
From evolutionary theory to philosophy of history: Raymond Aron and the crisis of French neo-transformism

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Abstract

Well into the 1940s, many French biologists rejected both Mendelian genetics and Darwinism in favour of neo-transformism, the claim that evolution proceeds by the inheritance of acquired characteristics. In 1931 the zoologist Maurice Caullery published *Le Problème d'évolution*, arguing that, while Lamarckian mechanisms could not be demonstrated in the present, they had nevertheless operated in the past. It was in this context that Raymond Aron expressed anxiety about the relationship about biology, history, and human autonomy in his 1938 *Introduction à la philosophie de l'histoire: essai sur les limites de l'objectivité historique*, in which he rejected both neo-Kantian and biological accounts of human history. Aron aspired to a philosophy of history that could explain the dual nature of human existence as fundamentally rooted in the biological, and at the same time, as a radical transcendence of natural law. I argue that Aron's encounter with evolutionary theory at this moment of epistemic crisis in evolutionary theory was crucial to the formation of his philosophy of history, and moreover that this case study demonstrates the importance of moving beyond the methodological divisions between intellectual history and history of science.

Keywords

Raymond Aron, Maurice Caullery, evolutionary theory, French biology, philosophy of history

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On March 26th, 1938, a young French doctoral candidate named Raymond Aron defended his dissertation in front of a baffled, if impressed, audience. Aron's *thèse secondaire*, *Essai sur la théorie de l'histoire dans l'Allemagne contemporaine*, was a forceful critique of German sociology. Even more inflammatory to his committee was his *thèse principale*, *Introduction à la philosophie de l'histoire: essai sur les limites de l'objectivité historique*, which was an attempt to overcome the French neo-Kantianism of his teachers, some of whom were in the room (*Revue de métaphysique et de morale*, 1938).

Aron's *Introduction à la philosophie de l'histoire* was remarkable not only as a philosophical achievement that announced the arrival of a major thinker of the 20th century, but also as a turning point in the relationship between philosophy and science in France. A decade earlier, Aron had begun this thesis as an analysis of the epistemology of Mendelian genetics, a project designed to fit within the idealist framework of his education. But the debates about genetics and evolutionary mechanisms, which were ongoing and in France throughout Aron's education, did not lend themselves to purely formal philosophical analysis. As Aron's interest shifted toward the philosophy of history he discovered that biologists and humanists were facing parallel explanatory limits. His thesis on genetics became instead a thesis on the philosophy of history.

By the time he completed the dissertation he was convinced that evolutionary theory and the philosophy of history had run up against the same epistemic stumbling blocks, namely an inability to theorize the relationship between observation and experience in the present and larger-scale accounts of the past. Thus Aron's project was not only the marker of a generational shift in French thought,¹ it was also a turning point in French philosophy's relationship with biology, marking the beginning of a period in which, rather than looking on as outsiders writing chapters in the history of rationality, philosophers began to see contemporary biology as an essential conceptual resource. Aron's thesis was thus an attempt to articulate a philosophy of history that could explain the dual nature of human existence as fundamentally rooted in the biological, and at the same time, as a radical transcendence of natural law. This work shocked the audience that day because Aron's critique of historical positivism was still a radical position in the French academy at the time. Aron argued that the historian's perspective necessarily shaped his understanding of the past in ways that were both limiting and ultimately enriching. In other words, perspectival limits were constitutive of human history, not an obstacle to be overcome on the path to a more total narrative. Though, as I argue below, Aron was not advocating true relativism, his interest in the limits of objectivity was interpreted as an audacious criticism of his teachers.²

Raymond Aron (1905–1983) was perhaps the most famous French liberal thinker of the 20th century. Philosopher, sociologist, journalist, Aron has mostly been celebrated as a champion of liberal reason against 'the totalitarian temptations of the age' (Judt, 2007: 137). Recent scholarship has also situated Aron's thought within a more complex genealogy of French liberalism (Steinmetz-Jenkins, 2014). This work has naturally focused on Aron's postwar writings. While some historians have emphasized the importance of Aron's *Introduction à la philosophie de l'histoire* in the formation of his methodology (Colquhoun, 1986; Davis, 2009; Fessard, 1980; Mesure, 1984), few have examined this work in its own terms. Reading *Introduction à la philosophie de*

l'histoire in its context, by which I mean not only Aron's biography or the European political climate of the thirties, but also the disciplinary context in which a philosophy of history came to seem important to Aron, brings to light another conversation in which Aron was participating: the ongoing debate about evolution and transformism in France. Attention to Aron's interest in evolutionary theory certainly helps us to understand the formation of an influential intellectual, but more importantly it brings the salience of the life sciences back into the foreground of the cultural and intellectual history of interwar France.

In what follows I situate Aron's thesis within the context of French debates about evolutionary theory. I argue that, though Aron did not ultimately write a thesis on Mendelism, it was his preliminary research into genetics and evolution that brought him to the philosophy of history. It was moreover the specific timing of Aron's foray into biology, in a period of crisis and epistemic upheaval among neo-transformists, that determined his interpretation of evolutionary theory in historical terms.

Plasticity and heredity in French neo-transformism

In 1928, Aron passed the *agrégation*, the national qualifying exam, with the highest score given that year (Aron, 1990: 25). When it came to the choice of a thesis topic, he settled on the epistemology of Mendelian genetics. His notes from this period have not survived, but in his memoirs he describes how he 'went to' the laboratory and 'read many books' on biology (ibid.: 38).³ At that time, a generation of biologists trained by the ethologist Alfred Giard (1846–1908) were active in Paris. The zoologist Étienne Rabaud (1868–1956) was the chair of experimental biology at the Sorbonne. Maurice Caullery (1868–1958), also a zoologist, was chair of 'the evolution of organized beings'. Giard had conceived of his project as a corrective to the tendency, since the work of Claude Bernard, to distinguish too sharply between morphology and physiology (De Bont, 2010). With regard to evolution, Giard saw his work as combining the ideas of Charles Darwin with those of Jean-Baptiste Lamarck. The resulting theory was arguably more Lamarckian than Darwinian, since Giard ended up reducing natural selection to a secondary process, always subject to the organism's interaction with its milieu.

After Giard's death, Caullery and Rabaud diverged sharply on matters of evolutionary theory. By the late twenties, in the wake of decades of failed and inconclusive experiments, Rabaud in particular moved into the realm of polemic. Caullery came, with reservations, to accept Mendelism. Experimental results were strongly in favour of Mendelian genetics, but this convinced him only that genes played a role in morphology, not in the evolution of species. Caullery continued to argue that, though they could not be reproduced in the present, Lamarckian mechanisms must have operated in the past. This claim, that there had been a rupture in natural law, provided a crucial hinge for Aron's philosophy of history, as I argue below. Where the neo-Kantian position implied an ontological/temporal rupture between human and natural law, the neo-transformist position came down to asserting a natural law that, while not demonstrable in the present, had nevertheless operated in the past. The question of history remained an inaccessible middle term.

The trajectory of Aron's interest in biology throws into relief the distinction between his approach to science and that of earlier generations of French philosophers. For Bergson in particular, philosophy had been an epistemic alternative that complemented the truths of science. Thus, whereas for Bergson philosophy came to the aid of science, in the case of biology adjudicating between conflicting concepts of the living, for Aron it was philosophy that needed help. Biology did not ultimately provide the solution, but it offered him a resource for thinking beyond the limits of French neo-Kantianism. In the *Introduction*, Aron went to great lengths to show how biology, the 'most interesting' example of an historical discipline, nevertheless lacked a true account of human history (Aron, 1981: 27). This was so because, as a 'mode of inference' about the relationship between past and present, evolutionary biology had no way to conceptualize the relationship between succession, the fact of change over time, and continuity, a *meaningful* explanation of those changes (ibid.: 29).

The twenties were a period of turmoil, frustration and polemic in French biology. Darwinism and Mendelism were still largely rejected, but neo-transformist models were beginning to crumble as well. As Laurent Loison (2011) has shown, plasticity – the organism's response to its environment, its development, self-adjustment and adaptation to its surroundings – was for many decades far more central than the question of heredity. When heredity did become a question, the focus was on soft inheritance, or the inheritance of acquired characteristics, and not on selection. It is to some degree this resistance to exploring the role of selection that marks late 19th and early 20th-century French biology as neo-Lamarckian.

In other words, French biologists were not strictly speaking followers of Lamarck. They were also inspired by two others of their countrymen, Claude Bernard and Louis Pasteur, which contributed to their experimental focus on physiology and microbiology. The theoretical framework of neo-Lamarckism was more mechanical than Lamarck's theory. Zoologist Frédéric Houssay (1860–1920) tried to demonstrate experimentally how the bodily structures of fish species were formed over time as the result of water pressure. Botanists Gaston Bonnier (1853–1922) and Julien Constantin (1857–1936) showed with some success how abiotic conditions acted directly on the morphology of plants. These experiments were all meant to demonstrate that organisms were plastic, and that changes occurred through individual responses to the environment, not through natural selection. Whereas for Lamarck characteristics were acquired as the result of 'a progressive internal, mechanical force', the organism's active *response* to the environment, neo-Lamarckians emphasized the action of the physical environment on the organism (Loison, 2011: 73).⁴

Loison argues that this prioritization of the action of the environment over that of the organism led to the concomitant prioritization of plasticity over heredity. A tension arose between the need to explain changes, individual morphological adaptations, and to explain stability, or the persistence of species. Phyletic evolution, or the evolution of species, was 'totally reduced to individual changes, and thus the organism was the only relevant level for studying the operations of evolutionary mechanisms' (Loison, 2011: 69). In other words, there was also an epistemic conflict between the unit of experimentation, the individual, and the explanandum, the species. Resolving that conflict required an account of heredity, which the neo-Lamarckians did not have. They believed

that the Bernardian concept of the *milieu intérieur* was a sufficient explanation of heritability. Acquired characteristics would find their way into germ cells. The precise mechanism was not, for them, important.

Why wasn't natural selection accepted as a possible solution? Before the modern synthesis, which brought a wide, if not complete, consensus on the compatibility of genetics and evolutionary theory, most biologists understood selection as purely negative force: they rejected the idea that selection could provide anything beyond the elimination of maladapted traits. In France, 'most biologists accepted [natural selection], but always by reducing its evolutionary role to almost nothing. Natural selection was seen as being responsible for destroying the unfit, but certainly was not responsible for the creation of the fittest' (Loison, 2011: 70). This may have been a misunderstanding of Darwin, but it was also a reflection of a genuine gap in scientific knowledge at the time. It was as yet unknown how it was, by what precise mechanism, novel traits were produced. Darwinian selection acted on phenotypes, but the source of new phenotypes still had to be explained. This was where Lamarck might have proven useful, and indeed seemed for a time to offer solutions as illustrated by the work of Houssay, Constantin, and Bonnier.

There were various attempts to reconcile the tension between plasticity and heredity, but the most significant for the questions Aron would face was Félix Le Dantec's (1869–1917) *La Stabilité de la vie. Étude énergétique de l'évolution des espèces* (1910). Le Dantec argued that plasticity was a property of all life, but that it decreased as complexity grew. That is, while the simplest organisms could be transformed by the environment, as complexity increased heredity became more powerful than plasticity. As Loison puts it, 'Biological evolution was then identified with the universe's thermodynamic transformation: plasticity should follow the same laws as entropy, but in the opposite direction' (Loison, 2011: 73). For Le Dantec, evolution was slowing down, perhaps already at a standstill. Thus, the neo-Lamarckian position found itself deeply at odds not only with Darwinian perfectibility, but also with any ideals of historical progress.

There is no evidence that Aron read Le Dantec's work, and since the biologist died in 1917 Aron could not have visited his laboratory at the Sorbonne. One of Le Dantec's colleagues, however, had a direct influence on Aron's philosophy. Maurice Caullery was not only still professionally active when Aron studied biology, but was also participating in an international conversation about the relationship between genetics, natural selection, and Lamarckism. Moreover, Caullery's 1931 *La Problème de l'évolution* (Caullery, 1931b), which Aron cited heavily in the second chapter of the *Introduction*, 'Natural Histories', was influenced by Le Dantec in important ways. Like Le Dantec had in 1910, Caullery argued in 1931 that evolution was reaching an end. As we shall see, this claim was one of the central problems Aron sought to address through his philosophy of history.

Maurice Caullery's descriptive biology

Caullery did not believe genetics could explain evolution. In an essay in *Science*, he addressed Thomas Hunt Morgan's famous *Drosophila melanogaster* experiments. Morgan (1866–1945), an American, had made huge strides towards localizing genes, arguing

in his 1915 *Mechanism of Mendelian Heredity* that the chromosome was the material basis for heredity. Morgan had also been able to produce dazzling experimental results in favour of Mendelian inheritance patterns. ‘As a result of these magnificent researches’, wrote Caullery, ‘which look upon genes and their localization as tangible realities, a veritable genetical mentality has been created. These, however, have merely imaginary existence’ (Caullery, 1931a: 256). For Caullery, genes were nothing more than a model, not necessarily a description of material reality. ‘They permit of experimentation and prognostication, and this justifies their use’, he wrote, ‘but it must not be forgotten that they are only symbols’ (ibid.).

Caullery’s suspicion of Morgan and the detached ‘genetical mentality’ he championed reflected the state of genetics as a discipline. Morgan and his students had been instrumental in isolating genetics from embryology and medicine, successfully carving out an independent discipline. While the separation of questions of heredity from those of development was critical to the astounding degree of experimental success achieved by Morgan and his students, the consolidation of the identity of their discipline rested on a process of mechanist abstraction, a move away from morphology toward a quantitative method (Allen, 1975: 41–72).⁵ If genetics was a formalization and not a description of material reality, Caullery nevertheless conceded that, ‘Everything happens as if genes were exactly as the geneticists say they are’ (Caullery, 1931a: 256).

In fact one of Caullery’s students, Émile Guyénot, had been instrumental in confirming Morgan’s experiments. Guyénot designed his doctoral research ostensibly to demonstrate that the morphological changes Morgan produced were actually a result of environmental conditions. He created an aseptic, controlled environment for his *Drosophila melanogaster* but his results ended up confirming Morgan’s, and therefore also Mendelian inheritance. As Richard M. Burian, Jean Gayon and Doris Zallen put it in their important article on the history of genetics in France, Guyénot’s work was significant because, ‘In effect, [he] brought genetics before a tribunal composed of Claude Bernard and Louis Pasteur’ (Burian *et al.*, 1988: 376). Caullery was supportive, and appears to have accepted a limited version of Mendelism after this point.

And yet, Caullery was still unwilling to accept genes as the basis of evolution. The problem was two-fold. First, Morgan’s experimental mutants did not produce new species, just recessive traits that faded with time. Second, Caullery did not think genetic mutation could explain the astounding degree of adaptation to their specific environments that species exhibited. While genetics might give a plausible account of heredity, it offered no explanation of plasticity. ‘To explain these facts [of adaption to the environment] it seems to me that it is impossible to reject the direct action of the environment on organisms or the influence of the phenotype on the genotype, that is to say, a mechanism of the kind suggested by Lamarck’ (Caullery, 1931a: 259). Any theory of evolution had to explain the highly coordinated nature of phenotypes, so intricate as to be ‘veritable machines’ (ibid.). Mendelism therefore did little to resolve theoretical obstacles faced by French neo-transformists.

Nevertheless, partly as a result of Guyénot’s work, Caullery had begun to doubt the plausibility of the inheritance of acquired characteristics. As Loison has shown, it was in the period around nineteen-teens that Caullery turned to Le Dantec’s work. Caullery began to believe with Le Dantec that ‘the ability of organisms to vary with

environmental conditions, or their *plasticity*, was a function that decreased with the complexity and morphological specialization of living beings' (Loison, 2010: 182). Caullery's position was further influenced, Loison contends, by Henri Poincaré's 1911 '*L'évolution des lois*', in which the latter discussed the possibility that natural law might change over time.

In 1914 Caullery published an essay in *Revue de métaphysique et de morale* on the nature of biological law. He argued that whereas, 'in the entire range of the inorganic world, in mechanics, physics, chemistry, the idea of law . . . is in fact a universal, uncontested given', this was not the same for biology (Caullery, 1914: 336). If it were, his 'task would be completed' (*ibid.*). Nevertheless the alternative, vitalism, was incoherent, merely a modern form of finalism that rendered experimentation meaningless. Caullery pointed to Claude Bernard's ambivalence about vitalism as symptomatic of this contradiction. On the one hand, Bernard's concept of *milieu intérieure* was a kind of 'intraorganic teleology'. On the other hand, Caullery pointed out, Bernard did not let this undermine experimental determinism. Therefore Bernard would have rejected 20th-century vitalism precisely because it rendered 'positive biology', in other words experimental knowledge, incoherent. Caullery concluded with a call for epistemic modesty, citing Le Dantec's assertion that 'honest' science should merely be descriptive.

Thus, by the time Aron encountered his work, Caullery's position was quite complicated: he had come to accept the existence of Mendelian genetic mechanisms, but not their role in evolution; he had serious doubts about the inheritance of acquired characteristics; finally, his epistemic models were unstable. Perhaps biology should restrict itself to description and give up the search for causes. Or perhaps there were biological laws, but they operated differently in the present than they had in the past. Meanwhile Caullery was diverging ever more sharply from his colleague Étienne Rabaud. Rabaud was extremely influential, and in his memoirs Aron remembered him as having 'declared war, once and for all, on Mendelism' (Aron, 1990: 38). Less willing to accept Mendelian mechanisms than Caullery, Rabaud had by this time come to reject the importance of morphological adaptation altogether.

Étienne Rabaud's rejection of morphology

Unlike Caullery, Rabaud remained resistant to the Mendelian genetic model of heredity. While he was known to complain about the poor quality of the American Mendelian experiments, this was more in the spirit of polemic than a reflection of any actual ignorance about the work of his colleagues abroad. Indeed, Rabaud was well aware of the advances being made across the Atlantic. It was not so much the results that he doubted as the epistemic basis for the experiments themselves.⁶ Rabaud's entrenched hostility to genetics was a philosophical commitment to an understanding of the organism as whole and therefore prior to any positive result of experimentation. This same commitment ultimately led him to abandon morphology as an explanatory register since it produced functionalist accounts that treated parts as prior to the whole.

In 1912, in a heated exchange with Arend Hagedoorn, a Dutch geneticist and proponent of Mendelism who was at the time a genetics adviser at the Vilmorin seed company in France, Rabaud wrote, 'It is not a matter, of course, of casting doubt on the

well-established *facts*, but of discussing their interpretation and above all their application to all biological phenomena' (Rabaud, 1912; Theunissen, 2014). The mode of interpretation appropriate to living things was not, Rabaud suggested, to be found in the language of genes. When Hagedoorn accused neo-Lamarckians of relying on reason rather than experimentation, Rabaud was adamant in his own defence. The French, he insisted, did not cling to neo-Lamarckism because they relied on reason above experimental proof, but rather because 'in observing and experimenting, it is not enough to represent [the] results by an assemblage of letters' (Rabaud, 1912: 124). For Rabaud, understanding the living required understanding organisms as cohesive wholes. Mendelian genetics, he argued, reduced living organisms to mere excited matter.

Having spent much of his career trying and failing to experimentally establish the inheritance of acquired characteristics, by the 1920s Rabaud had reached a turning point. In his 1922 *L'Adaptation et évolution*, Rabaud argued that in fact neither Lamarckian nor Darwinian mechanisms could explain evolution. The former was proving impossible to demonstrate, and the latter simply displaced the question. Even if one accepted that evolution proceeded through the accumulation of tiny variations, what was the source of these variations? 'In contrast to the neo-Lamarckian solution', he wrote, 'the Darwinian solution attributes a very secondary role to the environment and gives way to chance. It implies, nevertheless, a certain direction in the manner in which the variations succeed one another . . . All this happens as though these happy coincidences, as if by chance, tended towards a definite goal' (Rabaud, 1922: 19). For Rabaud, Darwinism implied finalism that was 'seductive and hollow' (*ibid.*: 279).

While Mendelian genetics might have seemed like a solution to Darwinian finalism, Rabaud's adamant rejection of its epistemic foundations led him instead to deny the importance of morphology altogether. As Loison has shown, this rejection of morphology was intricately linked to the neo-Lamarckian inclination towards holism. Thus, ultimately Rabaud came to see the idea of morphological adaptation as incoherent because it required conceptual division of the organism into parts (Loison, 2010: 150–1). He argued that,

The illusion comes from the fact that adaptation is usually considered only from the morphological point of view. In an organism, it is the form that draws first the attention and observers naturally tend to subordinate everything to anatomic dispositions. This mistake commits us to the impasse in which we find ourselves: having established as a principle the agreement between forms and conditions, we are not able to explain the origin of this agreement. (Rabaud, 1922: 71)

For Rabaud, morphology became an incidental by-product of physiology. Whereas Caullery's response to this impasse was to argue that Lamarckian mechanisms had operated in the past, Rabaud simply concluded that the very idea of morphological adaptation was an artefact of sloppy reasoning.

From evolutionary theory to philosophy of history

This was the state in which Aron found French biology when he began his dissertation research. After 18 months of mandatory military service in the French air corps,

he decided to pursue further research in Germany. In March of 1930, Aron travelled to Cologne on an exchange, with plans continue his research while working as a teaching assistant at the university. On a Sunday morning in November of 1930, 8 months after he had arrived in Cologne, he was walking along the banks of the Rhine when he had a moment of clarity, which he would later describe to his friend Paul Bertaux. In a letter to Bertaux dated November 19, 1930, Aron wrote that while walking that day he, ‘saw the connection between the disparate problems that pre-occupied me: philosophy becoming conscious of itself, philosophy chasing the phantom of history, socialism becoming spiritual reality once again by the rediscovery of the desire for values, or what have you, all appeared related’ (Aron, 1985: 281). Aron abandoned the project on genetics that November day, realizing that he could not separate his interest in politics from his philosophical project, and began his work on the philosophy of history. But his encounter with biology loomed large even in this new project, setting the stakes for the problem of historical objectivity that structured the *Introduction*.

Aron’s *Introduction* has often been understood as ‘[promoting] a relativist, anti-Marxist philosophy of history’ (Stewart, 2011: 42). While it is true that Aron was critical of Marxism, as he was of any position authorized by a totalizing account of history, relativism was never his aim. In fact, his epistemological project should be understood as a two-pronged attempt to develop a positive philosophy of history. On the one hand he wanted to rescue the insights of German neo-Kantianism from the tradition’s uncritical acceptance of science as the way out of relativism. On the other hand, he was equally dissatisfied with the epistemic leaps of faith made by his teachers in France. For French neo-Kantians, the leap was from logical to ontological truth. For the neo-transformists, the leap was from observation to mechanism. In both cases, one kind of knowledge was being translated into another without a plausible theory justifying the translation. For Aron, philosophy of history arose to provide such a theory. Thus, to the degree that the *Introduction* was a call for epistemic modesty, a search for the limits of knowledge, it was also an attempt to develop a substantive philosophy of history out of those very limits.

Therefore, rather than placing Aron’s work along a smooth trajectory in the rise of historical relativism, we should instead see it as one of the most sustained mid-20th-century attempts to overcome relativism. Not only did the *Introduction* give new life to the philosophy of history in France (Canguilhem, 1999), it did so in such a way as to make biology absolutely central. Aron challenged both his teachers and his peers to examine the relationship between their own epistemic practices as humanists and those of their colleagues in the sciences.

As a kind of prologue to the *Introduction*, Aron first wrote his *thèse secondaire*, *Essai sur une théories allemande de l’histoire: La philosophie critique de l’histoire*, to demonstrate that German neo-Kantianism could not circumvent the need for philosophy of history. The work was an examination of four German thinkers: Wilhelm Dilthey (1833–1911), Heinrich Rickert (1863–1936), Georg Simmel (1858–1918) and Max Weber (1864–1920). These German neo-Kantians had walked up to the edge of historical relativism but, Aron concluded, they had not been willing to jump. It might appear that historicism, which Aron defined as ‘the doctrine that proclaims the

relativity of values and philosophies as well as historical knowledge', had resulted from the philosophies of Dilthey, Rickert, Simmel and Weber (Aron, 1950: 289). As Aron wrote,

[Dilthey] recognizes the diversity of epochs and kinds of men, the integration of values and philosophies into original ensembles that express the unpredictable transformations of creative life. Weber asserts the unceasing novelty of the values with which men choose their destinies and renew their understanding of the past. Simmel also subordinates the cultural forms to the fate of an irrational force, life. (ibid.: 289)

Nevertheless, Aron contended, these apparent seeds of 'relativism and irrationalism' did not flourish. While these four thinkers placed the idea of a truly universal philosophy into doubt, they all ultimately embraced the truth of positive science. Though he hoped to avoid relativism as well, Aron was also keenly aware of the limits of positivism, alive as he was in particular of the epistemic turmoil within French biology.

At the beginning of the thesis Aron had asked whether it was 'possible to transpose the Kantian method in such a way as to make the philosophy of history unnecessary and to ground an original logic for historical science?' (Aron, 1950: 13). Ultimately he concluded that rather than making the philosophy of history obsolete, the German critique in fact provided an opening for such a philosophy. 'It is in effect', he wrote, 'through the intermediary of methodology that the philosophy of history, condemned by positivism in the second half of the nineteenth century, has regained the right of existence' (ibid.: 9). This 'right of existence' regained, Aron's hope was to exercise that right.

Aron concluded that what the German neo-Kantians ultimately offered was a new set of problems, a pair of contradictions. The subjects of his study all '[recognized] two fundamental antinomies . . . the first between historical relativity, which seems a fact, and universal truth required by reason, and the other between individual perspectives, partial and multiple, and the totality of historical development' (Aron, 1950: 294). These antinomies, the sticking points in a German conversation about methodology and objectivity in social science were, for Aron, an opening for the philosophy of history.

It was for this reason that *Introduction à la philosophie de l'histoire* opened with a clarification about the meaning of objectivity. Aron was not interested in the question of whether individual historians could free themselves of prejudice or preference. He was only interested in 'an ideal historian'. Referring to his title, he wrote that, 'objectivity does not mean impartiality, but universality' (Aron, 1981: 9).⁷ The subtitle, Aron clarified, was not the goal of the project but its starting point. Though he did not mention his *thèse secondaire*, it was clearly this work that provided him with his point of departure. Aron wanted to move from the Kantian question, 'Under what conditions is historical knowledge possible?', to the Weberian formulation, 'Is a universally valid science of history possible? To what extent?' (ibid.: 10). In other words, taking as a starting point the illegitimacy of the transcendental grounds of 19th-century philosophy of history, the task now was to map the limits of historical knowledge. It was precisely the failure of the German neo-Kantians as outlined in *La philosophie critique de l'histoire* that had definitively revealed the inadequacy of transcendental analysis for the philosophy of history in the 20th century.

When Aron turned to the question of natural history, he believed that both Lamarck and Darwin had been proven wrong. It is true that by this time only a small subset of scientists still believed in the inheritance of acquired characteristics. But here Caullery's influence becomes evident, because Aron also claimed that, 'they all consider genetic variation false' (Aron, 1981: 28). Mendelian mechanisms might create 'fluctuation', but they did not constitute true mutations because they did not result in the creation of new species. Palaeontologists had established a set of historical facts; different species have existed at different times, some of which have become extinct. Evolutionary biologists had experimented with various ways to alter organisms, either by Mendelian or Lamarckian methods. Aron's contention was that no one had actually theorized the relationship between these two bodies of knowledge. '[The] fact of succession', he wrote, 'does not logically impose the hypothesis of descent' (ibid.: 29).

Working through Caullery's text, Aron found, with Caullery, that no proof was definitive. For example, the idea that ontogeny recapitulated phylogeny, in other words that embryos developed physiologically by going through previous stages of evolution, was overturned in favour of the claim that 'the future', not the past, 'is already virtually inscribed in the egg' (Caullery, 1931b: 429). Thus, there was no agreement either about the mechanism of change or about the appropriate kind of knowledge that would in fact prove the theory of evolution.

Aron saw a series of indirect proofs, none of them definitive. At the same time, 'the laws of heredity, in particular genetics, suggest that living matter is remarkably stable' (Aron, 1981: 32). The fact was, no one could reproduce evolution in a laboratory. Yes, Morgan, as Aron was surely aware, had produced an array of mutants, but he had never been able to create a new species. Caullery had argued that, 'We can accept . . . with great likelihood, that once stabilized, groups [species] do not give rise to truly new groups' (Caullery, 1931b: 407). Surveying 50 years of experimental attempts had led him to this conclusion. This conflict between material evidence of evolution in the past (fossil records) and the inability to reproduce this process either by Mendelian/Darwinian or Lamarckian methods had resulted in a state of confusion, or, as Aron wrote, a 'crisis' in philosophy (Aron, 1981: 32).

To Aron, biologists appeared satisfied simply to demonstrate the fact of descent.⁸ Moreover, the standoff between transformism and Darwinism had resulted in the kind of anti-finalism exemplified by Rabaud. For biologists, history had become 'a series of events and no longer an intelligible sequence' (Aron, 1981: 33). This 'doctrine of chance' reached its apex in genetics, which made living forms 'reducible to assemblages of vital atoms, of genes' (ibid.). In 1938, Aron could follow by conceding that of course most biologists found this genetic concept of life unsatisfying. Caullery was among them, but Aron pointed out that, in claiming that different mechanisms operated in the past than could be observed today, Caullery was inverting the meaning of history. 'Thus history', wrote Aron, 'first necessary to positively explain finalism, is then invoked to show the unintelligibility of evolution. Contradictory double necessity, but undoubtedly an inevitable contradiction' (ibid.). What was left was a generalized confusion in biology about the nature of the inquiry. Controversy over the mechanism of evolution had led to a theoretically incoherent stance on history itself.

This crisis in French neo-transformism, and by extension philosophy, was the product of the kind of epistemic eclecticism practised by modern biologists. As biologists tried to bring various bodies of knowledge under one overarching theory of evolution, the contradictions between their epistemologies had become impossible to avoid. To Cournot (1912), for whom the history of nature was a reflection of metaphysical order, facts could never contradict theory. But as 20th-century biologists tried to construct histories of species, experimentation, observation and theory were often at odds.

In tracing this conflict, Aron was situating the philosophy of history with respect to the limits of scientific knowledge. However, he was careful to distinguish this project from Bergson's. In Aron's view, Bergson had not gone far enough in identifying the limits of scientific explanation. 'It is no longer about a provisional rupture', he wrote, 'a lacuna in science, it is about recognizing the limits beyond which our objective science cannot go' (Aron, 1981: 45). For Aron the crisis in evolutionary theory reflected a permanent limit of science, not a gap in knowledge that would be filled by further research. Moreover, it was precisely where science ran up against its limits that the need for philosophy of history surfaced.

But it wasn't just that science revealed the necessity of philosophy; it also pointed to what made history itself possible. While man might have evolved out of the animal world, his emergence was a rupture with evolution. Aron distinguished his position from that of materialists, including Marxists, who made claims similar on the surface. Yes, the creation of tools and with them the ability to alter conditions of existence was original to man, the 'first act of history' (Aron, 1981: 44). But the materialists implied that man's history was just an intervention or response to evolutionary conditions. Aron wanted to go further. Human history, he argued, was not just a sophisticated environmental response, but also implied a 'spiritual relationship between individuals' (ibid.).

Thus for Aron, the solution to the crisis of historical meaning was to be found in a new concept of man. For French neo-Kantians, the subject of history was rationality, and he therefore depended on an unsustainable division between human reason and natural law. The neo-transformist position, exemplified by Caullery, treated all history as the history of species, and entailed an equally unsustainable division between phenomena observable in the present and natural law operating in the past. German neo-Kantian sociologists had gotten closer, insisting on the importance of meaning and subjectivity, but they had ultimately fallen back on positivism, undermining any meaningful distinction between human and natural history. It was only by rethinking the very meaning of the human that history could become both intelligible and meaningful. Man was an animal whose essence was not animal, but historic. Like other animals, Aron suggested, humans were indeed biological, subject to evolution and natural law. Unlike other animals, man looked at himself, and when he did so, saw history.

Conclusion

The story of *Introduction à la philosophie de l'histoire* is worth telling in part because Aron was in a generation of philosophers that turned to the life sciences as a conceptual resource. Georges Canguilhem (1991, 2008, 2011), who spent the better part of his career thinking with and about biology, was a classmate and friend of Aron's at the

École normale supérieure. Maurice Merleau-Ponty, two years behind them, drew heavily on the empirical findings of psychology, biology, and the social sciences in his work on perception, embodiment, and history (1969, 2003, 2012). Merleau-Ponty in particular also drew on the work of another *normalien*, Raymond Ruyer, who in the forties developed a metaphysics out of his deep interest in embryology (1946, 1952; Gabel, 2016).

Yet few historians have attended to this turn towards biology in French thought. This is in part because of the somewhat arbitrary division of labour between intellectual historians and historians of science. For intellectual historians, science tends to be relegated to the realm of the cultural context of ideas, whereas historians of science are not usually interested in philosophers at all, suspicious as they are of what they have been taught is ‘traditional intellectual history’s neglect of practice, material culture, and complex, pluralistic contexts in favor of artificially tidied, abstract systems of thought’ (Tresch, 2014: 153). And yet as John Tresch has recently argued (*ibid.*), this disciplinary split, a product of the sociological turn taken by history of science in the seventies and eighties, might be overcome to the benefit of both sides. The preceding analysis of Aron’s encounter with neo-transformism is therefore also a case for a disciplinary reunion.⁹

The origin of Aron’s philosophy of history in an encounter with evolutionary theory suggests that disciplinary boundaries, even those between science and humanism, were more permeable in the mid-20th century than historians tend to acknowledge. In seeking to avoid the idealism of his French teachers and the positivism of German neo-Kantianism, Aron sought develop a philosophy of history that moved *through* the limits of biological accounts of the past. Caullery in particular had exposed these limits when the contradiction between his theory of transformation and his observations in the present led him to posit a temporal rupture in natural law. In this way, the crisis in French neo-transformism had disclosed to Aron the fault lines of historical thought. The epistemic aporias faced by scientists and philosophers alike were not temporary stumbling blocks on the way to a more perfect knowledge, but were rather signs of an underlying reality of human existence. Neither biology nor philosophy could capture the fullness of human temporality, and it was therefore at the generative intersection of science and humanism that history was to be found.

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Notes

1. On the sense of crisis in French academic philosophy in this period, see especially Hughes (1966), Kleinberg (2005), Roth (1988) and Sirinelli (1988).

2. The sociologist Paul Fauconnet famously accused Aron that day of being either ‘desperate or satanic’ (1938: 29). (*Revue de métaphysique et de morale*, 1938: 29)
3. Aron did not specify whose laboratory he visited, just that it was at the ENS.
4. This is why Georges Canguilhem argued that while Lamarck was at core a vitalist, neo-Lamarckians were mechanists through and through (Canguilhem, 2008: 103–4).
5. On Morgan’s conceptual distinction between heredity and development see Morgan (1910), Falk (1995) and Gilbert (1978).
6. Rabaud’s position has interesting parallels with what Garland Allen influentially described as the ‘revolt against morphology’ in the same period in American biology. However in Rabaud’s case, it was his commitment to a particular conception of the organism and not a move from natural history to experimentalism that led him to reject the significance of morphology. See Allen (1975, 1981) and critical response in Maienschein *et al.* (1981).
7. Any ‘escape’ from perspective was impossible in Aron’s view, but this did not foreclose the possibility of universality that began from perspectival truth. See Daston (1992).
8. This was of course a strange claim to make, given the ongoing heated debate about mechanism.
9. Some of this work might take place under the umbrella of the emerging field of ‘history of the humanities’.

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