

Review

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DO WE REALLY KNOW WHAT THIS THING CALLED PSYCHOTHERAPY IS?

Reflections on Przemysław Bąbel's "The War over Psychotherapy: Between Science, Practice, Business, and Politics"¹

Przemysław Bąbel's book (hereafter PB) poses a particular challenge as a point of departure for scholarly debate because it presents, in essence, a coherent body of interconnected views with which one may either agree or disagree, but which cannot easily be discussed except by offering an equally comprehensive and carefully thought-out alternative – something that is simply impossible within the space limitations imposed by a journal article. Such an alternative vision would, for obvious reasons, have to be comparable in scope. At the same time, *The War over Psychotherapy* is unquestionably a book that stimulates both reflection and discussion, for it has the character of a kind of manifesto – that is, a genre in which both the criticism of opposing views and the author's own proposals are expressed in particularly forceful language and presented in a sharpened, at times even deliberately exaggerated, form. Here the author clearly follows in the footsteps of his masters, the arch-behaviorists John B. Watson and Burrhus F. Skinner, whose rhetorical strategy might be summed up as a fondness for "stirring up a hornets' nest." Yet this has its advantages. It allows PB's book to function as something akin to a *target book* – that is, a book-length counterpart of what scholarly journals call a *target article*. As is well known, a target article is a major theoretical or review paper devoted to an important, and often controversial, issue in a given field, which subsequently becomes the subject of an open scholarly discussion. I therefore intend to take advantage of precisely such an invitation to debate.

At the outset, I must confess that I have no experience as a practicing psychotherapist. My reflections on PB's book will therefore be offered primarily from the perspective of

¹⁾ This article is based on a book review of Przemysław Bąbel's *The War over Psychotherapy: Between Science, Practice, Business, and Politics*, originally written for PWN Scientific Publishers. The present version has been revised, slightly expanded, and adapted to meet the editorial standards of *Psychoterapia*. While its emphasis has been adjusted, additional reflections have been incorporated, and the discussion has been updated to reflect the book's final published version, its central argument remains essentially unchanged. I decided to publish this text primarily because it demonstrates that my assessment of Bąbel's book is considerably more nuanced than might be inferred from the endorsement I provided for its cover.

someone who has spent many years teaching general scientific methodology together with the philosophy of science, which is of fundamental importance in the present context. For the central axis of the dispute over psychotherapy concerns the question of which of its schools and approaches may legitimately be regarded as scientific (*evidence-based*, as the term goes today), and thus permitted to practice publicly under the name of psychotherapy, and which may not, together with all the practical consequences that follow. This means that the disagreement is not merely framed by, but is itself fundamentally about, a more basic question: what counts as science, what does not, and who has the authority to decide.

I shall begin my reflections with a secondary issue, one that nevertheless immediately catches the eye and is, in a sense, rather puzzling. PB's book is unusual, at least for a publication of an essentially scholarly nature, in that it consists of two interwoven narrative strands. The first—and principal—takes the form of a scholarly discourse, to which I shall return in a moment, conducted at a high yet accessible academic level and devoted to the titular “war over psychotherapy.” The second consists of a collection of essays on the paintings of Caravaggio—essays that are, it should be emphasized, deeply personal, while at the same time revealing the author's considerable expertise in a field that is in some respects rather specialized (being devoted to a particular painter of the distant past) and far removed from his primary profession. This compositional device admits of several possible interpretations (I found no explanation in the book of why the author chose to structure it in this way). As I read it, however, three charitable explanations for this somewhat unexpected parallelism between art and science came to mind.

First, we undoubtedly learn that the author is a person deeply sensitive to art, and very much so, which immediately runs counter to the familiar stereotype of psychologists from the “objective” tradition, who, ever since the days of Watson and Skinner, have readily been portrayed as “behavioral engineers,” obsessed with control and at the same time suffering from complete blindness to the spiritual (and aesthetic) dimensions of human existence. Perhaps, then, this serves as a kind of *caveat*: critics cannot invoke the argument of the “soulless technocrat” here. Second, the fact that the arguments developed in the chapters on Caravaggio do not affect any part of the remaining analyses and reflections may suggest that the author adopts the following position: when we speak about art, we speak about art and aesthetics; when we speak about psychotherapy, we speak about science and practice. Since many maintain that the psychotherapy at the center of the book's titular conflict is also an art, this structural device seems intended to convey one simple message: I know something about art (as these brief essays clearly demonstrate), and, according to my understanding of its nature, psychotherapy is not an art and, moreover, by its very nature cannot be one (except, perhaps, in the metaphorical sense of such expressions as the “art of medicine” or the “art of war”). I have also encountered the view that such a procedure has no rational explanation, is essentially incomprehensible and unnecessary. However, this seems a bit exaggerated to me – and here is where a third interpretation comes in. The sections devoted to the Italian genius can be treated as necessary moments of respite between subsequent chapters, whose atmosphere is truly dense.

Why? Simply because the rest of the book has the character of a kind of crusade, one that might be described – borrowing from the title of Michel Houellebecq's debut novel – as not only an “extension,” but also a “clearing of the domain of struggle”. For

the overriding aim of the consistently developed argument is to determine, as precisely as possible, the very essence of psychotherapy. The author derives this from a particular understanding of what it means to practice psychology scientifically, which in turn rests upon a particular understanding of “what is this thing called science?” (a phrase I borrow, in turn, from the title of Alan F. Chalmers’s well-known book). In other words, the questions are: what psychotherapy is, how it should properly be practiced, and what benefits we may expect from practicing it in that way. The last of these questions is especially important because the book’s argument is developed against the backdrop of the current, and highly contentious, debate over the professional status of psychotherapists – that is, over who may legitimately claim that title, under what circumstances, and after satisfying which requirements. As already noted, the dispute is highly contentious, though even that is something of a euphemism. Much has already been said in the course of it – perhaps even too much – especially things that were unnecessary and, indeed, harmful. In this context, PB’s book is “clarifying” because, from the standpoint of a clearly defined epistemological position, it shows which issues should be given priority in the debate over psychotherapy, and why, and which should be relegated to the background or removed from the discussion altogether. It is also “expansive,” because it addresses questions of finance and the interests of particular interest groups, issues that in debates of this kind are usually ignored, given insufficient attention, or, worse still, carefully concealed. Those who believe that scientific psychology as practiced at universities (together with its practical extension in the form of CBT) is the only legitimate way of pursuing psychology’s aims, both in theory and in therapeutic intervention, will find substantial support for their position in PB’s arguments. Those who hold a different view, however, are equally free to engage the author in debate; although they face a very difficult task, they are by no means defenseless.

For my own part, I shall draw upon Jürgen Habermas’s well-known classification of human interests and the sciences that serve them into empirical-analytic, historical-hermeneutic, and critical.² Thomas Teo subsequently simplified this classification and adapted it to the needs of psychology by distinguishing three corresponding domains within the discipline, which he termed *scientia*, *cultura*, and *critica*.³ Viewed from this perspective, PB’s book clearly represents the first of these positions; the other two domains are either absent from it or addressed only to a very limited extent. The author is, of course, fully entitled to make such a choice, for there is nothing objectionable about it; indeed, it is simply one of the available options (within contemporary academic practice, it might even be described as the default one). In a debate as highly charged as the present one, however, it is worth bearing in mind that *cultura* and *critica* also have an important role to play. Whether they necessarily had to be incorporated into this particular book is, of course, open to discussion, since its primary purpose is to provide an accessible introduction to the current state of affairs for all interested readers, including patients. Its aims are therefore, to some extent, immediate and practical. Nevertheless, I believe that in any serious debate on the status of psychotherapy (and of psychology as a science), the issues discussed below should certainly be taken up within professional circles.

²) Habermas J. Knowledge and human interests. Boston: Beacon Press; 1971.

³) Teo T. Functions of knowledge in psychology. In: New Ideas in Psychology, 1999; 17: 1–15.

Let us begin, then, with those aspects of PB's book that may be discussed under the heading of *critica*. As already noted, the author's position – which enjoys considerable support within academic circles – is not easy to challenge, because it begins from a particular set of assumptions and then proceeds consistently through successive intermediate steps to its conclusions and final recommendations. In other words, PB analyzes what is laid down in the law, in influential academic publications, and in the positions adopted by professional bodies, in order to draw the logical conclusions that follow from them. If we accept these premises, it is, of course, difficult to disagree with the author's findings, even when they may be uncomfortable. *Critica*, however, is concerned with more fundamental questions. It encompasses what might be called the metatheoretical level – that is, a field of inquiry in which there is room to question established assumptions, prevailing opinions, accepted practices, and the like.

Let us pause for a moment, then, and allow ourselves a brief speculative digression. To begin with, PB starts from the assumption (let us repeat once again that this assumption is supported by the relevant legal regulations, the positions of professional bodies, and definitions found in respected scholarly publications, all of them written from the perspective of a particular paradigm) that psychotherapy is a procedure of a strictly medical (or therapeutic) nature and, as such, belongs within the broader model of interventions of precisely this kind, and no other – a view that has certain consequences. It therefore concerns individuals who must first undergo diagnosis, much as they do within the framework of so-called somatic medicine, since one cannot cure an illness that has not first been properly identified. Once the diagnosis has been established, the task is to implement empirically tested methods and practices with a demonstrable level of effectiveness, that is, those psychological interventions that constitute the optimal therapeutic means for a given nosological entity or disorder.

This most fundamental position is reflected in the author's language. Throughout the book, one repeatedly encounters such expressions as “evidence-based treatment” (EBT), “empirically supported treatment” (EST), and “evidence-based practice” (EBP), all of which, of course, are modeled on the familiar formula of EBM (“evidence-based medicine”). At a somewhat higher level, this position is associated with a particular conception of science that is rather rigorous, in the sense that it imposes specific theoretical and practical requirements on those entering psychiatry, psychology, and psychotherapy, including the completion of prescribed programs of study and professional training, as well as the acquisition of particular competencies. As a result, those who fail to meet these requirements are situated outside the boundaries thus defined, which means that they cannot, for example, practice as psychotherapists or offer psychotherapeutic services, because they are not legally entitled to do so. Such is the view from the position of the “insider.” What reservations might one raise at this point? Well, for example, those familiar from the history of science, the philosophy of science, and the broader field of science studies.

Let us begin with the linguistic issues just mentioned, for they quickly lead us into broader waters. I am well aware that the expression “scientifically proven methods” – together with its various related formulations – is in widespread use. Nevertheless, it seems to me at the very least controversial, and for several reasons. First, apart from its colloquial and legal use (as, for example, in the distinction between direct and circumstantial evidence), proofs, strictly speaking, exist only in the formal sciences, such as mathematics and logic. Whatever has been proved within those disciplines is regarded as true once and for all, beyond the possibility

of appeal, provided, of course, that the proof itself is valid and the axioms on which it rests remain unchanged. The Pythagorean theorem, which in Euclidean geometry states that, in a right triangle, the sum of the squares of the lengths of the two legs is equal to the square of the length of the hypotenuse, has been proved conclusively and admits of no dispute on that point.

This gives rise to the intriguing question of whether anything can likewise be proved in the empirical sciences and thus settled once and for all. That would be an extraordinarily bold claim, especially in light of Karl Raimund Popper's famous remark: "The game of science is, in principle, without end. He who decides one day that scientific statements do not call for any further test, and that they can be regarded as finally verified, retires from the game."⁴ PB naturally agrees that the conclusions and findings we reach should remain subject to continual further testing, which is valuable in itself and may well lead to the improvement of the therapeutic tools currently at our disposal. He does not, however, discuss at any greater length whether the empirical sciences are cumulative in character, as the positivists maintained, or paradigmatic, as argued by the socio-historical school of science studies. Yet if science is understood precisely as being "evidence-based," this would seem to be the most fundamental question of all.

The cumulative model of science regards scientific development as a process of successive additions to previously acquired knowledge, increasing what is already known in an additive manner, that is, without requiring any upheavals or changes of a revolutionary kind. The paradigmatic conception of science, by contrast, assumes that alongside periods in which science is practiced in the so-called "normal" mode, there are also moments (as we know from the history of science) when it enters a revolutionary phase. This occurs when so-called anomalies begin to accumulate – that is, phenomena, processes, or observations that conflict with the predictions and assumptions of the prevailing paradigm. There is no need to embark here upon an extensive discussion of these matters, for anyone who consciously practices science knows what is at stake and has read the works of Thomas Kuhn (and perhaps even those of Ludwik Fleck). One point, however, should be stated clearly: even if we provisionally accept the notion of "scientific proof," we must not forget that such proof is always obtained within procedures developed and recognized within a particular school, tradition, or methodological approach and is therefore established, as it were, from within a paradigm shared by a given community of researchers.

The author tacitly assumes that a form of objectivity is possible that transcends these limitations, that is, that there exist indisputable facts which, in the empirical sciences, are established through observation and subsequently expressed in so-called "protocol sentences" (or simply "observation statements"). These then serve as the basis of procedures of justification and thus acquire the status of the repeatedly invoked "evidence." According to this view, I am able, by means of my senses (or measuring instruments), to apprehend some fragment of reality in an indisputable manner and, on that basis, to state (to take a simple example) that a horse is galloping, trotting, walking, or cantering. Yet this is only apparently self-evident. I am immediately reminded of an anecdote from the life of Alfred Korzybski, the renowned Polish-American logician and philosopher and the founder of General Semantics, who, during one of his lectures, would raise his hand with the fingers clenched and ask his

⁴ Popper KR. *The Logic of Scientific Discovery*. London: Routledge; 2002: p. 32.

students what they saw. When they answered that it was a fist, he would then open his hand and ask them what had happened to the fist. This example succinctly illustrates that the way in which we individuate objects and processes and distinguish them from the rest of reality through language not only pre-structures our cognition but also misleads us, sometimes to a considerable degree, since we may unwittingly (and often unjustifiably) take to be stable and unchanging objects (“the fist”) entities that are nothing of the sort.

Yet an even better example comes to mind. Some time ago, a well-known professor of physics, who frequently appears in the media, was inveighing against humanists, postmodernists, and other varieties of what he regarded as cognitive nihilists, whom he held responsible for the lamentable political preferences of the Polish public and for the epidemic of post-truth. He argued that it was precisely because of their highly pernicious activities – that is, those of the postmodernists – that a deeply harmful epistemic relativism had spread throughout society, according to which it makes no difference whether a given claim is advanced by a politician, a scientist, or a social media influencer: it is treated merely as one possible narrative among many, a private opinion no more authoritative than any other, and therefore enjoying no privileged status whatsoever. Growing increasingly exasperated, the professor then turned to the interviewer in what was clearly intended as a striking lesson: “Is it not obvious to every reasonable person that indisputable facts exist? Surely no one can deny,” he continued, “that if I take this laptop standing before us and push it far enough toward the edge of the desk, it will eventually fall onto the floor. That is an objective fact, one that cannot be disputed, still less dismissed as merely a private opinion – isn’t it?” Watching this exchange, I, as one of those supposedly “sold-out humanists,” quickly came to the conclusion that the professor’s claim could, in fact, be challenged rather easily – for those who live by the sword die by the sword. One might simply maintain that, even if this operation were carried out in the “real world,” the laptop would not, under any circumstances, fall to the floor. I would venture to say that what would happen is nothing more and nothing less than that it would be attracted toward the Earth by the force of gravity. Newton, I imagine, would have agreed with me, would he not?

Why do I raise this point? Simply because, like Karl Raimund Popper before me, I wish to call into question the existence of observation statements and declare my steadfast faith in theoretical statements. “Fist,” “trot,” “gallop,” and “falling” are all theoretical terms, for they identify objects and the relations between them not in an objective or uniquely possible way, but because they are grounded in a particular theory, whether people are aware of it or not. Ordinary language users are usually oblivious to this fact, and the well-known saying that it is not we who speak language but language that speaks through us applies perfectly to them. In science, however, an awareness that our observations are, to a very large extent, shaped by and derived from the theories we accept is, in my view, indispensable – it is a genuine *sine qua non*. Had the physics professor’s desk been in Earth orbit, the laptop, once pushed beyond its edge, would have continued to “hang” beside it, and one would then have to ask what had changed in reality that made the laptop “fall” from the desk on Earth but not here. The answer is that, in the heat of the polemic, the professor allowed himself to be carried away and relied on a relatively unsophisticated theory as the background for his observation, whereas the theory according to which “between any two bodies there exists a force that depends on their masses and the distance separating them... etc., etc.” is far superior to the everyday notion of things

simply falling. It is superior above all because it not only works equally well here on Earth and in orbit around it but also enables us to understand this seemingly surprising difference.

The foregoing reflections lead to several straightforward questions. For example, is there only one psychology? Or, better still, is it desirable for one model of psychology to dominate all others, leaving no room for competing interpretations and analyses? An affirmative answer to this question would make sense if, and only if, it could be demonstrated beyond reasonable doubt and “once and for all” that the dominant model provides the best possible description and explanation of reality. In the case of psychology and psychotherapy, however, matters are by no means so straightforward. PB does not, for example, consider how the medicalization of psychotherapy relates to politics and systems of government or, in other words, to what extent it is bound up with a particular conception of the individual as a one-person enterprise, wholly responsible for every aspect of his or her existence, both its brighter and darker sides, including mental health. Such a conception inevitably pushes into the background a question raised in many quarters: namely, to what extent CBT functions as an instrument for sustaining the system of neoliberal capitalism, together with all its adverse social consequences (for example, politicians’ failure to undertake reforms that would, *ex opere operato*, substantially reduce the prevalence of mental health problems in the population, as has been argued in the literature and empirical research within community psychology and *psicología de la liberación*). That this is an important issue when assessing the mental condition of, say, individuals suffering from low mood, suicidal ideation, or prolonged despair scarcely requires argument. It turns out that when such individuals – diagnosed as suffering from “depression” – are given an opportunity for a better life (secure housing, stable employment, or an improved financial situation), their supposed “depression” often disappears “of its own accord.” Either the diagnosis was mistaken, or psychotherapy is not the only factor that ought to be taken into account in such cases. I write this, moreover, as someone who still remembers the previous political system, when official Marxist science demonstrably and “on the basis of evidence” (*disclosure*: irony) created such diagnostic categories as “anti-revolutionary paranoia” and “sluggish schizophrenia,” making it possible to confine individuals deemed inconvenient to the regime in psychiatric hospitals for years without any possibility of appeal or trial. For someone living “within” the political order in which we currently function, questions and doubts of this kind may seem unusual, or even bizarre. At a deeper level of analysis, however, they cannot simply be ignored.⁵ I shall mercifully pass over in silence the suggestion that science practiced in the so-called free countries of the West is somehow immune to the kinds of political pressure and distortion characteristic of authoritarian regimes.

Another issue open to discussion is one of the central tenets of the cognitive approach (as, indeed, of the medical model), namely the claim that the factor ultimately determining whether we accept or reject a given view is empirical – or, more precisely, experimental – research. This, in turn, leads to the question of whether there exists some prior revealed knowledge that tells us that all the important regularities governing human psychological functioning are

⁵ On this subject, I recommend Władimir Bukovsky’s *I powraca wiatr* [*And the Wind Returns*] (Kraków: Znak, 1999) and Bartłomiej Brażkiewicz’s *Psychiatria radziecka jako instrument walki z opozycją polityczną w latach 1918–1984* [*Soviet Psychiatry as an Instrument of the Struggle against the Political Opposition, 1918–1984*] (Toruń–Łysomice: Europejskie Centrum Edukacyjne, 2004).

testable by means of procedures of this kind. PB merely “glides over” the issue of the replication crisis in psychology, but perhaps it is not necessarily “curable” by means of ever more sophisticated experimental or statistical procedures, or even by meta-analyses. Perhaps – and why not, in for a penny, in for a pound – it stems instead from the inadequacy of the theories and metatheories adopted by researchers, and it is the shortcomings of those theoretical choices that are ultimately decisive. Moreover, if someone seeking the help of a psychotherapist were informed that the methods being used had been derived from scientific studies replicating at a rate of only 36 percent (the figure is taken from PB), I am not sure that this would be met with much understanding. I myself was once treated for a somatic illness with a method whose presumed effectiveness was only slightly higher (around 40 percent), and instead of recovering, my condition deteriorated. The lasting consequences of that treatment have remained with me to this day, almost two decades later. Why, then, was I subjected to such treatment? Because at the time it carried the label of EBM and was therefore regarded as the optimal course of action. To be clear, I have no intention of questioning the medical model in medicine, for some fifteen years later different medications successfully cured me of the same illness. Moreover, I fully recognize that there are diseases for which no existing treatment can offer a cure. Psychology, however, is different, because alongside those “relations between observable physical phenomena” so greatly cherished by advocates of the natural sciences, it must also deal with the entire realm of intentions, meanings, and sense, a realm that does not exist anywhere in nature apart from human beings. We have known this ever since Wilhelm Dilthey and his anti-naturalist revolt at the beginning of the twentieth century.⁶

These are, of course, only preliminary reflections, which require further clarification, elaboration, and stronger supporting argument. Before concluding this section, however, I should like to offer one example illustrating how unexpectedly scientific opinion can change. When I was first taught the history of psychology many years ago, the accepted interpretation of the doctrine of psychophysical parallelism held that it gave rise to two unfortunate consequences: epiphenomenalism and panpsychism. As is well known, Gustav Theodor Fechner was a panpsychist, and although his contributions to psychology were numerous, this particular view was certainly not counted among them. Indeed, until quite recently, anyone expressing such ideas would have been met by educated interlocutors with little more than a shrug of the shoulders, if not a sarcastic smile. Such a way of perceiving and interpreting reality might perhaps be expected of an Iroquois, an Aboriginal Australian, or some other animist still caught in the grip of an outdated worldview; it would surely be considered wholly unbecoming of an enlightened person, let alone a scholar – so the venomous reply would likely have gone. Yet in 2019 Philip Goff published *Galileo's Error: Foundations for a New Science of Consciousness*. On the website of its Polish publisher (PWN), the book is described as follows: “One of the world’s leading philosophers of mind presents a clear yet provocative

⁶⁾ Wilhelm Dilthey wrote as early as 1910: “[T]he tones that form a musical composition, the canvas on which we paint, the courtroom in which a verdict is pronounced, the prison in which a sentence is served, merely have their material in nature. Every operation of the human sciences dealing with such external states of affairs has to do merely with the sense and meaning they receive through the activity of spirit and how it serves the understanding that grasps this meaning and sense in them”. Dilthey W. *The Formation of the Historical World in the Human Sciences*. In: Makkreel RA, Rodi F, eds. *Selected Works of Wilhelm Dilthey*. Vol. III. Princeton, NJ: Princeton University Press; 2002:140–141.

argument for an entirely new theory of consciousness: panpsychism.”⁷ My point here is not whether Goff is right or wrong, but rather that views which at one time appeared to have been rejected “once and for all” may return to the debate and once again be taken seriously.

This line of argument opens up yet another important and highly sensitive area for discussion. To *critica*, which we have not yet left behind, we must now add *cultura*, leading us to the question of psychoanalysis and its typically marginal position within academic psychology. At major universities responsible for educating future psychologists, one encounters very few courses devoted to classical psychoanalysis; indeed, its absence has become almost the norm (not to mention that one is even less likely to find courses devoted to Jung, Lacan, Klein, Bion, Winnicott, and others). One cannot help gaining the impression that the contribution of the various schools of psychoanalysis to contemporary psychology is being systematically “erased” from the educational process. Within CBT circles, it is seldom acknowledged that one of its principal founders, Aaron T. Beck, was – certainly at the outset, but not only then – far closer to the Freudian tradition than is commonly assumed⁸ (his mentor was David Rapaport, a leading representative of so-called ego psychology), and that, contrary to the standard narrative, he never radically broke with psychoanalysis but rather, over the years, “sought to reconcile the psychodynamic thinking with which he was familiar with the demands of science while struggling to reformulate Freud’s theories and ideas.”⁹

PB’s book does not depart from this practice, although I should emphasize that in the final version the author abandoned the most emotionally charged accusations directed against Freud, his theories, and their followers, which I had pointed out in my publisher’s review. In the original version, from among the considerable number of critics of psychoanalysis, PB chose as his principal guide none other than Frederick Crews, a professor of English literature, whose book,¹⁰ in my opinion, bears witness to an obsession with Freud and constitutes a decidedly poor source for discrediting him. I would not mention this now were it not for the fact that Crews still appears, in a boxed insert, as the author of one of the recommended readings for those interested in the scientific status of psychoanalysis. In a witty review of the book, the well-known American intellectual Louis Menand remarks that it drives the stake into the heart of the “vampire” Freud with such determination that it ought to be sold together with a clove of garlic.¹¹ Particularly revealing is Crews’s eagerness

⁷) PWN Bookstore. *Błąd Galileusza*. [cited 3 May 2026]. Available from: <https://ksiegarnia.pwn.pl/Blad-Galileusza.931157234.p.html>

⁸) Rosner RI, Aaron T. Beck’s drawings and the psychoanalytic origin story of cognitive therapy. In: *Hist. Psychol.* 2012; 15(1): 1–18.

⁹) Romanowska M, Dobroczyński B. Pojęcie nieświadomości we wczesnej terapii poznawczej Aaron T. Becka [The Concept of the Unconscious in Aaron T. Beck’s Early Cognitive Therapy]. *Psychoterapia*. 2021;2(197):75. doi:10.12740/PT/139086

We have also discussed this issue in: Romanowska M, Dobroczyński B. *Unconscious Processes in Aaron Beck’s Cognitive Theory: Reconstruction and Discussion. Theory & Psychology*. 2020;30(2):223–242; and Romanowska M, Dobroczyński B. Procesy nieświadome we współczesnej terapii poznawczej Aaron T. Becka [Unconscious Processes in Aaron T. Beck’s Contemporary Cognitive Therapy]. *Psychoterapia*. 2021;3(198):5–15. doi:10.12740/PT/142223.

¹⁰) Crews F, *Freud: The Making of an Illusion*. New York 2017.

¹¹) Menand L. Why Freud Survives. *The New Yorker*. August 28, 2017. [cited 3 May 2026]. Available from: <https://www.newyorker.com/magazine/2017/08/28/why-freud-survives>

to dwell on Freud's alleged moral failings: his supposed autoeroticism, his alleged affair with Minna Bernays – his wife Martha's sister – his use of cocaine, and his bisexuality. In this context, it is difficult to resist quoting a longer passage from Menand's openly ironic discussion, in which, while succinctly summarizing the principal accusations advanced by Freud's uncompromising critic, he simultaneously performs their symbolic disarming:

Let's assume that Freud used cocaine as an anxiolytic and aphrodisiac. That he had an eye for sexy women. That he masturbated, solicited a prostitute, shared he-man fantasies with his girlfriend, and got crushes on male friends. Who cares? Human beings do these things. Even if Freud had sex with Minna Bernays – so what? The standard revisionist hypothesis is that the sex took place on trips that the two took together without Martha, of which, as Crews points out, there were a surprising number. But Crews imagines assignations in the family home in Vienna as well. He notes that Minna's bedroom was in a far corner of the house, meaning that "the nocturnal Sigmund could have visited it with impunity in predawn hours." Could he have? Apparently. Should he have? Probably not. Did he, in fact? No one knows. So why fantasize about it? A Freudian would suspect that there is something going on here.¹²

Such accusations, like many others, are rhetorically effective but, unfortunately, double-edged and hardly constitute a model of scholarly criticism. For almost everything that Crews wrote about Freud could equally well be said of the figure whom PB presents in *The War over Psychotherapy* as an almost exemplary model of how a psychologist ought to conduct scientific inquiry – namely, the founder of behaviorism, John B. Watson. It is by no means difficult to point to Watson's questionable character, his mistaken views on a variety of issues, or his ethically dubious conduct, which may well have been even more serious than Freud's. It requires no extensive knowledge of Watson's personal or academic life to arrive at the following diagnosis: he was a decidedly suspect individual who came into conflict with the law already in his youth and was, for that reason alone, a highly questionable role model, both in his private life and in science. His views on various aspects of human functioning – for example, the role of the brain in behavior – were challenged by means of "evidence-based procedures" as early as two years after the publication of the behaviorist manifesto. He was dismissed from Johns Hopkins University because of his affair with his student Rosalie Rayner, with whom he had earlier, driven by relentless ambition, conducted the "Little Albert" experiment, ethically questionable because of the harm inflicted on the young boy and the absence of any subsequent attempt to remedy it. Moreover, he held rather peculiar views on child-rearing, encouraging parents not to show their children too much affection, believing that this would foster greater independence and self-reliance. His son William, incidentally, died by suicide in 1954, perhaps as a consequence of the methods of upbringing employed by his father. After leaving Johns Hopkins University, Watson took a position at the J. Walter Thompson advertising agency, where he employed manipulative and deceptive techniques, using his knowledge of human behavior for commercial gain. He eventually rose to the posi-

¹²) Ibid.

tion of vice president and became a millionaire. Before his death, he burned all his papers, presumably in order to conceal both his borrowings from others and compromising aspects of his life. Is it easy to dismiss these accusations? Not really, although they are formulated in an unmistakably tendentious and exaggerated manner. But is this really what Watson's significance for psychology consists in? Clearly not, just as Freud's significance does not lie primarily in his mistakes. As Arthur O. Lovejoy, the founder of the history of ideas, put it in the work that has become canonical for the discipline: "most philosophic systems are original or distinctive rather in their patterns than in their components."¹³

Yet it is precisely the overall framework of psychoanalysis that retains its value in many respects, rather than some of Freud's less successful ideas. Whenever PB writes about psychoanalysis, however, it is difficult to escape the impression that he treats it somewhat patronizingly and takes evident pleasure in making fun of it (thus fitting perfectly into a certain tradition within Polish psychology, for extensive discussions ridiculing Freudianism, replete with mockery and derision, can already be found in Władysław Witwicki's psychology textbook, published before the Second World War).¹⁴ A similarly mocking and patronizing style has, moreover, a long-established tradition in international psychology as well, its best-known representative being Hans Eysenck, who brought the heaviest artillery to bear against Freud without refraining from ridicule and sarcasm. If, however, we turn to more nuanced accounts of psychoanalysis – accounts that are both reliable and supported by the vast body of literature grounded in "evidence-based" research – they present Freud's legacy in a far more balanced light and one decidedly more favorable to the founder of psychoanalysis. I shall begin with a text that, in my opinion, every critic of psychoanalysis ought to read twice before expressing an opinion about it: Drew Westen's nearly forty-page article published in 1998 (indeed, I suspect that most critics of psychoanalysis have never read it even once).¹⁵ It appeared in a highly respected journal, and from the very first footnote we can see that a great many "enemies of Freud" contributed to its final shape, for Westen acknowledges an entire constellation of distinguished scholars, not necessarily sympathetic to psychoanalysis, including Daryl Bem, Seymour Epstein, Kurt Fischer, David Funder, Glen Gabbard, John Kihlstrom, Stephen Kosslyn, Dan McAdams, Jonathan Shedler, Robert Sternberg, and Joel Weinberger.

In the published version of his book, PB does refer to Westen's article, but primarily in order to highlight the distinction between classical psychoanalysis and the psychodynamic approach, from which he concludes that the latter is acceptable within certain limits and deserves to be regarded as an EBP, whereas the former most certainly does not. It is difficult to disagree with this conclusion, just as it is difficult to imagine that there is still anyone who takes the classical version of psychoanalysis – with the couch – seriously or, moreover, continues to employ it in therapeutic practice. If this is indeed the case, as PB points out in his book, it is difficult even to know how to comment on it. What is particularly worth quoting from Westen's article, however, is the concluding passage, for

¹³) Lovejoy AO. *The Great Chain of Being: A Study of the History of an Idea*. Cambridge, MA: Harvard University Press; 1936, p. 3.

¹⁴) Rzepa T, Dobroczyński B. *Historia polskiej myśli psychologicznej*. Warszawa: PWN; 2019, esp. pp. 287–289.

¹⁵) Westen D. The Scientific Legacy of Sigmund Freud. Toward a Psychodynamically Informed Psychological Science. In: *Psychological Bulletin* 1998; 124(3): 333–371.

it succinctly captures the essence of this richly documented publication, whose central leitmotif is to demonstrate that:

Freud advanced several fundamental propositions, once highly controversial and unique to psychoanalysis, that have stood the test of time, and many of the implicit and explicit assumptions today at the heart of the school of thought he initiated have a considerably stronger empirical basis than has been widely supposed. This is probably the best any thinker could hope for in a rapidly developing discipline like ours 60 years after his death.¹⁶

To be fair, however, and to arrive at a kind of “final” conclusion on this matter, I shall refer to a book published only a few years ago (and therefore much more up to date than Westen’s article), which covers virtually everything contemporary psychology knows about the unconscious. Moreover, this body of knowledge is presented both from an academic perspective – that is, with reference to experimental research, theoretical work, and neuroscience – and from a clinical perspective, together with numerous suggestions and recommendations for psychotherapists.¹⁷ In order to avoid the sort of eristic tactics that can be tempting when defending someone whom one greatly admires and respects, I adopted the same procedure that PB himself employed in his book and asked ChatGPT,¹⁸ over the course of several successive prompts, what view of Freud the authors of this publication present. This is what I learned as a result (for the sake of clarity and ease of reading, I have set the entire passage in italics while preserving the boldface added by the AI):

*Weinberger and Stoycheva adopt a **nuanced and balanced** position toward Freud.*

1. **Freud’s enormous contribution.** *The authors state explicitly that Freud “changed the course of Western thought” by bringing the unconscious to the forefront. His topographical and structural models are presented fairly, with due attention to their internal logic and to the reasons why Freud himself subsequently revised them.*
2. **Psychoanalysis as the only “institution of the unconscious” for decades – but...** *Psychoanalysis consistently kept the unconscious “at the center,” yet it rarely provided adequate **empirical evidence**. The authors regard this lack of dialogue with academic psychology as a serious weakness, noting Kihlstrom’s criticisms and the long-standing debates concerning the scientific status of psychoanalysis.*
3. **Methodological criticism.** *Despite its historical importance, psychoanalysis remained largely disconnected from empirical research. The authors argue that its principal models (topographical, structural, and relational) are intellectually interesting but excessively speculative and only weakly falsifiable.*

¹⁶ Ibid. p. 362. I would particularly like to draw the reader’s attention to the passage addressing the question of why so many Freudian propositions nevertheless failed to withstand the test of time: “Grand theorists like Freud, Piaget, and Skinner are routinely the grandest purveyors of falsehood in the business. This reflects simple mathematics: The more propositions one advances (and the bolder those hypotheses are), the higher the probability that several will be wrong.”

¹⁷ Weinberger J, Stoycheva V. *The Unconscious: Theory, research, and clinical implications*. New York: The Guilford Press; 2020.

¹⁸ I would like to thank Dr. Dariusz Grabowski for his invaluable help in this endeavor.

4. **When psychoanalysis “meets the data.”** Weinberger and Stoycheva discuss research programs inspired by psychoanalysis (for example, Lloyd Silverman’s work on **subliminal psychodynamic activation**), which produced replicable findings consistent with psychoanalytic hypotheses concerning aggression, depression, and fantasies of fusion or “oneness.” Similarly, experimental studies of unconscious **motivation** suggest that some of Freud’s ideas do have empirical counterparts – provided they are translated into the language of contemporary cognitive science.
5. These findings show that **some** psychoanalytic propositions can be tested and, when properly operationalized, can indeed **receive empirical support**.
6. **Relational and post-Freudian revisions.** The authors discuss contemporary relational approaches that understand the unconscious as **co-constructed** within the therapeutic relationship. Greater emphasis is placed on **dissociation** than on classical repression, on the fluid boundary between the conscious and the unconscious, and on the view that “transference,” in its classical sense, is no longer an adequate description of what occurs in therapy. This illustrates **the internal evolution** of psychoanalysis.
7. **The standard of explanation.** In keeping with a scientific perspective, the authors emphasize that the **value** of theoretical constructs – including Freud’s – depends on their **explanatory and predictive power**. If they fail to meet this standard, they should be rejected or revised.

In summary, their position is this: Freud was right to regard **the unconscious** as central, and for that reason he deserves a place in the canon. **The language, mechanisms, and methods** of psychoanalysis, however, must be updated in light of contemporary evidence concerning automatic processes, implicit memory, latent learning, affective priming, and cognitive architecture. Many of psychoanalysis’s central intuitions can be defended, provided they are reformulated as operational hypotheses, empirically tested, and situated within contemporary models of the brain and mind. (End of AI quotation.)

It is time to draw some conclusions. Friedrich Nietzsche once complained about what he regarded as people’s widespread tendency to read only those works whose authors advance views with which they already agree, wryly observing that, from an intellectual point of view, this is a rather sterile practice. According to him, genuine intellectual benefit comes only from reading works that present well-argued positions with which we profoundly disagree. If we take the German philosopher’s advice seriously, then for all those who are unwilling or unable to accept PB’s views – and they will no doubt be numerous – his book is, in a certain sense, required reading. It will undoubtedly enable them to formulate counterarguments and to provide an even stronger justification for their own position. I would strongly encourage them to do precisely that. Those readers, on the other hand, who find themselves sympathetic to the book’s central claims will discover in it valuable support for their own views. It is a thoroughly documented work, and in support of his arguments the author draws upon a wide range of legal, scholarly, and statistical sources, taking care that they consistently reflect the current state of the art. Finally, I believe that this book ought to be read by all psychologists who genuinely care about the well-being of those who turn to them for help, for it will undoubtedly encourage a deeper reflection on their own understanding of the profession.