In January 1940, Konrad Lorenz wrote to one of his mentors, the Berlin ornithologist Oskar Heinroth, concerning the controversial views of an anthropologist named Georg Schwidetzky. What Schwidetzky claimed was that the human species was born of the crossing of different ape species, themselves the products of earlier crossings. Lorenz was appalled:

Regarding the poor madman [Schwidetzky]: just as a sex killer is, so is a poor madman, who personally can do nothing about his deficit mutations [Ausfallsmutationen]. Since he however is enormously harmful for the people as a whole, one slaughters him justly! I have sent the whole Schwidetzky package to Greite, who is just now very busy with the authoritarian suppression of trash in biological literature. The manner and way Schwidetzky derives us from hybrids out of hybrids (what an idea he has of the fertility of crossbreeds! Try once to cross the baboon and the gibbon!) is nothing short of propaganda for crossing human races, racial shame [Rassenschande] on the large scale. It will be forbidden immediately ....

Hard to say, really, which element here is most nauseating: the devotion to racial purity; the crass biologizing of human action; the damning of an opponent not merely as wrong but as mad, diseased; the notion that

censorship is the duty of those who know best; the pleasure in the exercise of power over others; the incidental talk of sex and death ("sex killer," "one slaughters him justly!"); the chummy enthusiasm – all those exclamation marks – and sham pity. When he wrote these words, Lorenz was a struggling youngish zoologist, based in Vienna, with a small but growing circle of admirers interested in his proposals about innate behavior patterns and their study. He was also a member of the Nazi Party; and by the time his proposals had grown so successful they garnered him a Nobel Prize, in 1973, his Nazi past, including the articles he had written for Walter Greite’s journal Der Biologe, had become an embarrassment. Small wonder the passage quoted above was left out of the version of the letter published in a 1988 volume collecting the Lorenz–Heinroth correspondence. Habits of authoritarian suppression die hard.

So rich in archival revelations is Richard W. Burkhardt, Jr.’s magnificent history, Patterns of Behavior: Konrad Lorenz, Niko Tinbergen, and the Founding of Ethology, that the matter of Lorenz’s letter (pp. 261–262) is easily overlooked. I draw attention to it for two reasons. First, it is a fair token of Burkhardt’s unflinching attitude towards Lorenz’s Nazism, dealt with at length in the book’s central chapter. Burkhardt is not, it should be emphasized, out to vilify Lorenz or to reduce ethology – the biological science of animal behavior launched by Lorenz and his Dutch collaborator and co-Nobelist, Niko Tinbergen – to Lorenz’s attempts, over a comparatively short period, to make ethology an echt Nazi science. But, as Burkhardt shows, those attempts mattered a great deal to contemporaries, trying to decide after the war what sort of endeavor ethology was and whether they wanted any part of it. Answers arrived at varied from place to place, according to local intellectual and institutional milieux which Burkhardt seeks to reconstruct – “ethology’s ecologies,” he calls them. He brings this ample contextualizing perspective to the whole of the disciplinary history he tells, from diverse beginnings in the USA, Britain, and the German-speaking lands in the early twentieth century through to diverse transformations and endings in the 1950s, 60s, and 70s. But it is his chapter on Lorenz and National Socialism where the approach yields its most valuable dividends. Building on the studies of Theodora Kalikow, Ute Deichmann, and Klaus Taschwer and Benedikt Föger, among others, Burkhardt clarifies the opportunities for disciplinary advancement under the Nazis and the mix of ideological affinity and amoral opportunism that made Lorenz so responsive.

The letter about Schwidetzky tells us a lot about Lorenz; but it equally well signals the quality of no-stone-unturned scholarship behind
Burkhardt’s book. Over the last quarter century, he has searched the archives, read the published works (in four languages), and interviewed many of his “actors.” Several of them, including Lorenz and Tinbergen, are now dead. So long was the book’s gestation, Burkhardt explains, that it even became part of the history it was chronicling, in the form of a 1982 letter addressed to him but never sent, found only a decade later among Tinbergen’s papers. In three single-spaced pages, Tinbergen energetically set out what he saw as his own distinctive contribution in relation to Lorenz’s. More so than in the briefer, milder edit of the letter Burkhardt received, Tinbergen here revealed how unrelaxed he was, even post-Nobel, about being seen as the instrument of Lorenz’s ideas, experimentalist brawn to Lorenz’s theoretical brain. Behind the concern lay more than just a creative scientist’s desire for due credit. The letters quoted in Burkhardt’s book and also in a fine biography from Tinbergen’s former student Hans Kruuk, *Niko’s Nature: A Life of Niko Tinbergen and His Science of Animal Behaviour*, show how psychologically fraught the two men’s relationship became after a war which Tinbergen had spent in defiance of the Nazis. The postwar replacement of Lorenz’s “classical” agenda for their science with Tinbergen’s rather different one was, we come to see, part of ethology’s de-Nazification, and at some level a punishment Tinbergen levied on Lorenz for his betrayal.

_Patterns of Behavior_ will be the standard historical work on ethology for a long time to come, so comprehensive is its coverage, exhaustive its scholarship, and finely calibrated its analysis. But it is a book of even more general interest, since the question Burkhardt asks of ethology – roughly, why it formed and developed as, when, and where it did, and not otherwise or elsewhere – can be asked of any scientific discipline. Before turning to Lorenz and Tinbergen, Burkhardt looks in some detail at the lives and works of a number of American and British scientific men of a previous generation, all of them interested in the natural behavior of birds, all of them responsible for ideas and attitudes later recognizable as “ethological,” yet none the founder of a new, behaviorally oriented biological science.

First up are Charles Otis Whitman and Wallace Craig. In a much-discussed lecture at Woods Hole in the late 1890s on animal behavior, Whitman noted how complex behaviors could serve as clues to evolutionary relationships among closely related species, so long as the exact forms those behaviors took were not adaptive in their own right but merely non-adaptive legacies, pointing towards a common ancestry. A powerful figure in American academic biology, Whitman dedicated his
declining years to the study of home-raised pigeons, mostly observing them but also doing occasional experiments, demonstrating at one point that individuals of one species raised among individuals of another will prefer to mate with the latter — an instance of what Lorenz would call “imprinting.” But Whitman published virtually nothing of this work in his lifetime. Craig was Whitman’s student, also taking up pigeon behavior but with distinctive emphases on its biological functions and the interconnections of instinct, movement, and environment. The fact that Craig’s classic paper of 1908, “The Voices of Pigeons Regarded as a Means of Social Control,” appeared in the *American Journal of Sociology* becomes, after Burkhardt’s treatment, a symbol of the general awkwardness of fit between the sort of research Whitman and Craig were doing and the institutional map ordering biological knowledge in their America. Whatever potential for disciplinary innovation there may have been went unexploited, with Whitman past his prime and Craig too self-effacing and encumbered with teaching to do much in that way.

The situation in Britain was different but the disciplinary non-results the same. Here Burkhardt draws attention to three amateur ornithologists, Edmund Selous, Henry Eliot Howard, and Frederick Kirkman, and one dilettantish professional, Julian Huxley. All were pioneers in the close description of the natural behavior of birds observed for prolonged periods in the field, although only Huxley, with his 1914 paper on the courtship habits of the great crested grebe, is well remembered for it. There is nothing surprising in finding that people working outside the universities, or flitting around inside them, never seeded a new discipline. What is surprising — and absorbingly interesting — is to find that their collective raising of the standard of field studies of bird behavior expressed not some swing of the zeitgeist towards “the field,” nor an observation-for-observation’s-sake reaction against speculative evolutionism, but concern to test one of the controversial evolutionary theories of the day, Darwin’s theory of sexual selection. Selous believed that his detailed field diaries vindicated Darwin on the role of female choice. Howard was skeptical, arguing that male displays functioned not to woo choosy females but to ward off male rivals for territory. Kirkman’s great editorial enterprise, *The British Bird Book*, dwelt in such detail on displays in order to throw light, he wrote, on the “vexed question of sexual selection” (p. 99). In his crested grebe paper, Huxley like Howard tended towards skepticism, arguing that the identical displays of males and females, which were followed not by coition but by calm, were best explained as useful for cementing the pair bond formed before the ritual takes place.
Burkhardt largely accepts Lorenz’s claim that he came to know the achievements just described, as well as other, similar ones nearer to home, only after developing his own versions of them independently, first as a boy naturalist with a love of raising birds, then as a medical student whose studies of comparative anatomy led him to explore the potential in behavioral comparisons, then as a doctoral student in zoology engaged in a fundamental rethinking of the nature of the instinctive lives of animals. The partial exception here is the work of the Hamburg zoologist Jakob von Uexküll, whom Lorenz credited as the source of such key ethological notions as “releasers” (patterns of stimuli that conform to “innate schemas” and thereby “unlock” adaptive, instinctive behavioral responses), “companions” (other animals of the same species with whom the animal joins in particular, social-functional activities), and the “Umwelt” (the world as the animal perceives it and acts upon it). Prolific in introducing the technical language around which a new science might coalesce, Lorenz promoted that science in other time-honored ways too. He stressed the advantages of his new biological approach to animal behavior over rival approaches then dominant, in particular the labs-and-learning behaviorism of American animal psychologists and the mixture of subjectivism and vitalism flourishing among their European counterparts. With the help of like-minded recruits, he also gradually took control of a new organization, the German Society for Animal Behavior, turning its organ, the Zeitschrift für Tierpsychologie, into ethology’s house journal. And not least, he published papers so impressive in their combination of observational precision and theoretical reach that there was soon an international buzz around them.

Most immediately important among the recruits was Niko Tinbergen, whom Lorenz first met in 1936, when he was thirty-three and Tinbergen twenty-nine (they had already been corresponding for a year). Less noisily than Lorenz, Tinbergen had been building up a complementary research program, emphatically biological in its approach to animal behavior but – in keeping with specifically Dutch traditions in natural history – much more closely tied to observation in natural settings and controlled experiment. In some ways Tinbergen’s was the more successful enterprise at this point. Unlike Lorenz, struggling to find university support, Tinbergen had glided into a post in the zoology department at Leiden, where he persuaded growing numbers of students to join him in examining instinct in species across the animal kingdom, in birds (especially the herring gull) but also insects (digger wasps) and fish (the soon-iconic sticklebacks). What he did not
have – and what he prized in Lorenz’s work – was a theory to make unified sense of it all. On meeting, they hit it off; and in the spring of 1937 Tinbergen went to Lorenz’s family home in Altenberg, outside Vienna, for a sustained spell of collaborative research. Among other topics, they investigated the responses of different hand-reared birds to the sight of predators, showing, by the use of cardboard dummies moving on a wire between two trees, that it was sometimes shape and sometimes flight pattern which elicited instinctive escape reactions. Wrapped in Lorenzian explanations, trading the old idea of instinctive movements as glorified reflexes for a new emphasis on an inner energy economy, modeled on hydraulic systems, the Altenberg experiments had an elegance that defined the new science at its exciting best.

When Tinbergen, decades later, sought to identify his debt to Lorenz, he reached for counterfactual language. If not for Lorenz, Tinbergen judged, he would “have remained a piece worker without a comprehensive theoretical approach” (p. 199). It is too little noted how often our most deeply felt reflections on the past involve similar cognitive operations, imaginatively removing or changing this or that person or event or context and “seeing” what the outcome is. To reckon how big an impact Lorenz had made on him, Tinbergen imagined his career in a Lorenzless world. We lack a good understanding of how this mode of historical reasoning works, how it can conjure a more profound knowledge of the facts out of speculative flights away from the facts. But it is a start at least to notice its ubiquity, especially when the stakes are high. In one of the most penetrating passages on Lorenz’s Nazism in *Patterns of Behavior* we find ourselves asked to imagine Lorenz in a setting other than the one he in fact inhabited at the time of the Anschluss, the unification of Austria and the German Reich in spring 1938. First Burkhardt gives us the facts, including extracts from letters Lorenz wrote at the time. To Heinroth in March 1938 (though again the published edition of the correspondence omitted it):

> We all cheer like little children over the ‘Anschluss.’ For scientists it is a release to belong now to the larger Germany instead of to the damned Jesuit rabble. You cannot have the slightest idea what a festive mood reigned and still reigns in all of Austria, especially on the first day. One apparently has to have thoroughly suffered under the black thing [the Schuschnigg regime], in order to understand fully the value of Hitler. The revolutions carried out at the university so far are to that extent clearly to the good, so that one can hope for a golden age for our mutilated departments!! (p. 238)
To Erwin Stresemann, another German ornithological mentor, Lorenz wrote in April 1938 of the golden age he foresaw for ethology in particular, now that Karl Bühler, head of the psychological institute at the University of Vienna (and generous patron of Lorenz), had been jailed, possibly for having lied about his wife’s Jewish ancestry. There was talk of Lorenz taking over the institute – a prospect he relished, not least for the opportunities it would give to promote a version of human social psychology that, he wrote, “would be ideologically welcome, as much welcome as it earlier was unwelcome.” It sounds horrible to say it, he went on, but the institute of his dreams, a complementary double institute, in which a psychiatrist and neurologist on the one hand and a comparative zoologist on the other squeeze human psychology dead and put something new in its place, would be something genuine. Above all, something really ‘properly’ German, since I must (in the strictest confidence) say, that human psychology in its modern German versions is always still from an expert’s point of view noticeably derived from the thought of Jewish-babbling, verbose, Jewish leaders. One of the few cases, where I fully acknowledge the perniciousness [Schädlingstum] of the Jews. There are greed-addicted [raffsüchtige] and asocial Aryans enough, but making nonsense of science through multiple discourses, that really [is something that] only Jewish human psychologists bring about. (pp. 240–1)

It is Burkhardt’s great merit to help the reader keep intellectual poise in the midst of such repellent material. With the aid of counterfactual conjecture, he gives us a Lorenz who was and was not a casualty of circumstance. What follows is, in my view, a masterpiece of the historian’s art, well worth quoting in full:

Had Lorenz in 1938 been living outside the sphere of influence of the Nazi regime, or had he been established in his much-longed-for Kaiser Wilhelm Institute, he might never have floated “in confidence” the image of himself as someone who could replace a predominantly “Jewish” human psychology with a new psychology that was “properly German.” Similarly, had he not felt thwarted prior to 1938 by the antievolutionary views of the Austrian Catholics and the Schuschnigg regime, he might not have been so attracted to the new political order that the Anschluss brought. But it would be misrepresenting his mood in March and April of 1938 to portray him, at the time of the Anschluss, as simply trying to make the best of a difficult situation. To the contrary, he saw the
change as extremely promising, and he was ebullient about it. The National Socialists had a clear commitment to viewing human behavior and the body politic in biological terms. His own views on human nature, he believed, were finally going to be “ideologically welcome.” He was confident he could coordinate his own interests with those of the new political order in such a way as to ensure the progress of his research and his career. On 28 June 1938 he applied for membership in the Nazi Party. In his application he claimed: “I was as a German thinker and scientist naturally always National Socialist.” (pp. 241–2)

Was Lorenz right about that? Was his vision of ethology always, in some core sense, as one with Nazi values? Mostly, no. For one thing, all of the characteristic ideas predated Lorenz’s involvement with the Nazi party. Furthermore, it was never Lorenz’s self-conscious ambition to construct a Nazi biology, in the way that his Soviet contemporary, T. D. Lysenko, aimed expressly to construct a socialist one. As Burkhardt stresses, what really engaged Lorenz’s interests was not human politics but animal behavior and his own efforts to understand it and make a living doing so. Yet Nazism and Lorenzian ethology did share a matrix; and in at least one important respect, that common background bred a convergence of opinion which – as the letter about Schwidetzky illustrates – Lorenz later exploited. What was common was a preference for the wild, natural, and instinctive, associated with purity, health, and uniformity, over the domesticated, civilized, and learned, associated with degeneration, disease, and variability. Assiduous as ever, Burkhardt tracks this cluster back to Lorenz’s favorite book from childhood, and quotes the late Fritz Ringer on how, in the German culture of the day, “the young were taught a dangerous respect for their own vital urges” (p. 275). At the same time that Nazi ideologues preached the racial and moral virtues of close-to-nature peasants as against their corrupt, miscegenating counterparts in the cities, Lorenz preached the racial and moral virtues of wild animals, with precision-honed instincts, as against their corrupt, miscegenating counterparts on the farm, where the cross-species mating was both cause and effect of instinctual blurring. He did not take up these views in order to make himself useful to the Nazis; but when he sought to make himself useful, these were the views he exhibited, in letters but also in lectures and published writings, and with the lessons for humankind made explicit.

For a brief time, it worked. In 1941, Lorenz was appointed to a chair in psychology at Königsberg – Kant’s hometown – where an institute for comparative psychology was to be established around him. Later
that year, however, he was drafted into the German military, serving first as physician-psychologist at a hospital in Poznan, Poland, then, from the spring of 1944, as a troop physician on the eastern front, where he was soon taken prisoner by the Russians. Exactly what Lorenz got up to as a Nazi physician is still, I gather from Burkhardt, unclear, though we know he assisted the race psychologist Rudolf Hippius. In prison in Russia, Lorenz wrote copiously, including a manuscript – recently published – in which he now presented the insights of ethology as advancing the dialectical-materialist cause. The moment it suited him to drop the Nazi-ideological front, in other words, he did so, dressing his science up instead in garb from an all-out competitor. His Marxist phase did not last long; but it proved good practice for the years ahead of acting as if his Nazi phase had never happened.

The 1950s turned out to be ethology’s glory years; they take up the bulk of the second half of Patterns of Behavior. While specialists will savor the details of the accounts here of meetings, hirings, and the surrounding hustle and gossip, Burkhardt’s disciplinary focus remains tight, so that out of the particulars there emerges a general answer to the question of ethology’s postwar success. Three developments stand out as especially important. First, the science benefited from powerful patrons and congenial settings in England’s two ancient universities: Oxford, home to Darwinian studies in animal ecology, field ornithology, and, from 1949, Tinbergen; and Cambridge, where William Thorpe, a distinguished entomologist turned Lorenz enthusiast, founded an ornithological field station as a hub for ethological research. Second, Lorenz and Tinbergen used their talents for publicizing their science to the full, chronicling its successes in popular books such as Lorenz’s King Solomon’s Ring (1949, English translation 1951) and Tinbergen’s The Herring Gull’s World (1953), as well as making television appearances and films of their own. Third, and crucially, Tinbergen chose to continue collaborating with Lorenz, despite the latter’s having become, as Tinbergen put it in letters to friends after the war, “nazi-infected” (pp. 283 and 288), and despite Tinbergen himself having spent two years in a Nazi prison camp, arrested after resigning his post at Leiden in protest at the replacement of Jewish and anti-Nazi colleagues. His postwar correspondence testifies to what he called the strong “psychological aversion” (p. 281) acquired towards German scientists in the war years, but also to his optimism that it would, in time, be overcome – at least enough.

Not everyone was as willing to forgive and forget. If one event organizes Burkhardt’s diverse materials in these packed chapters, it is
the publication in 1953 of the American student of bird behavior Daniel Lehrman’s scorching critique of Lorenzian theory and practice. In Lehrman’s view, the innate/learned dichotomy on which Lorenz’s analyses depended was a conceptual disaster, leading ethologists to treat as preformed what in fact develops as the organism interacts with its world – a point Lehrman drove home with example after example. As his paper has now been canonized as an early statement of “developmental systems theory,” and the later friendship between Lehrman and Lorenz is so widely known, it is salutary to be reminded of the paper’s politics; for, as Burkhardt informs us, the rather muted paragraphs that readers now encounter on the weakness of ethological analogies between animal and human behavior – as illustrated by Lorenz’s instinct-theoretic explanations of why German laws restricting interracial marriages were sound and why Asiatic faces strike “us” as “enigmatic” (p. 387) – are vestiges of what in an earlier draft was a much more extensive and impassioned discussion. Characteristically, Burkhardt digs even deeper, restoring to attention not just Lehrman’s motivations but their roots in the ambience and agendas of the Department of Animal Behavior in New York’s American Museum of Natural History, where support for leftwing causes merged with innovative experimental work on the physiological underpinnings of natural behavior. Lehrman was no labs-and-learning man; his attack hit all the harder.

One result of the exchanges with Lehrman was the gradual, retrospective emergence of “classical” ethology, where that honorific at once acknowledged ethology’s debt to Lorenzian concepts and distanced the science from them. No single conceptual structure ever again unified ethologists. Even Lorenz, who continued to defend his ideas, insisted (not very convincingly) that he had never meant talk of “action-specific energies” and so forth to be taken literally. With its original ontology scrapped, ethology nevertheless thrived, and nowhere more so than at Tinbergen’s Oxford, resting point for Burkhardt’s narrative. For Tinbergen, the Lehrman critique became a means of escape from Lorenz’s shadow and an occasion for redefining ethology along lines much more distinctively his own. As Oxonian emphases on the adaptive design of organisms in their ecological settings seeped in, and the old Lorenzian ambition to infer phylogenies from non-adaptive traits faded, Tinbergen and his students came to be known above all for field experiments revealing the survival value in even the smallest behavioral details. At an international congress in 1957, Tinbergen spoke of transformation. “I feel that I have gone, like a pupating caterpillar,
through a phase of breakdown and reorganization, and that I am now more or less like the butterfly hatching something new...“ (p. 405). His many intellectual offspring, and the work they pioneered or inspired in sociobiology, behavioral ecology and cognitive ethology – classical ethology’s most visible disciplinary successors – are the subjects of the final pages of Kruuk’s biography.

If I have said little here about the latter, it is because, although published first, it now functions as an amiable companion to Burkhardt’s book, right down to Kruuk’s quotation at length (pp. 301–303) of Tinbergen’s unsent letter to Burkhardt (who provides only a paraphrase). Kruuk adds much fascinating detail to the story, not least with his insider’s sense of Dutch Calvinism and its shaping influence on Tinbergen. Those seeking more challenging companionship should reach instead for Robert Boakes’s history of labs-and-learning comparative psychology, From Darwin to Behaviourism: Psychology and the Minds of Animals (Cambridge, 1984). So different are their casts of characters that it takes some effort to join up Boakes’s history with Burkhardt’s. But the occasional points of overlap are all the more intriguing. Consider, for instance, that Edward Thorndike, begetter of the psychologist’s puzzle-box experiment, was a regular at Whitman’s Woods Hole, and wrote enthusiastically about Whitman’s proposals concerning behavior and phylogeny. Or that Karl Lashley, so much a guiding spirit in the animal behavior work being done at the American Museum, collaborated productively with the arch behaviorist John Watson. Or that Karl von Frisch, awarded the Nobel alongside Lorenz and Tinbergen for famous studies of the “dance language” of bees, barely figures in either book. Vital though disciplinary histories are, the history of a science, as these examples suggest, is larger than the disciplines it contains. With the commanding work of Boakes and, now, Burkhardt to hand, the task of mapping the modern science of animal behavior as it developed within but also between, in spite of, and in isolation from its major disciplines can begin in earnest.