

ANTI-REDUCTIONISM AT THE CONFLUENCE OF PHILOSOPHY
AND SCIENCE: ARTHUR KOESTLER
AND THE BIOLOGICAL PERIPHERY.

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SUMMARY

The Hungarian-born intellectual Arthur Koestler produced a wide-ranging corpus of written work throughout the mid twentieth century. Despite being the subject of two huge biographies in recent years, his long-standing engagement with numerous scientific disciplines remains unexplored. This paper situates Koestler's scientific philosophy within the context of mid-twentieth century science and explores his relationship with key figures, including Dennis Gábor, C. H. Waddington, Ludwig von Bertalanffy and J. R. Smythies. The argument presented is threefold. First, surprisingly, serious scientists, particularly in the biological sciences, took Koestler's scientific work seriously; second, despite Koestler's best efforts, his allies could not agree on a single articulation of anti-reductionism; and third, the reductionist/anti-reductionist debates of the mid twentieth century constituted a battle for the authority to speak on behalf of 'science' that led Koestler into direct conflict with figures including Peter Medawar. By exploring the community associated with Koestler, the paper sheds new light on the status of scientific authority and the relationship between scientists' metaphysical beliefs and their practices.

Keywords: Arthur Koestler; anti-reductionism; holism; C. H. Waddington; Dennis Gábor; Peter Medawar

RESUMEN:

El intelectual Arthur Koestler, nacido en Hungría, produjo un extenso *corpus* de trabajos escritos a lo largo de la mitad del siglo XX. A pesar de haber sido objeto de dos grandes biografías en los últimos años, su compromiso de larga data con numerosas disciplinas científicas sigue sin ser explorado. Este artículo sitúa la filosofía científica de Koestler en el contexto de la ciencia de mediados del siglo XX y explora su relación con figuras clave, incluyendo a Dennis Gábor, C. H. Waddington, Ludwig von Bertalanffy y J. R. Smythies. El argumento presentado es triple. Primero, de manera sorprendente, científicos serios, particularmente en las ciencias biológicas, tomaron en serio el trabajo científico de Koestler; segundo, a pesar de los mejores esfuerzos de Koestler, sus aliados no pudieron ponerse de acuerdo en una única articulación del anti-reduccionismo; y tercero, los debates reduccionistas/anti-reduccionistas de mediados del siglo XX constituyeron una batalla por la autoridad para hablar en nombre de la 'ciencia', lo que llevó a Koestler a un conflicto directo con figuras como Peter Medawar. Al explorar la comunidad asociada con Koestler, el artículo arroja nueva luz sobre el estatus de la autoridad científica y la relación entre las creencias metafísicas de los científicos y sus prácticas.

Palabras clave: Arthur Koestler; anti-reduccionismo; holismo; C. H. Waddington; Dennis Gábor; Peter Medawar

INTRODUCTION

Biology, neurology,
Aesthetics and psychology
Ethics, epistemology,
The art of terminology
I'll study, and with them, I do resolve
The riddle of the Universe I'll solve¹.
(Mamaine Koestler, 1947)

His scientific ideas were borrowed or didn't amount to much.²
(Tibor Fischer, 2010)

In 1968, the Hungarian–British intellectual and novelist Arthur Koestler (1905–83) gathered luminaries from numerous scientific disciplines for a symposium in the idyllic setting of Alpbach, among the Austrian Alps. The theme of the event was ‘Beyond reductionism’, and it consisted of presentations and extended discussions on topics across the biological and psychological sciences. Among the distinguished list of attendees were the British developmental biologist C. H. Waddington, the Swedish biochemist Holger Hyden, the Austrian–British economist F. A. Hayek, the American neuroscientist Seymour Kety, and the Austrian bio-mathematician Ludwig von Bertalanffy. All had philosophical inclinations that interacted strongly with their respective areas of scientific research, and over the course of several days, the group outlined and discussed numerous arguments against the principal object of Koestler's discontent: what he termed ‘the totalitarian claims of the neo-Darwinian orthodoxy’³. The meeting was chaired by the British ethologist W. H. Thorpe and sought to meet Koestler's ambition to demonstrate ‘the integrative power of the whole over its parts’ across the sciences.⁴ The goal at Alpbach and across Koestler's scientific writings (identified in the first epigraph by his second wife, Mamaine) was to mobilize concepts from numerous disciplines to unify anti-reductionist theories of biology and psychology, thereby transforming both scientific thought and society.

The most recent and authoritative biography of Koestler has dismissed the gathering as a ‘foolhardy’ endeavour in which Koestler's own contributions received a lukewarm reception, the whole event leaving him ‘disappointed’⁵. We can see this same attitude towards Koestler's engagement with scientific topics in the second epigraph, taken from a review by Tibor Fischer of the most recent biography of Koestler by Michael Scammell, and across the evaluation of his scientific writings. On this reading, Koestler's status as an outsider—cemented and perhaps exaggerated by his fascination with parapsychology late in his career—caused a fundamental rift between his theoretical work and ‘conventional’ (empirical) scientific practice.⁶ According to this view, Koestler's disconnect from mainstream science was exposed not just during the Alpbach Symposium but also in reviews of his work and exchanges in the scientific and popular press. One such review of the published proceedings from Alpbach in *New Scientist* was given the uncompromising title ‘A critique of the Koestler clique’.

The author of that review, David Newth, was at that time Regius Professor of Zoology at the University of Glasgow, and he chose that moment to pose a provocative question about the relationship between science and philosophy: ‘Need a scientist's philosophical linen be as clean as his laboratory glassware?’⁷ Newth's answer was emphatically negative; he regarded the philosophical preoccupations of scientists as at best unimportant musings and at worst dangerous distractions. The metaphysical wrangling between scientists from reductionist and anti-reductionist perspectives was also dismissed by June Goodfield as ‘irrelevant to what is actually done in the laboratory, mere echoes from the sidelines whose impact and influence are effectively nil’.⁸ However, a closer examination of the community from whom Koestler drew his scientific allies shows that their professional practices were informed to a significant degree by their engagement with philosophical arguments about science.

Whereas Newth and Goodfield frame philosophical and scientific enterprise as somehow separate kinds of intellectual activity, I argue instead that the ideas expressed at and around the Alpbach Symposium offer a revealing snapshot into both the practice and the philosophy of twentieth-century biology. Surrounding Koestler were significant figures from a range of scientific disciplines, all of whom had become dissatisfied with what they saw as the philosophical impoverishment of the biological and psychological sciences. Although Koestler was not part of this professional community, he was nevertheless an important source of ideological inspiration. Ultimately, the Alpbach Symposium brought together a diverse range of individual researchers who represented a broader reaction against the expansion of the reductionist ideologies of the physical sciences into the biological and psychological realms. Neither Koestler nor his scientific allies seriously questioned the benefits derived from molecular biology, such as the discovery of the structure of DNA in the previous decade, yet they shared a nagging discontent with the resulting determinist and reductionist agenda. As Paul Weiss noted in a telling comment at Alpbach, ‘Nothing that I am saying about molecular biology should be construed as a lack of appreciation of the tremendous advances made in that field. It’s only a warning against the monopolistic position often taken there.’⁹

This is not a paper about the proceedings at Alpbach, nor is it an attempt to rehabilitate Koestler or his philosophy of science. Rather, I aim to show that his status as an outsider was mitigated by his scientific connections, and that his work on scientific subjects gained greater credibility among a particular subgroup of researchers than one might expect: serious scientists took Koestler’s science seriously. Motivated by Evelyn Fox Keller’s claim that ‘the primacy of the gene as the core explanatory concept of biological structure and function is more a feature of the twentieth century than it will be the twenty-first,’ this paper reveals important and previously hidden features of biological anti-reductionism in the mid-twentieth century.¹⁰

I begin by outlining the powerful political origins of Koestler’s own interests in questions of science, before moving to consider his connections with the scientific community, especially Dennis Gábor, C. H. Waddington, Ludwig von Bertalanffy, and J. R. Smythies. By looking at the events, publications, and ideas associated with this group of researchers, I argue that we can see mid-twentieth-century scientific endeavor as an activity in which philosophical perspective was a core component and in which high-profile, high-stakes battles for the authority to speak on behalf of ‘science’ brought Koestler into conflict with vocal opponents, most notably Peter Medawar. This forces us to think in new ways about the status of science during the mid-twentieth century, especially in Britain, and reflect on the ideological challenges posed by radically different ways of conceptualizing social and biological systems, from molecular biology and macroeconomics to cybernetics and cognition.

Far from constituting a distinctive scientific orthodoxy, reductionism in the period from around 1950 to 1975 was highly heterogeneous. Following the work of Christopher Lawrence, George Weisz, and others on the development of holism in the period leading to 1950, we see that in subsequent years anti-reductionist thought drew inspiration from within and outside the sciences, and the nature of the issue—at once scientific and ideological—enabled public intellectual figures such as Koestler to make contributions to a major scientific debate.¹¹ Ultimately his publications—which were aimed at scientists but tended to find greater favor with wider public audiences—constructed for Koestler an attentive audience both inside and beyond mainstream professional science¹². In looking at this key period, we are also encouraged to reflect on the wider origins of mid-twentieth-century biological anti-reductionism, whose roots can be traced back to the systems theories of the 1920s, which themselves drew conceptually on the work of an earlier post-Kantian idealism associated with Leibniz, Goethe (often cited as the chief source of Koestler’s inspiration), and others, as well as prominent vitalist–materialist debates of the late eighteenth and nineteenth centuries.¹³

UTOPIA AND SYNTHESIS: ARTHUR KOESTLER

We begin, though, with Koestler. Although generally better known for his fiction and journalistic writing, his corpus of work ranges over a wide variety of subjects. Politically active, especially during his early life, the controversial Koestler was initially an ardent supporter of communism before becoming one of its most

outspoken critics in the late 1930s. After that, he adopted a complex political position that was as much informed by opposition to ideologies —Zionism as well as communism— as by commitment to single issues, including vocal criticism of the death penalty and support for euthanasia and prison reform. Having gained some experience as a science journalist earlier in his career, Koestler turned his attention toward creating, rather than reporting, scientific work after settling in Britain in 1940. His scientific agenda was closely informed by his political beliefs, and he became a central figure for the anti-reductionist movement within the biological sciences during the mid-twentieth century.

Koestler divided his literary life into two distinct phases: the ‘Search for Utopia’ —encompassing his politically motivated writings of the 1930s and early to mid-1940s— and the ‘Search for a Synthesis,’ which comprised his scientific and other work from the mid-1940s onwards. Koestler saw these periods as mutually exclusive, but this retrospective self-categorization is misleading, despite Koestler’s own admission that he underwent such a radical shift that it made him ‘feel sometimes as if I had undergone a change of sex.’¹⁴

Koestler’s scientific work can be seen, in one sense, as an attempt to formulate what was termed ‘a new form of mysticism,’ drawing on the widely recognized persistence of holistic thought in German culture during the first half of the twentieth century.¹⁵ However, his close friend John Beloff, the controversial parapsychologist, noted that, for Koestler, there were, in fact, three central doctrines against which his scientific writing was directed.¹⁶ These were neo-Darwinism, behaviorism, and reductionism—singled out by Koestler as ‘cardinal fallacies ... from which we are only gradually beginning to free ourselves.’¹⁷ His deep-seated mistrust of deterministic philosophy —stemming from his rejection of communism in the late 1930s— led to a search for a particular formulation of the sciences in which the purposefulness of human existence was restored.

The first attempt to articulate these perspectives came in *The Yogi and the Commissar*, a collection of essays published in 1945. *The Yogi and the Commissar* is undeniably a political text, providing a thinly veiled critique of communism following Koestler’s break with the ideology in the run-up to World War II. However, placing the collection at the beginning of his new phase of scientific inquiry yields a quite different analysis. In outlining two reified philosophical positions —those of the Yogi and the Commissar— we can see the hallmarks of Koestler’s later scientific work: it was an articulation of a system that avoided the pitfalls of both reductionism and holism.

The figure of the Commissar extolled the primacy of the power of change from outside a system, a strictly utilitarian ethics, and, more broadly, radical social reorganization. He also represented inclinations characteristic of the scientific doctrines that Koestler would later go on to attack vociferously: that the Universe is a large, clockwork entity, that logical reasoning is an unfailing compass in guiding knowledge, and that determinism is rife within the human condition. The dogmatism that Koestler saw in the Commissar would later be applied to his reductionist opponents: ‘you cannot argue with a naked Commissar —he starts at once to beat his chest and next he strangles you, whether you be friend or foe, in his deadly embrace’.¹⁸

By contrast, the Yogi saw reduction to mechanism and components as lacking explanatory potency. Koestler noted that the position of the Yogi was just as problematic as the Commissar’s: ‘you cannot argue with the ultra-violet skeleton [Yogi] either, because words mean nothing to him.’¹⁹ This division has been succinctly represented as follows: whereas ‘the “Commissar” loses himself in the fanatic oneness of the authoritarian institution, the “Yogi” loses himself in the ecstatic oneness of self-denial.’²⁰ Koestler originally saw this dichotomy as unbridgeable, arguing that ‘one might as well ask a homosexual to make a little effort towards the opposite sex, and *vice versa*’, although he would later articulate an anti-reductionism in which biological and social entities displayed tendencies of both integration and autonomy.²¹

The scientific implications of the worldview expressed in *The Yogi and the Commissar* were not left to the imagination. From Koestler’s perspective, ‘The modern physicist ... denies that his task should be to “explain” anything, and he takes a masochistic delight in producing formulae which establish with precision the degree of imprecision in his statements.’²² Koestler also had the then-standard stimulus–response model in his sights, arguing instead that patterns of influence in biological systems are ‘that of a network, not of a causal chain.’²³

NEW SCIENTIFIC CONNECTIONS: *INSIGHT AND OUTLOOK*

According to David Cesarani, the final essay in *The Yogi and the Commissar* represents ‘the most concise and accessible expression of Koestler’s unified theory.’²⁴ From this more general account of political and natural systems, Koestler moved to align his work more closely with empirical science in *Insight and Outlook*. Koestler and his then-partner, Mamaine Paget, endured endless headaches during its preparation. According to Paget, ‘he [Koestler] is doing hellish work correcting endless French versions of his books and play, and therefore can’t get on with his book [*Insight and Outlook*].’²⁵ Having previously been concerned chiefly with journalistic pieces and novels, Koestler was therefore taking his work in a different direction by tackling both scientific subjects and current research in the field. To help refine his thinking and achieve recognition by practicing scientists, Koestler began to correspond with several figures who were sympathetic to his aims.

One of the first such connections was initiated by a letter that he received from Dennis Gábor in 1946. In his own words, Gábor, now perhaps best remembered for his discoveries relating to holography for which he received the 1971 Nobel Prize in Physics, was ‘by profession an engineer-physicist, by inclination rather a mathematician-inventor.’²⁶ Koestler and Gábor shared a common background: both had been born in Budapest around the turn of the twentieth century to Jewish parents and later settled in Britain.²⁷ It was, however, common ground on matters of science rather than heritage that sustained their discussions.

The correspondence between Koestler and Gábor ran from 1946 until 1972, and it reveals Koestler’s changing attitudes towards his own and others’ scientific inclinations. Gábor noted that since 1938, when he read Koestler’s novel *The Gladiators*, he had been an ardent admirer of his work.²⁸ It was not until some eight years later, however, that Gábor first wrote, heralding the significance of Koestler’s writings for the scientific community: ‘Your books are widely read among the younger engineers and scientists ... your influence is becoming very strong on the younger generation.’²⁹ Although Gábor singled out *Darkness at Noon* —the anti-communist novel, arguably Koestler’s most famous work, which drew inspiration from the Moscow Show Trials of the 1930s—as being among the publications that had gained Koestler such notoriety, he also highlighted the importance of his perspectives on science, articulated at this point only in *The Yogi and the Commissar*.³⁰

The synchronicity between Gábor and Koestler’s approaches to scientific thought was also evident, with the former asserting: ‘I am myself rather romantically and philosophically inclined, and I am still shocked by the nose-to-the-ground attitude of ... the most successful scientists, especially the British.’³¹ In spring 1946, when Gábor initiated the correspondence, Koestler was struggling with material for *Insight and Outlook*; in this context, the encouragement from Gábor came as a welcome boost.³² Although Koestler already had some familiarity with scientific topics, he was still a relative newcomer to the field, and it is clear that he was profoundly influenced by his fellow countryman.³³ Indeed, in responding to Koestler’s comments that he was writing a text concerning ‘the psychology of the higher mental functions,’ Gábor replied that he was ‘very much interested ... and should be naturally grateful for any opportunity to study it beforehand.’³⁴

Accordingly, just as Koestler’s work on *Insight and Outlook* was coming to a conclusion at the end of 1948, so too did his correspondence with Gábor become more animated. On the basis of his perusal of the manuscript, Gábor wrote that Koestler had ‘made a great discovery, and though, from what I hear, the reception in general is only lukewarm, you can be sure that sooner or later, it will be classed with Freud’s *Traumdeutung* [“The Interpretation of Dreams”] and a very few other great books.’³⁵ Gábor’s contribution was extensive: he made theoretical suggestions for Koestler’s approach and highlighted recent publications that might be useful for future work. The comparison between the ideas presented in *Insight and Outlook* and Freud’s masterpiece gave Koestler renewed confidence in both his own theoretical position and his ability to reach and influence a professional scientific audience.

At this stage of his engagement with scientific issues, Koestler was contacted by Eric Strauss, the noted psychiatrist who in 1940 had opened the first outpatient clinic for electroconvulsive therapy in England.³⁶ Strauss had heard that Koestler was planning to write a book grounding ethics in a naturalistic psychology and resolved to send Koestler one of his own commentaries on current trends in the field.³⁷ Koestler had already written considerable sections of *Insight and Outlook*; nevertheless, he still took the opportunity to

ask Strauss how he might gain further credibility among scientific communities:

As a professional novelist, I have some misgivings about its reception in academic circles and by the scientific press, who, as a rule, have no great sympathy for outsiders butting in ... I thought it might help its reception if I had proved by a very small contribution to the theory of vision that I am not entirely a crank."³⁸

As if to further underscore his desire to secure recognition by the mainstream scientific establishment, Koestler also noted that the production of the book 'at present matters more to me than to see my novels on the best-seller list,' and he sought ways of ensuring that he reached the intended audience.³⁹ To this end, Koestler outlined to Strauss a seemingly original observation that he had made concerning perception. Strauss encouraged him to publish these results, saying that the 'discovery of retinal after-images of cortical origin is quite original and must be considered an important observation.'⁴⁰ However, when Koestler submitted these observations, via Strauss, to the editor of the *British Journal of Psychology*, Frederick Bartlett, they were promptly returned, with Bartlett commenting: 'I am sorry that I cannot publish it.... The observation which you have made has been made already a good many times before.'⁴¹

Koestler finally published *Insight and Outlook* in 1949. Although he acknowledged the 'many valuable corrections and suggestions' that he had received, Koestler still wrote to Gábor that he was 'rather frightened of its reception by academic circles.'⁴² In the book, Koestler begins with biology as an explanatory foundation upon which to base his ethical system. Cesarani has argued that *Insight and Outlook* 'cannot be divorced from Koestler's politics any more than John Locke's *An Essay Concerning Human Understanding* or Hume's *Treatise of Human Nature* can be sundered from their political philosophy.'⁴³ However, perhaps of greater significance is the fact that Koestler—a political activist, essayist, and novelist—had chosen to launch an ambitious foray into the realm of science, with the support and encouragement of two prominent figures in Gábor and Strauss. Several reviews of *Insight and Outlook* were critical of the 'lofty assumptions, empirical holes, unfounded hypotheses, and apologies for the absence of supporting data,' but the strategy of the book and the process of engaging first-hand with scientists remained a clear *modus operandi* over the remainder of Koestler's career.⁴⁴ Gábor, reminiscing some years later, continued to recall *Insight and Outlook* as being 'brilliant,' while Koestler found often-overlooked favor among the medical community, where an editorial in *The Lancet* commended his work, especially as 'he uses neurology and psychology to illuminate each,' and described *Insight and Outlook* as a 'remarkable book.'⁴⁵

In the preface to 'Insight and Outlook' Koestler stated explicitly that he set out

[First] to show that ... all the creative activities of man are based on a common pattern.... [Second] to show the possibility of a system of ethics which is neither utilitarian nor dogmatic, but derived from the same integrative tendency in the evolutionary process to which the creative activities of art and discovery are traced.⁴⁶

Koestler's choice of subtitle —*An Enquiry into the Common Foundations of Science, Art, and Social Ethics*— is also revealing. The source of this link between these areas, he argued, was the expression of biological organization in the social domain. The foundations of Koestler's system rested on the seemingly opposed properties of hierarchy and autonomy. He identified the 'functional units' present in both biological and social systems and, drawing on research from across the biological sciences, articulated a universal theory that described the arrangement and operation of living systems. In his own words:

[This] concept of the 'functional whole' is defined as the pattern of relations between its parts, and not as the mere sum of its parts. Mere summational aggregation of parts does not lead to the formation of functional wholes. A heap of coal dust does not behave as a functional whole, and whatever its size, its integrative hierarchy reaches the upper limit on the crystalline level.⁴⁷

Koestler took the position that although each sub-whole, of which the organism is constituted, retains a certain degree of autonomy, it is nonetheless subject to control by higher levels of sub-wholes. He termed these sub-wholes ‘holons,’ and the hierarchical systems that they inhabit ‘holarchies.’ From Koestler’s perspective, this structural arrangement was, of course, not confined to biology but extended upwards and downwards, from society to subatomic particles. It is particularly striking that, when extrapolated to the level of society, Koestler’s system appears to bear all the hallmarks of a stable political system, with individuals and groups at every level having a certain degree of autonomy while remaining subservient to the needs of the system as a whole; this description of biological systems therefore reflected his disenchantment with communism.⁴⁸

CRITICISM AND COMMUNITY: *THE ACT OF CREATION AND THE GHOST IN THE MACHINE*

After the mixed reception of *Insight and Outlook*, Koestler further expanded his engagement with scientific issues; his attempts to create a universal synthesis of the natural sciences and humanities also extended into the historical realm.⁴⁹ With *The Sleepwalkers* (1959), a then-standard text for historians of science and commended by Gábor as being ‘a brilliant piece of psychological history of science,’ Koestler charted the “‘history of cosmology” from the Greeks to Newton,’ showing, as his title suggests, that ‘the intellectual giants of the scientific revolution were only moral dwarfs.’⁵⁰ As one review noted, however, ‘this conclusion had been discernible already in Mr. Koestler’s earlier volumes.... His old thesis is now only reinforced historically to show that modern science... is the work of antisocial schemers, cowards, liars, hypocrites, irresponsible cranks, or contemptuous snobs.’⁵¹ However, although *The Sleepwalkers* was a historical critique of rampant scientism, Koestler made increased efforts to gather credibility from professional scientists in the production of the sequel to *Insight and Outlook*.

This counterpart was entitled *The Act of Creation*, ‘in which he [Koestler] tried, with only very partial success, to connect psychology and neuropsychology.’⁵² It was first published in 1964 and represented an attempt to explain the ‘creative act,’ which Koestler saw as common to the creative arts and sciences: holons again were at the core of the system.⁵³ After a lapse in correspondence during the 1950s, Koestler once again contacted Dennis Gábor to assist with the details of *The Act of Creation*. After reading initial drafts of sections dealing with the psychology of creativity, Gábor wrote to Koestler in the spring of 1960, commending him for his ‘bold dash into the field of “metaphorical” psychology.’⁵⁴ He went even further to highlight the perceived novelty of Koestler’s thesis, noting that

Professional psychologists and neurophysiologists will be angry with you because you have started digging a field which they wanted to open up only in the next century.... They will be even angrier if you succeed in concretizing your picture. In this, I shall be very happy to help you.⁵⁵

Although Koestler wrestled with the challenges of presenting a theory that crossed previously unsynthesized disciplines, he again found a willing ally in Gábor, who suggested that Koestler might collaborate with him to develop ‘powerful mathematics which, by symbolic operations, reveals things which you could not see without it.’⁵⁶ Although Gábor doubted whether this was possible, he expressed hope that Koestler would nevertheless be successful in the endeavor.

At the same time as he was engaged in completing the manuscript for *The Act of Creation*, Koestler contacted the Swedish neurobiologist Holger Hyden to request a supply of tricyanoaminopropene — a neurostimulant that Hyden and his colleagues had claimed could cause increased mental function. Koestler planned to use these tablets to create a series of experiments in collaboration with the Society for Psychical Research, of which he was an active and long-standing member, and he asked Hyden for advice about dosage.⁵⁷ Talk quickly turned to the book that he was close to finishing, and he persuaded Hyden to look over a series of sections related particularly to neurobiology. Hyden was happy to oblige and provided a few suggested additions, which were so well received by Koestler that Hyden ultimately agreed to “‘ghost” anonymously ... the five or six sentences that you suggest.’⁵⁸

Koestler also persuaded Cyril Burt, then a well-respected child psychologist, to write a glowing preface. Their extensive correspondence from 1961 shows Burt to be an enthusiastic supporter of Koestler's ideas who readily forgave 'the presumptuous enterprise of a transgressor [Koestler]' and was happy to make his 'heresies more respectable.'⁵⁹ In a draft of the preface, which Koestler received for proofreading in 1961, Burt wrote: 'Koestler's theory is, on his own admission, speculative. As he says... some or most of the details may be proven wrong. But, as [psychologist Karl] Lashley wrote, psychology and physiology must ultimately coalesce.'⁶⁰

Lashley, active and influential in the field of psychology since at least 1913, was a source of inspiration for both Koestler and Burt. Indeed, he was duly recognized by Koestler as one of the key figures whose work the behaviorist B. F. Skinner had failed to recognize. It was Skinner whom Koestler had in his sights throughout large sections of *The Act of Creation*. Ultimately, the opening section of the book provided a still broader biological and social basis for the creative act, which was due, Koestler argued, to a fundamental process of the joining together of two previously unconnected thoughts, fields, or perspectives: 'bisociation.'⁶¹ This was Koestler's own term to explain creativity, although it constituted little more than a summary of existing theoretical perspectives on the subject. The remainder was devoted to lending his holarchic system support through biological evidence.

The Act of Creation did attract attention from the scientific community, albeit in a manner not entirely in keeping with Koestler's ambition to be recognized as an original contributor to the methodological framework of science. Reviews in academic journals ranging from *Science* and *Isis* (the latter by a less-than-impressed George Gaylord Simpson) to *The American Journal of Psychology* were highly critical; in the last of these, the American psychologist of creativity Howard E. Gruber described the book as being 'strewn with bewildering neologisms, overly cute epigrams, and heavy-handed verbal mystique,' while John Maynard Smith characterized the book as 'magnificent... but it is not science.'⁶² These responses were belied by largely positive reviews in the mainstream British newspapers, despite a 'drubbing' in *New Statesman* from Peter Medawar, a long-standing opponent of Koestler.⁶³

The notes that Medawar compiled when composing his review are highly revealing. By the mid-1960s, he was established as a high-profile scientific authority in the public domain and had already produced a scathing review of Pierre Teilhard de Chardin's *The Phenomenon of Man* (1961), which had 'a feeble argument, abominably expressed,' as well as more general commentaries on the practice of science.⁶⁴ He now turned his attention to Koestler, whom he saw as an unwelcome and ill-informed commentator on matters of science; the philosopher Morton White noted that in many of Medawar's commentaries, 'Koestler is... given a pretty rough time when he ventures to generalize about scientific activity.'⁶⁵ Medawar's published review of *The Act of Creation* focused principally on Koestler's lack of experience in the practical business of doing scientific research; his lack of understanding was a secondary concern.⁶⁶ The opening of the review noted that 'Like other amateurs, Koestler finds it difficult to understand why scientists seem so often to shirk the study of really fundamental or challenging problems.'⁶⁷ Medawar's explanation for this coined a phrase that would become the title of a key set of essays on scientific practice: 'if politics is the art of the possible, research is surely the art of the soluble.'⁶⁸ In his unpublished preparatory notes, however, we can see the roots of Medawar's objections far more clearly. Here, Medawar put his points rather more bluntly: many of the topics that Koestler complained were neglected by scientists, such as 'the genetics of behavior,' were, Medawar argued, left alone simply because they were 'bl**dy difficult.'⁶⁹

In the aftermath of Medawar's review, Karl Miller, the high-profile literary editor at *New Statesman*, printed Koestler's response to the original review — a letter that 'struck me [Miller] as exceptionally weak' — and then invited Medawar to continue the exchange.⁷⁰ Although Medawar agreed, his response to Miller was exasperated: he began, 'Curse Koestler!' He also strongly encouraged Miller to put an end to 'the Koestler commitment' as soon as possible.⁷¹ Both Medawar's reaction to the prospect of continued public debate and his preparation for the review reveal a disdain for the amateurish nature of Koestler's foray into scientific writing. In his unpublished notes, Medawar bemoaned 'the work of an amateur,' for 'K[oestler] quotes with satisfaction from [American zoologist Charles Manning] Child just those generalizations which

I can remember revolting against as a student.⁷² Elsewhere, although he acknowledged that ‘Koestler is excellent on Darwin’s claim to have been a “true Baconian”... [and] excellent too on the popular conception of the scientist as an ice-cold logician and analyst,’ other sections were ‘comically inept,’ displaying ‘AMATEURISHNESS...[with] many, many errors of fact or interpretation.’⁷³

Koestler was not deterred, nor was his relationship with Gábor damaged by the overwhelmingly negative reception of *The Act of Creation* within professional circles. The denunciation of his lack of familiarity with scientific practice and the latest research in relevant fields, especially psychology, rather inspired Koestler to embark on a new book—the final one of his ‘trilogy’—intended to target the ideologies which for him had become symbolic of the bankruptcy of society and the impotence and arrogance of science: behaviorism, reductionism, and neo-Darwinism.

Ultimately, the next book became one of his better-known and most controversial later works: *The Ghost in the Machine*. In a series of amusing side-swipes at his twin objects of attack, Koestler reiterated his earlier arguments about the necessarily hierarchical structure of everything from language and military units to cellular and mental processes in the body. He reformulated a series of criticisms of neo-Darwinism drawn largely from the perspective of Lysenkoism and attempted to rehabilitate a radically teleological form of Lamarckism that had seemingly long since been rendered obsolete.⁷⁴ During the preparation of the manuscript, Koestler once again turned to Gábor for assistance. Despite the increasingly polemical tone of the work, Gábor received the nearly completed manuscript in July 1967, just a few weeks before its scheduled publication.⁷⁵

It was at this stage, however, that the scientific outlooks of Gábor and Koestler began to drift apart. Gábor replied a few days later with a contradictory message; on the one hand, he had been ‘clucking with approval’ for almost the first 200 pages of the book; however, he then came across a series of egregious errors on the origin and meaning of the second law of thermodynamics when he ‘started to wince.’ He urged Koestler to ‘correct this and the few pages after, or physicists will be down on you like a ton of bricks!’⁷⁶ For Gábor, *The Ghost in the Machine* had seen Koestler leave behind the ground that had been so impressive in his earlier writings. This prompted regret from Gábor, who wished that Koestler ‘had rather tried to continue the line of thought which he started in his first book [‘Insight and Outlook’]; the introspective analysis of his own creative mind.’⁷⁷ In simple terms, Gábor later wrote, with ‘The Ghost in the Machine’ ‘Arthur took a line which I can only consider as a-scientific.’⁷⁸

A MEETING OF MINDS? WADDINGTON, BERTALANFFY, AND SMYTHIES AT ALPBACH, 1968

By the time that Gábor and Koestler parted intellectual ways in the late 1960s, Koestler had established a far wider network of advisers and supporters from across the scientific community. When Gábor alerted him to mistakes in the draft of *The Ghost in the Machine*, for example, Koestler was able to respond confidently that ‘[t]he Bertalanffys were here to visit us, and he [Ludwig von Bertalanffy] caught it. So that bit is all right and the catastrophe has been avoided.’⁷⁹

Koestler had already been striving for recognition from the scientific community for more than 20 years when he convened the Alpbach Symposium in 1968. *The Ghost in the Machine* had been published the previous year and represented Koestler’s most forthright attack on what he perceived to be the dominant ideology within the biological sciences: an aggressive and damaging form of reductionism. With support from a few key acquaintances, Koestler drew together an impressive array of researchers from across a range of scientific disciplines for the meeting at Alpbach, which he hoped would achieve two goals. The first was an articulation of a stronger and more unified expression of anti-reductionism that might more effectively challenge his three principal objects of opposition: neo-Darwinism, behaviorism, and reductionism. The second was simply recognition by and inclusion within the scientific community. However, as the proceedings of the Alpbach Symposium make clear, the scientists with whom Koestler collaborated were themselves intellectually and disciplinarily diverse, and his desire to orchestrate an effective opposition to ‘the neo-Darwinian orthodoxy’ proved too ambitious. We can get a sense of the diversity within the anti-

reductionists at Alpbach by looking briefly at three key attendees: C. H. Waddington, von Bertalanffy, and J. R. Smythies.

At the time of the Symposium, Waddington was Professor of Animal Genetics at the University of Edinburgh, a post he held from 1947 until his death⁸⁰. He later recalled how, while an undergraduate at Cambridge, an introduction to the process philosophy of Alfred North Whitehead and the earlier theoretical work of Henri Bergson had a profound influence on his intellectual development and his approach to biology.⁸¹ For Waddington, ‘a scientist’s metaphysical beliefs are not mere epiphenomena, but have a definite and ascertainable influence on the work he produces.’⁸² While at Cambridge, Waddington, along with select others, including Joseph Needham and J. H. Woodger, formed the Theoretical Biology Club, a group of left-wing radicals who were collectively opposed to what we might term the gene-centered view of evolution and, in more general terms, reductionism in biology.⁸³ After a period of research under Thomas Hunt Morgan in 1935, he returned to Cambridge, where in 1942 he coined the term ‘epigenetics’ to describe the study of extra-genetic factors that influenced the developmental pathways of an organism.⁸⁴

Waddington was therefore already committed to a perspective on the biological sciences that not only rejected the central tenets of reductionism but also embraced the importance of philosophical perspectives in driving research agendas. For Ludwig von Bertalanffy, the anti-reductionist agenda stemmed from similar beginnings during his earliest work on General Systems Theory (GST) in the 1930s.⁸⁵ Koestler was a great admirer of von Bertalanffy, who, like Waddington, had been profoundly influenced by philosophy in his early career and had provided both practical and theoretical contributions to biology.⁸⁶ For example, von Bertalanffy devised mathematical formulae to describe the growth of an organism over time, providing inspiration for ecologist Howard Odum in the 1960s, as well as developing wider applications of his GST to other areas throughout his career.⁸⁷

In contrast with the clear metaphysical basis of the anti-reductionism of Waddington and von Bertalanffy, the approach of Koestler’s co-editor of the Alpbach proceedings, J. R. Smythies, stemmed far more from his biological research. Smythies —now the only surviving Alpbach participant— studied medicine at Cambridge and University College Hospital before specializing in neuropsychology, particularly the psychology of perception.⁸⁸ In his own terms, his anti-reductionism ‘added a dimension’ to his research agenda, but his publications on ‘the reductionist debate were separate.’⁸⁹ He had first met Koestler at the Society for Psychical Research in London during the early 1960s, and after several such encounters, during which discussion was particularly focused on the use of hallucinogenic drugs, he was invited to participate a few months before the Symposium.⁹⁰ Smythies’s own expression of anti-reductionism can perhaps best be seen in his review of Francis Crick’s *The Astonishing Hypothesis*, in which Smythies bemoans Crick’s reliance on ‘a series of all-pervasive but totally false folk beliefs about how perception works’: his attacks on Crick stemmed not from ideological disagreement but from empirical research findings.⁹¹

Waddington, von Bertalanffy, and Smythies were, on the surface, an ideal fit for the Alpbach Symposium. However, after Waddington presented on ‘the theory of evolution today,’ Koestler was left disappointed that Waddington’s enthusiasm for the possibility of a Lamarckian mechanism for natural selection had waned considerably compared to his earlier work.⁹² The rest of the Symposium followed in much the same vein: rather than presenting a single, unified response to the tyranny of the reductionists, the picture that emerged was fragmented and disjointed. Nevertheless, from the published proceedings, edited jointly by Koestler and Smythies, it is clear that Koestler was able to engage with his scientific kindred spirits on a more-or-less level intellectual playing field. He pressed von Bertalanffy for more details, robustly stood his ground when questioning Waddington, and responded positively to Paul MacLean, who gave a paper extolling the potential of the ideas presented in *The Ghost in the Machine*. In Smythies’s words, in the reductionism debates, ‘everyone was an amateur.’⁹³

Koestler was simultaneously at the heart and on the periphery of debates about the nature and future of the biological sciences. As an outsider, he was extraordinary in his capacity to engage in theoretical debates about the sciences and their explanatory power in relation to the human condition, yet he also suffered from accusations of amateurism. Responses varied from placation to hostility. However, the legacy of both

his work and the ideas expressed at Alpbach continue to have important ramifications for a wide range of academic disciplines, both within and beyond the sciences.⁹⁴

CONCLUSION: BEYOND ‘THE KOESTLER PROBLEM

In his book *The Return to Cosmology* (1985), Stephen Toulmin dwelt at length on what he termed ‘the Koestler problem.’ This is essentially the view that ‘behind and below [Koestler’s] arguments about behaviorism and evolution and statistics there lies a deeper, and an essentially philosophical, view of the world.’⁹⁵ Toulmin, a long-time critic who had been engaged in public disagreements with Koestler at least as early as 1968, provided a detailed and persuasive account of the shortfalls of Koestler’s scientific worldview⁹⁶. Indeed, the vast majority of the explorations of Koestler’s relationship with science have been either critiques of his approach from philosophical or scientific perspectives, or biographical accounts according to which he failed to garner the credibility which he so desperately sought from professional scientists. For example, in an unpublished manuscript dating from around 1969 and bundled with Francis Crick’s papers, Robert Olby noted that when he ‘read the works of anti-reductionists like Wigner, Polanyi, Elsasser and Koestler, I feel seriously disturbed ... these critics of molecular biology are, it seems to me, pursuing red herrings.’⁹⁷ However, when we leave aside the assumed naivety of Koestler’s scientific work and instead view him as a figure around whom an appreciable number of influential figures of science gathered, several important things become clear.

First, Koestler enjoyed continued and surprising support from several eminent researchers who made significant contributions to their respective scientific fields throughout his career. Alpbach represented one of the only occasions when a representative cross-section from this community came together in an attempt by the architect of the meeting to bring unity to the anti-reductionist set. However, just as the idea of a monolithic entity called ‘reductionism’ belied the tremendous diversity of its adherents, so the hope for a unanimous voice from anti-reductionists floundered in the face of subtle yet profound ideological differences: Koestler, for example, articulated an anti-reductionism that was distinct from holism. Examination of the interactions between Koestler and his ‘Alpbach mafia,’ which was a wider network and included figures such as Burt and Gábor, reveals an inherent plurality among the anti-reductionists who sought to displace and destabilize the perceived dominance of neo-Darwinism.⁹⁸

Second, although Newth, Goodfield, and others have downplayed the impact of scientists’ metaphysics on their everyday research, it is clear that in the case of both the ‘Koestler clique’ and their reductionist opponents, the battleground of philosophy was just as important as individual theories of perception, cognition, or evolution. The very public arguments between Koestler and his critics—chief among them Peter Medawar and Stephen Toulmin—demonstrate the significance of the points at issue in these debates. That both felt the need to continue debunking Koestler’s ideas from, respectively, a scientific and a philosophical perspective showed that there was a very real sense of anxiety at the heart of the reductionism/anti-reductionism debate. At stake was far more than a set of abstract truths about the natural world; the motivation was nothing less than an attempt to reclaim wider authority to speak on behalf of science. In Koestler’s case, his theoretical framework stemmed from his earlier political beliefs—reflecting an anti-communist, anti-Zionist stance rather than adherence to a specific doctrine—and spoke to the organization of society as well as the whole realm of the biological sciences.

Third, when we look to the impact of philosophical positions on scientific theories (and vice versa), we see that for both Koestler and his set, including C. H. Waddington, Ludwig von Bertalanffy, and J. R. Smythies, central elements of their work lay at the intersection between science and philosophy, with important reciprocal implications for both.⁹⁹ Uncovering the significance of these influences challenges historical disciplinary boundaries and shows still deeper connections between cognitive sciences, metaphysics, and politics, explored in the American case by Cohen-Cole.¹⁰⁰

Harold Harris, the former literary editor of the *London Evening Standard*, asserted that for Koestler ‘it has always seemed to me that recognition of his contribution to science has meant far more to him than the much wider international reputation he enjoys as the creator of one of the great novels of the

twentieth century.’¹⁰¹ Given Koestler’s commitment to scientific issues over a long period, it is important for historians to take seriously his relationship with the wider community of scientists with whom he engaged. There were important reciprocal influences: several of the most prominent neglected figures from twentieth-century science enabled Koestler to explore questions often considered beyond the reach of the amateur, and led him directly into the territory of, and into conflict with, other public authorities on scientific practice, especially Peter Medawar. Episodes that called into question scientific credibility and the epistemological status of experimental observation and replication —such as the much-publicized debate about E. J. Steele’s evidence for somatic selection in the late 1970s and early 1980s— saw Koestler acting alongside Medawar, Karl Popper, and others as a respected arbiter and scientific commentator.¹⁰²

The approach to biology promoted by Koestler drew on the long tradition of holistic thought from Virchow’s concept of the ‘cell state,’ through to Gestalt psychology and the many manifestations of holistic thought in interwar German science identified by Anne Harrington.¹⁰³ A deeper understanding of the reinterpretation by Koestler and others of this approach to living systems sheds new light on how anti-reductionist thought was reconfigured at a time of increasing dominance for molecular, mechanistic explanations of biological phenomena. Koestler represented one of the most vocal of a number of Hungarian-born intellectuals and scientists who promoted anti-reductionism in the second half of the twentieth century. The opposition of his friends Michael Polanyi and Dennis Gábor, and fellow-Hungarian Eugene Wigner, to the reductionist orthodoxy might even highlight a manifestation of what Medawar privately characterized as the misguided views of ‘uninformed biologists, particularly on the continent.’¹⁰⁴

At the same time, in an era dominated by discussion of C. P. Snow’s ‘two cultures,’ Koestler—who unsurprisingly argued against this ‘absurd division’ in *The Act of Creation*—sought to mount a very public challenge against the perceived dominance of a dangerously reductionist biology and gain authority to question what he regarded as an unpalatable scientific orthodoxy.¹⁰⁵ In doing so, this outsider encountered strong resistance but was also successful in bringing together disparate groups of researchers from across and beyond scientific disciplines; their interactions reveal the complex ways in which politics, ideology, and philosophy informed and were informed by the practice of science in the mid-twentieth century.

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