evidence in what I found initially to be a somewhat disconcerting way. Those of us clinicians who attempt to think scientifically and, therefore, pay attention to the body of empirical data bearing on the clinician's cognitive functioning have often—especially if we have a personal commitment to such noble traditions as, in my case, psychoanalysis—analogized processes of complex clinical reconstruction, "the integrated picture of the individual patient's structure and dynamics," to theory construction of the nomothetic sort. That is, while it may be plausible to say that a simple unweighted arithmetical sum of certain test scores or demographic or life-history facts is the most efficient way to forecast subsequent delinquency or survival in engineering school, we have the idea that we do not expect any such mechanical, automated, straightforward "clerical" process to be capable of fathoming the patient's mind in more theoretically interesting respects. So we make a special place for this in our thinking, and I confess that I continue to do this, especially with regard to psychoanalytic interpretation of a moderately complicated dream. But I am aware that these special clinician's hold-outs against objectification of the inferential process might merely be sort of wishful-thinking "last-stand" positions which the actuarial enemy has not as yet approached with grenade and flame thrower! Logicians have pointed out repeatedly that "theories" in the life sciences, and even in certain portions of the inorganic disciplines (e g., historical geology), are not cosmological theories of completely general nature, such as Maxwell's equations or Quantum Mechanics which, as Professor Feyerabend puts it, "claim to say something about everything that happens." So that what appear at first blush to be general scientific theories, thought of as nomothetic rather than idiographic (e.g., the theory of evolution, the theory of continental drift, or Freud's 1895 theory about the specific life-history origins of hysteria versus obsessional neurosis) are apparently nomothetic but when examined closely turn out to be idiographic in character. However soothing these metatheoretical thoughts may be to the hard-pressed psychodynamic clinician (like myself), what does Dr. Faust proceed to do, taking off from the shocking findings regarding the poor cognitive efficiency of clinical inference? Instead of containing that distressing generalization by analogizing *other*, more complicated sorts of clinical inference to scientific theories (with the associated consoling thought that in that case they are in pretty good shape although they might need a little tightening up here and there), he turns the argument on its head, generalizes the statement about low inferential competence, and carries the battle into the field of scientific theory construction itself. This is a clever and daring move on his part which, even if the reader ends up rejecting it, can hardly fail to be of interest to a thoughtful person. He challenges as unproved, as not antecedently very probable, the widely held notion—hardly questioned by most of us, including a somewhat hard-boiled cynic like myself—that run of mine scientific thinking, even by reasonably competent practitioners of the scientist's craft, is anywhere optimal in strategy and tactics. He bolsters this criticism—which, after all, could almost stand on its own feet, once raised—by evidence from recent research by cognitive psychologists into the problem-solving and decision processes, as well as nonexperimental data taken from the history of science. So that analogously to saying after surveying the literature on clinical prediction, "Well, we may be surprised and disappointed by these studies, but, after all, why should we have *expected* the clinician or the clinical team to be a highly efficient information processor and inference maker?" he proceeds by saying, "What grounds are there, theoretical or empirical, for supposing that most scientific reasoning, whether about theories or about the design and interpretation of experiments, is of high quality?"

To enter wholeheartedly and open-mindedly into the ramifications of this development in Dr. Faust's thought, I found it necessary to shed, or at least momentarily set aside, some of the residues of the logical positivism that I learned almost half a century ago. For example, as an undergraduate in the late 1930s I heard from lecturers, and saw repeatedly in articles or textbooks on philosophy of science, that the logician and philosopher of science is not so arrogant as to try to *prescribe* for the scientist how he should go about his business, but that the aim of the philosophy of science discipline is to give a rational reconstruction of scientific knowledge, or even, in the extremely sanitized form held by Carnap, narrowly defined as giving the syntax (and, later, the semantics) of the "language" of science. Now anyone, whether scientist (perhaps likely) or philosopher (hardly likely today), who still holds to that hypermodest view of the philosopher's enterprise will probably be made a little nervous by some of Dr. Faust's text, and especially some of the constructive suggestions for improving scientific reasoning that appear later in the book.

I remember as a student being somewhat puzzled by this becoming modesty of the philosopher in his insistence that he was only describing (or analyzing, or at most "reconstructing") what the scientist does and certainly not laying down any methodological rules or in any way trying to pass judgment upon the scientist. That is, the enterprise is purely *descriptive* (although in a funny sense of that term, being a mixture of empirical and formal subject matter) but not at all *prescriptive*. Now one knows that there are no algorithms in inductive logic that are of such a sort as to be immediately applicable to a quantification of inductive scientific reasoning. Although, of course, there are some for specialized subfields of inductive inference such as Bayes' Theorem, and Fisher's maximum likelihood criterion for choice of a statistical estimator. Nevertheless, it would seem that if we allow ourselves the jurisprudent's distinction between "principles" and "rules," between some admittedly vague guidelines or rules of thumb that one can sometimes fruitfully violate (but only at a certain statistical peril), and some strict constraints that it is irrational, incoherent, or maybe downright senseless to transcend, then it's hard to believe that the result of the philosopher's reconstruction of scientific knowledge would not even contain any such broad principles. After all, philosophers of science and logicians distinguish their enterprise from that of historians, and the latter are equally adamant about maintaining the distinction. All one needs to do is to pick up a book or article written by a historian of science, and then one written by a logician or philosopher of science, to see that while the clarity and sharpness of the distinction has perhaps been exaggerated (for complicated reasons as I would hold), there is certainly a striking difference between the two in respect to both goals and methods. But if philosophy of science as "reconstruction" differs importantly from pure non-philosophical history of science, it is hard to see what *is* its true subject matter, if there are no princlples of rationality or of conceptual clarity, no prescriptions about how to frame definitions and how to detect flaws in suggested concept formations, no way to explain or justify the confirmation of theories, and the like. Faced with this paradox, we may conclude that Carnap and Co. were excessively modest, perhaps because they were great admirers of the scientific edifice and antagonistic to traditional philosophy (being greatly embarrassed by such philosphical bloopers as Hegel's trying to dictate to scientists on metaphysical armchair grounds how many planets there could be, or thinkers deciding philosophically about determinism rather than looking to experimental physics). We conclude tentatively that they should have said forthrightly that to the extent that any kind of rational reconstruction of scientific knowledge can be successfully carried through, it will, when surveyed over a set of advanced scientific disciplines, permit some generalizations not only about *rules* of the game (e.g., one cannot tendentiously select which data to include as evidence, literally ignoring the rest), but also about strategies that *tend* to have good scientific payoff (not "rules" but hints, helps, guidelines, "good advice in general") and, furthermore, that this latter conclusion will not come from the logician's armchair but from reflections upon the history of science itself. But this means that the philosopher of science will, if the discipline succeeds in getting anywhere with its *own* aims, be in a position to criticize the scientist and to give advice. At a less sophisticated level this almost seems obvious, because surely the professional logician would have no hesitation in pointing out a formal fallacy committed by a scientist in a theoretical article; scientists criticize one another for a material fallacy such as the fallacy of Neglected Aspect; or for forgetting the asymmetry between corroboration and falsification; or for ignoring the fact that the alleged dozen replications of a certain experimental result all come from one of two laboratories and the director of the second was a Ph.D. trained at the first; or a statistician points out that a certain estimator is biased, or reminds the scientist that one pays a price for correcting a bias in the form of increased sampling error variance. All these critical, prescriptive, normative comments being common and accepted scientific practice, it seems strange to say that the results of such a scholarly enterprise as philosophy-cum-history of science cannot include helpful advice and at times, at least, a possibility of powerful negative criticism.

We were also taught in my youth that philosophy of science was not empirical and that it was necessary always to be crystal clear in one's thinking about whether one was operating in Reichenbach's "context of discovery" or "context of justification." Looking back today, I find it somewhat strange that we received this so uncritically. I remember a conversation with the late Grover Maxwell in which I was dragging my feet in connection with some aspects of his and Donald Campbell's epistemology, on the grounds that we want epistemology not to be conflated, however slightly, with empirical psychology and sociology of knowledge. Maxwell cured me of that absurd statement by a single question, which I have subsequently come to think of as "Maxwell's Thunderbolt" (which should be added to better known destructive tools such as Occam's Razor and Hume's Guillotine). "Well, Meehl, what epistemological or methodological statements can you derive *from logic alone*?" One immediately sees that such standard old Vienna "rules of the scientific game" as inter-subjective testability are rooted in social and biological generalizations about human brains being connected to sense organs, people having overlapping perceptual fields, most of us who are sane and have corrected vision being able to see pretty much the same things under carefully specified conditions, etc. One thinks of B. F. Skinner's famous answer to Boring as to the intersubjectivity criterion, that whether Robinson Crusoe has developed a good science of ornithology or botany on his desert island depends not upon his being joined by his man Friday (with whom he can have "intersubjective agreement" on some sentences) but upon whether the sentences that Robinson Crusoe speaks to himself enable him to predict what happens and gain scientific control of his subject matter! The obvious point is that in the life sciences there are certain commonsensical theories about the day-to-day persistence of macroscopic bodies, about the limitations of human sense organs, the fallibility of human memory (leading us to record observations at the time) and the like, which do not normally give rise to any deep questions in cognitive psychology but which are presupposed in any epistemological discussion and *a fortiori*, any discussion of that subdivision of applied epistemology we call "methodology of science." It is a result of physiological facts about the human animal, for instance, that we erect physics and chemistry and astronomy upon the deliverances of the eye more than the ear and of the distance receptors more than smell and touch, it being an empirical fact (as N. R. Campbell pointed out in 1920) that essential unanimity of judgments by normal, trained, careful, and honest persons can be reached for judgments of spatial coincidence or betweenness, temporal succession, and number.

I may say here, and I am not sure whether this is a point that Dr. Faust and I would be in agreement about, that I see a certain danger in the current emphasis upon cognitive psychology in the social sciences as bearing upon methodology. As I read the record, the advances in the other sciences where human perceptual distortions or individual differences played an important role did not occur primarily by making corrections for them (despite the famous origin of experimental psychology in the "personal equation" discovered as a result of the disagreement in star transit observations by Maskelyne and Kinnebrook) or, to any great extent, by the perceptual training of the observers (although that is sometimes unavoidable as, say, in pathology), but primarily by replacing the human sense organ and especially the human memory with some other device. Today we see replacement of a human making a measurement by an electronic device making the measurement and what the human eye has delivered to it is a computer printout. I see a certain obscurantist danger lurking in the background for psychologists and—perhaps more for sociologists and anthropologists—in current enthusiasm for the theory-dependence and observer-infection emphasized by Kuhn, Feyerabend, Hanson and Co., because I do not see the history of the more developed sciences reflecting a positive *building in* of the human element, or even *correcting for it*, so much as a systematic *elimination of it* at every stage where it tends appreciably to determine the protocol results.

That brings me to the only other place where I would like to have had more chance to debate with Dr. Faust because I think we disagree somewhat. He accepts happily the current emphasis upon the theory infection of observations, and I do not believe that accepting it to that extent is a necessary part of his thesis, although he might be able to convince me otherwise by more extended discussion. It seems to me that whereas our theories (and I include here such low-order "theories" as predilections for how to slice up the flux of sensory experience into appropriate chunks and the discernment of meaningful recurrences of "more of the same"), whether implicit or explicit, do motivate our bothering to observe at all, will help determine which features of the isolated situation we control (eliminate, hold fixed, or ourselves manipulate to various levels), which events under those controlled conditions we select to pay attention to, which aspects of such selected events we pay attention to, how we break up the properties of the aspects for recording purposes and, if they exist in degrees, what metric we choose, and finally, what kind of mathematical manipulation of these data we engage in—all of these are partly theory-determined and some of them may be completely theory-determined. But it remains the case that *given* the above list having been settled for a particular research study, then what the *results* are, which numbers or which qualitative predicates appear in the protocol, must not be theory-determined, because if it were the argument would be viciously circular and we would not be empiricists at all. The short way of saying that is that if the theory, in addition to determining all the other things I have mentioned, determined the outcome, the *numbers*, or the *predicates* in the protocols themselves, then we could save ourselves a lot of time by not going into the lab or the field or the clinic, since we already know the answer. And, of course, this important point which still defines the empiricist (whatever his philosophy of science may be) must not be conflated with the important point (neglected by most Vienna positivists and by some tough-minded psychologists even today) that there are occasions by which the candidate protocol is properly excluded from the corpus on theoretical grounds. But we must be careful here: The exclusion of a candidate protocol from the corpus because a strongly corroborated theory forbids it is not the same as the theoretical ascertainment of the content of a candidate protocol, this latter purporting to describe observations we made in the laboratory. The widely heard expression, "Theory determines in part how you interpret what you see" badly needs parsing, because it has several interpretations, some of which are correct, some of which are misleading, and others of which are just plain wrong. So that even Otto Neurath's infamous business that caused Bertrand Russell so much distress, that we will admit a candidate protocol if we find we can "fit it in [*eingliedern*]," means that *given* the protocol offered by the investigator, we decide whether to "include it in," meanwhile hoping—but not requiring—that we will be able to explain its occurrence if it is excluded. Neurath surely did not mean here that the observer chooses to record the number "17° C" or the predicate "green hue" either whimsically or on the basis of belief in some theory. What the protocol of an honest reporter (right or wrong, but still honest) *says* he saw or heard must be distinguished from the decision of the scientific community, which may include this observer himself, whether to receive it into the corpus. Whether these worries of mine about the theory infection or theory dependence of observations would be agreeable to Dr. Faust I do not know; but it does not seem to me that any of the main points in the book would require that he disagree with them in the interest of consistency.

Dr. Faust's analysis of sources of judgment errors into analytic categories should be helpful both to psychologists researching cognitive processes in complex situations and to clinical teachers and supervisors trying to teach and train clinicians to reason more effectively. One hopes that clinical readers will take seriously his challenging of what is probably the clinician's most common defense mechanism, faced with such a large body of discouraging data about his or her own performance, which is to say that the unique superiority of the clinical brain is manifested in complex cognitive tasks even if it doesn't show up in simple ones; or, if the basic variables are fairly simple and few in number, in the complexity of their optimal combination in a configural function, a plausible guess about results of future research that I permitted myself in 1954 and attempted unsuccessfully to exemplify in MMPI profile interpretation.

Dr. Faust's hard-hitting discussion of the rather unsatisfactory character of literature reviews I hope will be taken with the seriousness it deserves. My prediction is that it will not, because in the behavioral sciences (especially in the soft areas like clinical, personality, social, and counseling psychology) the relatively weak state of theories and the modest reliability and validity of measuring instruments makes it difficult—in many sectors impossible—to generate numerical point predictions, or narrow-range predictions, or even predictions of the mathematical function form with parameters to be filled in. Hence theory testing in these fields is frequently restricted to performing of directional statistical significance tests. The almost exclusive reliance upon establishment of a non-null difference between two groups, or a non-zero correlation between two variables, makes the usual literature summary well nigh uninterpretable by a sophisticated reader. I realize this is a strong statement—stronger than Dr. Faust makes, although he comes mighty close to it—and I am preparing a scholarly paper that will I hope prove the point. When one is forced to rely upon scanning a heap of significance tests, some of which pan out and others not, there are six nuisance factors working that are *variable*, *countervailing*, of *unknown* but *non-negligible magnitude*. These six factors are: Lack of strict deducibility of predictions from the theory, problematic character of unavoidable auxiliary hypotheses, inadequate statistical power, influence of the ubiquitous "crud factor" (in social science everything is correlated with everything and the null hypothesis taken literally is always false), bias in submission of reports in favor of those that come out significant, and editorial preference for acceptance of significant results because of Fisher's point that the null-hypothesis can be refuted but cannot be proved. The first three of these tend to work against successful corroboration of a good theory; the latter three work in favor of pseudo-corroboration of theories having negligible verisimilitude. Since one does not know in a particular domain the relative magnitudes of these six opposed forces, it is usually not possible to interpret the box score of a heap of significance tests in the soft areas of psychology.

The author suggests that one decision aid that might extend the scientist's capacities to make judgments would be to use actuarial methods to evaluate theories. This suggestion will come as a shock to those conventional psychologists who suffer the delusion that this is already being done, a statistical significance test being (in its way) an "actuarial" method. But that, of course, is not at all what Faust is talking about here. He is talking about a more souped-up kind of actuarial thinking in which preferred theories are found actuarially, that is, by the study of the history of science in various domains, to possess certain signs of parameters. (I hope I do not misunderstand his intentions here, since the discussion of this point in the book is quite compact.) This notion revives an idea put forth almost a half-century ago by Hans Reichenbach in his *Experience and Prediction* (1938). Reichenbach attempted to defend his "identity theory" of probability, that all probabilities are really relative frequencies, including those "logical" probabilities represented in the concept of degree of confirmation or degree of evidentiary support, considered by Keynes and Carnap (and by most scholars today) to be fundamentally different from the notion of relative frequency. When we find ourselves critically examining the internal structure of a theory in relationship to its evidentiary support, it certainly does not *look* as though we were doing any kind of frequency counting. But Reichenbach's idea—which he did not spell out in sufficient detail to know whether it makes sense—was that various features of that relationship, and various internal properties of the theories, are empirically correlated with long-term success ratios. I daresay most philosophers and historians of science will drag their feet about Dr. Faust's suggestion that we could apply actuarial procedures to the life history of a major theory over a period of time, but once you get used to the idea it seems reasonable and probably do-able. I have often thought that even competent historians of science, or philosophers of science relying heavily upon history of science arguments in support of their methodological proposals, are not sufficiently conscious of the extent to which such history-based arguments are inherently *statistical* in nature, because they involve a sampling of episodes. Controversies about falsifiability and theory-based exclusion, ad hocness, and incommensurability, paradigm shifts which continue to rage among philosophers of science are replete with historical examples showing either that somebody did stick profitably to a degenerating research program, or abandoned it wisely, that somebody accepted what appeared to be *prima facie* a definitive falsifier and somebody else (as it turns out wisely, after the fact) did not, and the like; one has the impression that the authors do not fully realize that this kind of debate is difficult to move unless some sort of sensible claim can be made about the manner in which the theories, investigators, and episodes were selected. You can prove almost anything by historical examples if allowed to choose them tendentiously.

The most shocking suggestion of all, and one which, in addition to its novelty, is deeply threatening to the scientist's self-image, is the notion of using a computer to invent theories. While there is no algorithm for theory construction comparable to those for testing the validity of a complex statement in the propositional calculus (which computers have been able to do for many years) now that we have reached the stage where computers can play grand-master-level chess—and there are recorded instances of grand masters being defeated by computers—it is surely rash for the scientist to say that no kind of algorithm for concocting theories could ever be built. That's a fascinating possibility, and one can speculate about kinds of meta-principles to embody in the program. Example: We say there are four basic kinds of entities in the world (structures, events, states, dispositions), and there is a quite limited number of antecedently plausible ways in which these four kinds of entities can be put together in a theoretical postulate set. We set the computer to work trying them out vis-a-vis a fact collection. This is music of the future, but it is to be hoped that readers will not dismiss Faust's thoughts along these lines.

I would have liked to see some discussion of the extent to which garden-variety mistakes of reasoning can be reduced in frequency by systematic "inoculation" instruction at the graduate level and to what degree such instruction has to be tied to a particular subject-matter domain. For example, reports of the brighter, more scientifically oriented clinical psychology students about their internship experiences convince me that many clinical supervisors functioning in those clinical installations do not grasp the importance of Bayes' Theorem in clinical decision making and information collecting, despite the fact that a clear, vigorous, and widely cited presentation of this methodological point was made by Albert Rosen and myself 30 years ago and had been fairly clearly made by psychologists in the area of personnel psychology a half-century ago! It turns out that we need not invoke any interesting and complicated explanations of this social phenomenon, because the fact is that the reason clinicians don't think that way is that nobody ever told them about it or assigned it in their reading when they were in graduate school. As an undergraduate I was exposed to one of the "greats" in applied psychology, Professor Donald G. Paterson, in a course in differential psychology which relied on the textbook for certain important but not thrilling factual matters, so that Paterson could devote class time to critical examination of empirical studies that purported to show something that they didn't show because they were methodologically defective. Taking studies apart piece by piece and bone by bone, with the constant reappearance of certain bloopers (e.g., failure to control for selective migration, spurious index correlation, subtle contamination of ratings, insufficient statistical power, failing to consider effects on correlation of restricted range, use of an inappropriate descriptive statistic, picking out a few significant differences from a much larger batch of comparisons initially made, the assumption that a variable like social class is always an input causal variable and never an output variable itself influenced by genetic factors of intelligence or temperament)—this reiteration of certain basic methodological warnings in a diversity of empirical contexts is analogous to working through in psychoanalysis. I am willing to conjecture for falsification that there is no substitute for this kind of pedagogy if you want to teach psychology students to think straight about what research proves or doesn't prove. When I served for five years on the American Board of Professional Psychology, examining clinicians who had received their doctorates from accredited schools, I was aghast at how many of them had never had such course and (as a result?) had a naive approach to what was known and what had been proved. I would be curious to know whether Dr. Faust thinks that this is an over-optimistic view of the impact of formal classroom instruction. Undergraduate courses in general logic might be thought beneficial in that respect, and I do not have any hard data as to whether they are, but my anecdotal impression is that, for some reason not clear to me, such courses often do not "take" to the degree that one might hope. Is this because of their generality? After all, the various formal and material fallacies found in logic textbooks are usually exemplified so as to cover a wide range of content which would, if it fails pedagogically, look like my conjecture is falsified.

To conclude on an optimistic note, Dr. Faust and I and the reviewer and publisher must have faith that the right sort of verbal instruction can sometimes improve scholars' problem-solving practices, else why have we thought it good to publish this book?

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