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David Hull: a memoir

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David Lee Hull was born on June 15, 1935 and died on August 12, 2010. Compulsory service in the military was followed by 4 years at Illinois Wesleyan University, where he majored in biology. He then joined the new program in the history and philosophy of science at Indiana University, and was one of the earliest to get a doctorate from that institution. His first job was in the philosophy department at the University of Wisconsin at Milwaukee. He worked there from 1964 to 1984. He then moved to the philosophy department at Northwestern University and worked there from 1984 to 2000, when he retired. He garnered a number of honors, including a Guggenheim Fellowship, Membership in the American Academy of Arts and Sciences, and Fellowship in the American Association for the Advancement of Science. He had two honorary degrees, from Illinois Wesleyan and the University of Guelph in Canada. He was president of the Society of Systematic Zoology in 1984–1985 and of the Philosophy of Science Association in 1985–1986.

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David Hull was the author of many papers, mainly published between 1965 and 1985. He was the author of several books, *The Philosophy of Biology* (Prentice-Hall, 1974), *Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science* (The University of Chicago Press, 1988), and two collections of essays, *The Metaphysics of Evolution* (SUNY Press, 1989) and *Science and Selection: Essays on Biological Evolution and the Philosophy of Science* (Cambridge University Press, 2000). He was the editor of *Darwin and His Critics: The Reception of Darwin's Theory of Evolution by the Scientific Community* (Harvard University Press, 1973a), *Selection Theory and Social Construction: The Evolutionary Naturalistic Epistemology of Donald T. Campbell* (SUNY Press, 2001) with Cecilia Heyes, and (with me) *The Philosophy of Biology (Oxford Readings in Philosophy)* (Oxford University Press, 1998) and *The Cambridge Companion to the Philosophy of Biology* (Cambridge University Press, 2007). He was the long-time editor of the University of Chicago Press Science and its Conceptual Foundations Series, and from 1985 to 2000 an associate editor of *Biology and Philosophy*. In 1989 I edited *What the Philosophy of Biology is Not: Essays Dedicated to David Hull* (Kluwer).

I first met David Hull in 1968, at the first meeting of the Philosophy of Science Association in Pittsburgh. I had just returned from England, working on my PhD thesis, and was an untenured lecturer at the new university in Guelph, Ontario. I had decided to write on the philosophy of biology, mainly for strategic reasons, namely that there was not much literature on the subject and what there was was pretty awful. (I certainly did not choose the topic because of a prior interest or competence. Like most boys educated in England in the 1950s, I had never taken a biology course in my life.) However, from the beginning I realized that there was one star in the making in the field (if one can so label a rather arid area). Someone called David Hull was starting to produce papers at a high rate and it was clear that they were of much higher quality than almost everything else.

In particular there was a blockbuster, two-part paper in the *British Journal for the Philosophy of Science*, "The effects of essentialism on taxonomy: 2000 years of stasis" (Hull 1965). Arguing that we simply cannot apply notions worked out in the context of mathematics and physics to the life sciences, Hull lambasted one and all, old and young, ancient and modern, for their native assumptions about classification. In an age of evolution, he declaimed with vigor, we simply must think in terms of change, of development, and this applies to taxonomic notions as much as to anything else in science. It was heady stuff and I was almost pathetically eager to meet the owner of this vibrant voice. Parenthetically, I later learnt that there was somewhat of a history behind this paper. It had been prepared for a class that Karl Popper was giving, in his role as a visiting professor at Indiana. Although I am sure that the paper was quite sincere, it was prepared with Popper's prejudices in mind, namely that there is something different about evolutionary biology. However, whereas Popper thought the differences reflected badly on biology, Hull thought precisely the other! Apparently, Popper took the paper home, told the editor of the journal that it was to be published, and one day quite out of the blue David Hull received the proofs.

In Pittsburgh, David Hull did not disappoint. Wesley Salmon (who was the program chair) wanted to get this first meeting off with a bit of a start, showing the range of interests in the philosophy of science community as well as introducing new and young voices to the group. There was a plenary session on the philosophy of biology and Hull gave a major paper, "What the philosophy of biology is not" (Hull 1969). This was a call to arms, doing to the whole field what he had earlier done to students of classification. Broad-ranging, he showed how poor was so much of the material in the field, and again and again he stressed that if the job is to be done properly, then it must be based on a first-hand acquaintance of the science, and not simply (as was then so often the case). It was heady stuff, and I came away from Pittsburgh incredibly excited, feeling that I had indeed chosen a great area to work in and that the people with whom I would be laboring in the field were worthy of my respect and admiration. I should mention that also at Pittsburgh at that first meeting was Ken Schaffner, later to go off into the philosophy of medicine, and a young graduate student who seemed to be everywhere, to know everyone, and to have an opinion on every topic, one William Wimsatt. Who would not have been as thrilled as I?

There was no side to David Hull. In the academic world, you don't get much less important than being an untenured lecturer at a new university in Canada, especially one that was truly an extension of an already-existing agricultural college, with domestic science thrown into provide wives for the farmers. In the academic world you don't give a presentation much worse than I gave in Pittsburgh. My first ever acceptance of any kind—my wife and I had pizza and beer to celebrate—the organizer warned strictly that I was not go over the allotted 20 min. I practiced and practiced in front of a mirror, and when I stood up before my audience lost my nerve, read so rapidly that I took less than 12 min, and sat down shaking and sweating before a totally bemused group of people. My pathetic inadequacies made no difference. David Hull and I struck up a friendship at that meeting that lasted until the day of his death, a friendship that was never, ever marred by a cross word, and that ripened into a deep affection. For me, outside my family, my relationship with him was greater than I have ever had with another person. Five years older than I, better educated and more knowledgeable, a million times more diplomatic, he was my mentor and my friend. When people complain to me, as they often do, that they have been added to a project only because someone else has refused or failed to come through, I explain that my whole life has been one of picking the crumbs from David's table, of writing papers or giving talks when he was too busy or otherwise engaged. I have never felt belittled by this and am proud to think that I might substitute.

Rather than simply giving a chronological account of what he did, let me start with that Pittsburgh paper and draw out what I think are significant themes and threads in David Hull's work. As I have said, first and foremost the paper was a cry, a demand, that the philosophy of biology be based on a solid understanding of the pertinent science. Hull's work on taxonomy, which already included a paper in the leading science journal *Evolution*, was showing the way. He knew about classification, he talked to leading practitioners of the art (like the Harvard-based Ernst Mayr), and he was thinking hard about what it all meant. I think it fair to say

that in the early years (the 1960s) he was more concerned to spell out the issues and the problems, than to push a positive program. This came later in the 1970s, when the taxonomist Michael Ghiselin (1974) proposed his groundbreaking claim that biological species should not be regarded as sets of individuals but as integrated wholes—"species as individuals." Hull seized on this idea, endorsing and extending it. He published a couple of seminal papers on the subject and was a major factor in its great success in the (slowly growing) philosophy of biology community (Hull 1976, 1978).

I should say that this was one area where David and I were in complete and utter philosophical disagreement. We could not have been further apart. Much influenced by the nineteenth-century philosopher of science William Whewell, I had struck my colors on the mast of consilience (Ruse 1969). I argued that species were groups of members (individual organisms) and that the special nature of species (their reality or objectivity) came through the coincidence of groupings made according to different criteria of admission or rejection. Thus a species delimited by genetic criteria coincided with a species delimited by morphological criteria, and the consilience proved that there was more than mere subjective grouping. We argued this back and forth by letter and in conversation when we met, and once in a while in print. David gave me terrific advice when first he sent something off criticizing me. Far better to be criticized than ignored.

Only gradually did it come through to me that this disagreement about species was the tip of a much bigger iceberg of difference. I had—to be honest, to a great extent I still do—endorsed with enthusiasm the then-dominant philosophy of science of logical empiricism. Articulated by such thinkers as Ernst Nagel (1961) and Carl Hempel (1966), it sees science as a body of laws, bound together preferably in deductive relationships. Explanations involve showing how phenomena fall under laws ("covering law explanation"); theories are bodies of laws in deductive axiom systems ("hypothetico-deductive systems"); and older theories are rejected or shown to be the deductive consequences of newer theories ("theory reduction"). Above all, search for unseen, theoretical entities to do the heavy lifting when explanation is needed. Small is beautiful, and even smaller is even more beautiful. Although I was working from an already-committed position, I was bound to reject species as individuals. Reading George Williams's *Adaptation and Natural Selection* (1966) was my road-to-Damascus experience. If individual selection rules all, then there was no way that a species could be an individual. It was one against all, all of the time.

It would be wrong to say that David Hull was an anti-reductionist, emergentist holist, if by this you mean that he was in any way sympathetic to soft-side thinking as represented by the vitalists or fellow travelers. He was from the Mid-West and in respects a more solid, pragmatic thinker it would be hard to imagine. But there was something of the holist about him, always. He was ever deeply influenced by his teachers at Indiana, above all the charismatic Norwood Russell Hanson and (perhaps even more) the Australian Michael Scriven. They had been at the forefront of the attack on logical empiricism, stressing how divorced it is from real, messy science, and how ahistorical a philosophy it presents. They urged that one look at a piece of science in its own right, and—although they were working more in parallel with

Thomas Kuhn than derivatively—notions like incommensurable were never far from the surface.

In his little textbook on the philosophy of biology, this philosophy came out strongly. Hull (1974) lambasted the claims that the logical empiricist thoughts about reduction apply to the biological sciences. Explicating both Mendelian and molecular genetics, he argued forcefully that there is no easy deductive relationship between them, the older being a logical consequence of the newer. "I find the logical empiricist analysis of reduction inadequate at best, wrongheaded at worst" (12). Adding, "the conclusion seems inescapable that the logical empiricist analysis of reduction is not very instructive in the case of genetics. For my own part, I found that it hindered rather than facilitated understanding the relationship between Mendelian and molecular genetics" (44). Elsewhere he even went so far as to say that moving from Mendelian to molecular genetics and conversely involved "gestalt switches" (Hull 1973b, 626).

Although there were other reasons why the species-as-individuals thesis was found attractive, reasons I shall give shortly, I see the thesis very much at one with this overall view of science. And then around 1980, Hull started to think seriously about natural selection and its operation. Although he was never (as I saw it) a full-blooded enthusiast for group selection, he was one of the pioneers in stressing that we are faced with a hierarchical situation. We have selection at the level of the gene, but also let it never be forgotten that we have selection at the level of the organism. He thought long and hard about issues to do with biological individuality, and famously introduced the distinction between replicators (genes) and interactors (organisms) (Hull 1980). He preferred the term "interactor" over the alternative used by Richard Dawkins, "vehicle", because this brought out the active nature of organisms rather than simply as passive containers for the real forces of evolution, the genes. Others, like Elliott Sober (1984), picked up on this point. It is worth noting how committed is Sober to some kind of holistic vision of science.

That crusading early paper, "What the philosophy of biology is not", did something less too, perhaps more by default. It led us firmly away from, or perhaps more accurately did not steer us firmly towards, other areas of biology than evolutionary theory. It is true that molecular biology was to get some attention, but generally this came only in the context of evolutionary thinking. Areas like ecology got little or no treatment and the same then was true of areas like embryology (even though now they are firmly integrated into the evolutionary story). In part, this emphasis on evolution was because frankly it is the area with the most obvious philosophical issues. In part, this was because the most philosophy-friendly biologists were the evolutionists. Ernst Mayr was very welcoming—although, fitting in nicely with Hull's inclinations, he had his own agenda, namely using philosophy to reinforce the autonomy of traditional biology in the face of the molecular tsunami.

But also I think in Hull's case (as was certainly true of me), very significant was the turn to history which was an important part of the post-Kuhnian scene in the 1960s. (In Hull's case, reinforced by being in a department of *history* and philosophy of science and being taught by someone as sensitive to history as Hanson.) Charles Darwin—especially for those of us in the Anglophone world—is the giant in the history of biology. It was natural therefore to think that his

achievements dominate biology today and are the major source of philosophical interest and inquiry. Be this as it may, it is certain that in Hull's thinking Darwin and today were linked in one great whole and that work in one area was complemented by work on the other area. Hull took this belief so seriously that his first book (*Darwin and His Critics*) was a collection of responses to Darwin's *Origin*, by scientists, and he himself wrote a long introduction to the volume, one that stressed how important was philosophy in trying to understand both what Darwin had done and what his critics were led to say. To be fair, in respects Hull and others like myself were here also following in the footsteps of Michael Ghiselin, who had mined the field well in his *The Triumph of the Darwinian Method* (1969), which looked at the whole of Darwin's work in the light of philosophy.

Let me make one final point about that paper given at Pittsburgh. Although it was indeed very critical of the existing work in the philosophy of biology, it was interesting in the way that bridges were never completely burnt. Even those most taken to task were not subjected to a Richard Dawkins-like blast, from which relationships could never be repaired. The case of Marjorie Grene (1959) was instructive. She had published a paper in the late 1950s, "Two evolutionary theories", in which she compared the paleontological thinking of the Darwinian George Gaylord Simpson with the German saltationist (evolution by jumps) Otto Schindewolf. Favoring the latter, naturally she earned the scorn of many active evolutionists. David however, although critical was sympathetic to her thinking and even sent his paper to her before publication for her comments. I used to think that this was in part because, although he was a committed Darwinian, nevertheless he rather liked the holism that Schindewolf represented. I think now it was deeper and a reflection of the way in which David Hull networked with his community, gaining respect and friendship with everyone including those most likely to disagree. The difference with me was striking. I wrote a fiery paper criticizing Grene for many faults, not all of which are now apparent to me. As it happens the paper, thanks to a very severe referee's report, was never published. The referee never forgave me, although later in life I reread her paper and now think that although wrong about Darwinism, in respects it is path breaking in the way she shows how visual images can determine the route of scientific thinking.

Networking was very important to David Hull and it on this that I want to focus now. The 1970s saw a revolution in taxonomic thought and practice. It moved from a rather ecumenical evolutionary approach, as represented in somewhat different ways by Mayr and Simpson, to an approach inspired by the German taxonomic theorist Willi Hennig (1966). Although Hull did engage in the theoretical issues, he was never as connected as were some later philosophers, notably Elliott Sober (1988). But, knowing all of the main actors, both for and against Hennig's approach (which became known as "cladism"), he grew increasingly interested in the sociological factors at work in a scientific revolution. At the same time, he became increasingly convinced that an evolutionary approach to change was the key to true understanding. Here he paralleled a number of other people, perhaps significantly also located in the Chicago area. I refer specifically to Stephen Toulmin (1967, 1972), Donald Campbell (1974), Robert J. Richards (1987), and Bill Wimsatt (2007).

Hull embarked on a full-scale inquiry, going around and interviewing people at length and also diving into the archives, especially those of the journal *Systematic Zoology* (published by the organization of which he was to become president). Eventually, this emerged as a major book, *Science as a Process*, that looked at the taxonomic revolution and that gave an evolutionary interpretation to what happened. In certain respects the analysis was not that original, because Hull followed others (Toulmin especially) in seeing a fairly direct analogy between organic change and scientific change. Organisms/ideas are produced, they struggle for ascendance, winners (the fittest) emerge, and so change occurs and the process of science goes on. I am not sure, but given that Popper also held similar ideas and given that Hull had been a student, if only in one course, of Popper, there may have been an influence here.

In other respects the analysis was original, and in my opinion this is the most important piece of work that David Hull did. Seizing on the notion of networking, something that he himself had used to the full to gain the confidence of everyone in the taxonomic wars (and at times they got quite vicious), Hull argued that networking is the key to scientific success. Junior scientists need senior scientists to help them gain entry to the world of science. Senior scientists need junior scientists to carry on the work and to respect their achievements. Carefully marshalling his arguments, Hull showed how this works in science, how this had worked in the taxonomic disputes, and why so many other things fall into place. In a way, this starts to make David Hull sound a bit like a social constructivist—the truth doesn't really matter but who you know does matter—but I think this would be to trivialize what he was trying to say. The truth does matter. Ultimately you are not going to get respect without it. But it is only a necessary condition and by no means always a sufficient condition. Unless a philosopher pays full attention to the sociological side to science, he or she will only get part of the picture, if that.

It is clear that in arguing as he did, Hull was being influenced by ideas in the rapidly growing field of evolutionary social studies, sociobiology. Robert Trivers's (1971) thinking about reciprocal altruism was a major inspiration. Given this, it is worth noting that more generally Hull stood on the sidelines when sociobiology, particularly the human version, became so heated a topic in the late 1970s. I often used to wonder if this lack of full involvement was because he did not want to alienate half of his friends, as he would have done had he taken a firm stand. (Which half would of course depend on which side of the dispute he had stood.) There may have been truth in this, but I think really that at the time he was so fully engaged in his own major project (on taxonomy) that he really had little energy or interest to give to something that was clearly generating a lot more heat than light. It just wasn't his fight.

Nor incidentally was the fight between science and religion. I got involved precisely because I was contacted by the Creationists after David had turned down their request that he participate in a debate between evolutionists and Creationists. (In that particular case, he was probably right. I ended up before an audience of 3,000, and at the end all of 10 voted for the evolutionists.) Here I am quite sure that David's lack of engagement stemmed simply from a lack of interest. He had been brought up as a Roman Catholic but become a non-believer early in his teens. From

then on, his attitude was not so much one of hostility to religion as total indifference. He could not see why anyone would be a believer, or why anyone like me would want to engage with believers. It would be a bit like having an obsession with undertakers, although to their credit undertakers do have an important social function.

Let me conclude by turning briefly to the more personal. David was gay. When I first knew him, this was not something one could announce publicly but I think I sensed it from the first and certainly early in the 1970s I knew and he knew that I knew. He had a lover, Richard Wellman ("Dick"), a teacher in the Chicago school system, and this was a major reason why David always worked in or within reach of Chicago. Like many gay men, David formed intense relationships with older women, in his case with his aunt Ginger and with Bella, a woman who had been David's landlady in Milwaukee and who later came to live in his house in Chicago. (I never knew Dick but knew both Ginger and Bella, and became very close to the latter.) I know that being gay was very important to David, and even to this day I am a little surprised that he let me (a very straight man) so closely into his confidence and affections. I think it was because we came together at crucial points in our professional and intellectual lives—rightly or wrongly we really did think ourselves pioneers in an unknown land. Also we realized that in the other there were things lacking in ourselves. In my case, any sense of balance or political savvy. I suspect in his case he admired my willingness to go out and fight in public for the things I think right. He also loved my family, especially my daughter Emily.

One thing of which I am quite sure is that his personal and professional life did sometimes overlap. As I have explained, philosophically he was prepared for the species-as-individuals thesis. But it also tied in importantly with his sexuality. He resented deeply the biological species concept which puts such a great emphasis on reproductive intentions and abilities for true species membership. He had no intention of reproducing yet felt fully human. I don't think he would have accepted the species-as-individuals thesis had he not thought it right, but he did find it psychically satisfying also.

The 1980s were very hard on David Hull. Dick died of Aids, as did a huge number of David's friends and acquaintances. (He told me that as soon as he first heard rumors of Aids, he ceased all sexual activity and indeed remained infection free all of his life.) David nursed and cherished many through their final days. Not entirely facetiously, I called him "the Mother Teresa of the gay community." He put his life back together again, making a home for another old female friend Jerry. (I used to joke that after all of these years, David Hull was revealed as a closet heterosexual.) He also became the mentor of many young men, gay and straight. I am very glad to say that in the years as he started to go downhill, these young friends returned the love and affection a thousand fold and cared for him to the end.

I felt that David never really recovered from the terrible days at the height of the Aids crisis. Dick was dying as David was trying to finish *Science as a Process*, and he was always the first to admit that the book could have used another draft. Later, he never really recovered his full energy or zest. I urged him to rework *Science as a Process*, focusing on the parts that were truly original and that would bring him the attention that he deserved. But it was never to be. Others will have to go back and find what he mined and bring it to its full gloss.

What I have not yet said and what I want to say now in ending so this is the thing you remember is what a good friend David Hull was to all. He gave and gave and gave. There was no paper too trivial or too boring for him to comment on. There was no insecure young professor too unimportant for him to encourage and advise. The time he put into the Conceptual Foundations series was truly stupendous. The efforts he made for his various associations were humbling. I don't think he had a huge number of graduate students, but those that he did have were looked after well beyond the expected norms. There are some people who are simply better than others. All of 5′4″, David Hull was the biggest man I ever knew.

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