DONALD DAVIDSON AND THE ANALYTIC REVOLUTION IN AMERICAN PHILOSOPHY, 1940–1970*

JOEL ISAAC

Christ's College, Cambridge

ABSTRACT. *Histories of analytic philosophy in the United States have typically focused on the reception of logical positivism, and especially on responses to the work of the Vienna Circle. Such accounts often call attention to the purportedly positivist-inspired marginalization of normative concerns in American philosophy: according to this story, the overweening positivist concern for logic and physics as paradigms of knowledge displaced questions of value and social relations. This article argues that the reception framework encourages us to mistake the real sources of the analytic revolution in post-war philosophy. These are to be found in debates about intentional action and practical reasoning – debates in which 'normative' questions of value and social action were in fact central. Discussion of these topics took place within a transatlantic community of Wittgensteinians, ordinary languages philosophers, logical empiricists, and decision theorists. These different strands of 'analytical' thinking were bound together into a new philosophical mainstream not by a positivist alliance with logic and physics, but by the rapid development of the mathematical and behavioural sciences during the Second World War and its immediate aftermath. An illustrative application of this new framework for interpreting the analytic revolution is found in the early career and writings of Donald Davidson.*

When historians examine the rise of analytic philosophy in the United States, they typically tell a story about the reception and transformation of logical positivism on American soil. According to some, the Vienna Circle, transplanted from the cafés of Vienna and Prague to the seminar rooms of Harvard and Berkeley, cleared the field for technical work in epistemology, semantics,

757

and mathematical logic. In doing so, it is argued, the Circle and its American acolytes decimated indigenous populations of pragmatists, critical realists, and naturalists.[1] The philosopher Stanley Cavell, himself present at the creation of the analytical mainstream in American philosophy, has captured the essence of this view in a story about an elderly colleague in the Berkeley philosophy department in the late 1950s. The professor confessed to Cavell that an encounter with the convener of the Vienna Circle, Moritz Schlick, left his own philosophical views so shaken that he considered abandoning his career and going to medical school. Only his advanced age prevented him from doing so, and he decided instead to 'brave[ ] it out in philosophy' until retirement.[2]

The same story of reception and transformation has been told in many other ways. Approaching the history of the analytic revolution from the perspective of the émigré alumni of the Vienna Circle, the philosopher of science Michael Friedman and his students have treated the intellectual migration as a turning point in the history of logical empiricism. Whereas the Vienna Circle and its affiliates were interested in unifying the mathematical and natural sciences through a metalinguistic account of the a priori, after the emigration this constructive programme fell before W. V. Quine's attack on the analytic–synthetic distinction and his advocacy of naturalism in epistemology.[3] Other scholars have called attention to the stripping away of the cultural aspirations of the exiled Vienna Circle in the context of the Cold War, a conjuncture that some suggest transformed scientific philosophy into the intramural, purely professional enterprise we now know as analytic philosophy.[4] Another significant perspective on the sources of the post-war analytic turn in American philosophy focuses on the transformation of the concepts and ethos of scientific philosophy by the Second World War, which subjected logical empiricist ideas about the unity of science to the new 'cyborg' disciplines of Operations Research, communications engineering, and computing.[5]

[1] Bruce Kuklick, *A history of philosophy in America, 1720–2000* (Oxford, 2001), pp. 232–7, 243–58; James T. Kloppenberg, 'Pragmatism: an old name for some new ways of thinking?', *Journal of American History*, 83 (1996), pp. 100–38.
[2] Stanley Cavell, *Little did I know* (Stanford, CA, 2010), pp. 352–3.
[3] Michael Friedman, *Reconsidering logical positivism* (Cambridge, 1999); Alan Richardson and Thomas Uebel, eds., *The Cambridge companion to logical empiricism* (Cambridge, 2007); Gary L. Hardcastle and Alan W. Richardson, eds., *Logical empiricism in North America* (Minneapolis, MN, 2003).
[4] George R. Reisch, *How the Cold War transformed philosophy of science: to the icy slopes of logic* (Cambridge, 2005); Don Howard, 'Two left turns make a right: on the curious political career of North American philosophy of science at midcentury', in Hardcastle and Richardson, eds., *Logical empiricism in North America*, pp. 25–93.
[5] Peter Galison, 'The Americanization of unity', *Daedalus*, 127 (1998), pp. 45–71; Philip Mirowski, 'Cyborg agonistes: economics meets operations research in mid-century', *Social Studies of Science*, 29 (1999), pp. 685–718; idem, 'The scientific dimensions of social knowledge and their distant echoes in 20th-century American philosophy of science', *Studies in History and Philosophy of Science: Part A*, 35 (2004), pp. 283–326.

My purpose in this article is to complicate this story, and to suggest a more nuanced explanation of the emergence of an analytic mainstream in American philosophy. The linear narrative of ecological succession sketched above – of the migration, reception, and acculturation of scientific philosophy – is not straightforwardly wrong. Nevertheless, the analytic revolution in American philosophy is liable to be misunderstood when the topic is approached solely in the terms of a reception history in which one virulent strain of philosophy is at once 'Americanized' and then installed as the professional consensus. My alternative interpretation is grounded in two claims: first, that the rise of analytic philosophy in the American context must be understood as part of a broader set of mid-twentieth-century innovations in the mathematical and behavioural sciences; and, second, that these developments were rooted in transformative changes in the pursuit of science and scholarship in the United States. To defend these claims is to suggest that reception histories focusing on the transformation of logical empiricism on American soil have their limits. Not only is the account we ought to provide of the analytic revolution not exclusively one of cultural migration and displacement; it is also, as I shall highlight as we proceed, one that includes a record of engagement with normative issues of value, rationality, and social action. The key lesson here, and what I hope will be a spur to further inquiry among intellectual historians and historically inclined philosophers alike, is that we know less about the sources of contemporary analytic philosophy than we think we do.

Because the historical terrain that this approach to the topic must cover is so vast, I will introduce broader themes through an assessment of one exemplary case: the early career of the philosopher Donald Davidson. Davidson is today recognized as one of the most important systematic philosophers of the twentieth century. His mature work on interpretation, communication, and agency is the subject of several lengthy expository tomes.[6] I shall focus my remarks on Davidson's path to his career-making paper, 'Actions reasons, and causes' (1963). I show how Davidson combined an appreciation of formal methods arising from the transatlantic tradition of scientific philosophy with an evolving expertise in theories of decision, behaviour, and value. In post-war America, the early and middle-period figureheads of analytic philosophy – Russell, Wittgenstein, Frank Ramsey, Alfred Tarski, and Rudolf

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[6] For two contrasting takes on Davidson's philosophy, see, on the one hand, Ernest LePore and Kirk Ludwig, *Donald Davidson: meaning, truth, language, and reality* (Oxford, 2005), and idem, *Donald Davidson's truth-theoretic semantics* (Oxford, 2007); and, on the other, Jeff Malpas, ed., *Dialogues with Davidson: acting, interpreting, understanding* (Cambridge, MA, 2011). An accessible introduction to Davidson is provided in Bjørn T. Ramberg, *Donald Davidson's philosophy of language: an introduction* (Oxford, 1989). The key collections of essays on Davidson are Ernest LePore and Brian P McLaughlin, eds., *Actions and events: perspectives on the philosophy of Donald Davidson* (Oxford, 1985); and Ernest LePore, ed., *Truth and interpretation: perspectives on the philosophy of Donald Davidson* (Oxford, 1986).

Carnap – mattered to would-be analytic philosophers in so far as their ideas and techniques showed up within an expanded field of conceptual possibilities in which the tools of philosophical analysis were not sharply distinguished from those of the mathematical and behavioural sciences.

I

Published in the *Journal of Philosophy* in 1963, Donald Davidson's essay 'Actions, reasons, and causes' is an acknowledged classic of post-war analytic philosophy. Davidson considered his paper 'a reaction against a widely accepted doctrine that the explanation of an intentional action in terms of its motives or reasons could not relate reasons and actions as cause and effect'.[7] When Davidson composed his intervention, it was widely held among philosophers of action that a reason explained an action by redescribing it in such a way as to give the agent's reason for performing the action. According to these philosophers, however, redescription had nothing to do with the explanatory scheme of cause and effect because, as Davidson glossed this position, 'causal relations are essentially nomological and based on induction while our knowledge that an agent has acted on reasons is not usually dependent on induction or knowledge of serious laws'.[8] In *Intention* (1957), the book credited with reviving the philosophy of action as a serious field, Elizabeth Anscombe captured the essence of this view with her emphasis upon the way in which an agent's intentions were 'known' to that agent 'without observation'.[9]

   The anticausalist theory of action at which Davidson took aim was associated with three overlapping groups of philosophers.[10] The first encompassed the authors of the so-called 'red books' published in Routledge and Kegan Paul's Studies in Philosophical Psychology. Especially prominent entries in this series were authored by Abraham Melden, Anthony Kenny, and Peter Winch. Although she did not publish in the series, Anscombe was closely associated with the philosophical psychologists. Davidson called these thinkers 'neo-Wittgensteinians' because they drew on the later Wittgenstein's ideas about conceptual 'logic' to attack the pretensions of scientific accounts of thought and agency.[11] The second group was composed of Oxonian philosophers of ordinary language, notably Gilbert Ryle, Stuart Hampshire, and

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[7] Davidson, 'Introduction', *Essays on actions and events* (2nd edn, Oxford, 2001), p. xvi.

[8] Ibid.

[9] Elizabeth Anscombe, *Intention* (Oxford, 1957). See also Richard Moran, 'Anscombe on "practical knowledge"', *Royal Institute of Philosophy Supplement*, 55 (2004), pp. 43–68.

[10] Davidson cites members of each group at the beginning of his essay. See 'Actions, reasons, and causes', *Journal of Philosophy*, 60 (1963), p. 685 n. 1.

[11] Davidson identified as the target of his critique of anticausal theories of action 'most of the books in the series edited by R. F. Holland, *Studies in Philosophical Psychology*' and singled out Anthony Kenny, *Action, emotion, and will* (London, 1963), A. I. Melden, *Free action* (London, 1961), Peter Winch, *The idea of a social science* (London, 1958), and R. S. Peters, *The concept of motivation* (London, 1958). He also specifically addressed Anscombe, *Intention*.

H. L. A. Hart.[12] Finally, defences of the autonomy of the explanation of action were also mounted by philosophers of history like William Dray, who was in turn inspired by the writings of the British Idealist R. G. Collingwood.[13] These figures were convinced that the explanation of action differed in kind from the explanation of natural phenomena. Understanding human action, they argued, turned on establishing a *logical* or *conceptual* link between the explanandum – the action – and the explanans – the reasons (beliefs, desires, etc.) that explained the action in question. The challenge for the interpreter of an action was to place it in a pattern of beliefs and desires that made it intelligible – a pattern that rationalized the action. This was above all an exercise in rede-scribing actions, not in experimental inquiry. In the natural sciences, by contrast, explanation involved the identification of the cause of an event that stood in need of explanation, and this in turn required that the investigator discover an empirical or observational connection between explanandum and explanans. The anticausalists therefore agreed with Hume's claim that knowl-edge of cause and effect 'arises entirely from experience, when we find [from observation] that any particular objects are constantly conjoined with each other'.[14] But they insisted that, because the connection between reasons and actions was logical or conceptual rather than empirical, the explanation of action was not a form of causal explanation.

The anticausalist thesis that the connection between explanandum and explanans in the explanation of action must be ascribed to the logic of the concepts employed in such an enterprise rested on the purported demon-stration that the explanation of action violated the standards of causal ex-planation. In so far as reasons explained actions, the inapplicability of the causal scheme of explanation to reason-explanations indicated that reasons did not 'explain' actions through the empirical discovery of a causal relation between events. In *Causation in the law* (1959), H. L. A. Hart and A. M. Honoré noted that singular causal statements necessarily implied a covering law and then observed that the attempt to derive general laws from the explanation of intentional actions quickly lapsed into absurdity.[15] What possible sense could it make to say that Smith's stepping on Jones's toes because he wanted to take revenge on Jones for his cruel quip about Smith (this being a condensed explanation of Smith's action) was covered by a general law about the relation between cruel quips and foot-stampings? Another typical argument offered by anticausalists ran as follows: causal relations by definition related two inde-pendent events – one event identified as the cause and the other the effect – but

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[12] In 'Actions, reasons, and causes', Davidson drew on Stuart Hampshire, *Thought and action* (London, 1959), H. L. A. Hart and A. M. Honoré, *Causation in the law* (Oxford, 1959), and Gilbert Ryle, *The concept of mind* (New York, NY, 1949).

[13] Davidson cited William H. Dray, *Laws and explanation in history* (Oxford, 1957).

[14] David Hume, *Enquiries concerning human understanding and concerning the principles of morals*, ed. P. H. Nidditch (1777; Oxford, 1975), pp. 27–8.

[15] Hart and Honoré, *Causation*, pp. 52–3.

reason-explanations treated only a single event under different descriptions. For example, when one explained an agent's raising of their arm by noting that they were signalling a turn (or, strictly speaking, that they wanted or intended, by raising their arm, to signal a turn) one was describing one and the same event. Moreover, as both Abraham Melden and Gilbert Ryle had insisted, the purported 'causes' of actions – beliefs and intentions – were not events *sensu stricto* but mental states or physiological dispositions – e.g., the desire to signal a turn and/or the belief that raising one's arm would satisfy this desire.[16] Finally, Oxonian and neo-Wittgensteinian philosophers alike noted that the explanation of action was often identical to the justification of the action in question. An interpreter might explain an agent's dropping of the tray of drinks by pointing out that the agent had tripped on a loose floorboard, or that they had intended to prevent an unwitting customer from consuming a cup of tea they knew to be poisoned. Such explanations were also justifications or excuses for the action under interpretation, and it seemed clear that such normative elements were absent from the causal explanations typical in the natural sciences.[17]

The most general criticisms the anticausalists levied against causal explanations of human actions hinged on a claim about the special status of what it was to have 'knowledge' of the intentions that were said to be the 'cause' of actions. When explaining the actions of a third party, one did not usually first observe an intention or disposition and then witness the action one wished to explain. First came the action, and then the post-hoc attempt to find a way of describing it that captured the agent's intention in acting. In the case of self-knowledge, of knowing one's own intentions, it was, as one commentator put it, 'absurd' to say that one first acted and then observed one's actions to look for the intention with which the act was performed; rather, one had an intention, and one acted.[18] In these cases, it seemed natural to speak of 'knowledge without observation'. The anticausalists concluded that explanations in the natural and human sciences differed in regard to *method*. Natural sciences relied on the empirical method and focused on establishing causal relations between events; the human sciences centred on a logical or conceptual form of inquiry rooted in the redescription of phenomena in such as way as to render them intelligible.[19]

In 'Actions, reasons, and causes', Davidson's principal criticism of the anticausalists was that the case against causation in explanations that invoked

[16] See Melden, *Free action*, pp. 20–1; Ryle, *Concept of mind*, pp. 80–147.

[17] Anscombe, *Intention*, pp. 69–77; J. L. Austin, 'A plea for excuses', *Proceedings of the Aristotelian Society*, 57 (1956), pp. 1–30.

[18] Keith Donnellan, 'Knowing what I am doing', *Journal of Philosophy*, 60 (1963), pp. 401–9; Stuart Hampshire and H. L. A. Hart, 'Decision, intention and certainty', *Mind*, 67 (1958), pp. 1–12.

[19] Giuseppina D'Oro, 'Davidson and the autonomy of the human sciences', in Malpas, ed., *Dialogues with Davidson*, pp. 285–6.

reasons had not been made.[20] There were two parts to this argument. The first sketched an account of what was involved in explaining an action by citing an agent's reason for performing it, while the second defended the thesis that, when properly understood, the construction of an agent's reason for performing an action necessarily involved treating the reason as the cause of that action. Although Davidson insisted that the second claim followed directly from his account of the proper form of a reason-explanation, his remarks in support of the treatment of reasons as causes rested on a survey of the limits of contemporary action theory. This theory, according to Davidson, went opaque at the point where it departed from the explanatory scheme of cause and effect. In response to the supposedly telling anticausal observation that 'reasons alone justify actions in the course of explaining them', Davidson pointed out that the appeal to justification did not tie reasons to actions in a way that would be explanatory. Specifically, it did not take account of the possibility that

a person can have a reason for an action, and perform the action, and yet this reason not be the reason why he did it. Central to the relation between a reason and an action it explains is the idea that the agent performed the action *because* he had the reason.[21]

What this meant, Davidson said, was that neo-Wittgensteinian remarks about redescribing actions so that they were situated within broader, explanation-yielding patterns of rules, norms, conventions, forms of life, and so on – these remarks amounted to so much hand-waving until such patterns could bridge the gap between showing that an agent had a reason to act, and did act, and that this reason was in fact the reason the action was performed. Davidson was proposing that explanations of action, if they were to be authentic explanations, had to meet a standard of descriptive adequacy – they had to describe the reason that had in fact moved the agent to act, and thereby had to meet empirical as well as logical standards. But then what else could one say other than that the agent performed the action *because* they had the reason cited as its explanation? Davidson devoted much of the second part of his essay to explicating and defending the concept of causation contained within that 'because'. Part of his task was to show that the singular causal statements on which action explanations depended ('Smith stepped on Jones's toes because he wanted to take revenge on Jones') did not need to appeal to explicit general laws, as Hempel and other reductionists had suggested.

The significance of these claims becomes clearer when we consider the first, programmatic part of Davidson's argument. For Davidson, giving a reason for an agent's action involved the construction of what he called a 'primary reason'. A primary reason attributed to an agent two dispositions or states: (1) a 'pro attitude' – paradigmatically a desire but more generally any evaluative attitude in reference to which an action would appeal to the agent; and (2) a belief or

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[20] Davidson, 'Actions, reasons, and causes', p. 692.   [21] Ibid., p. 691.

some form of recognition that the action in question possessed the appealing property. An action, like other events or objects, was usually given alternative descriptions. Davidson acknowledged that the desires and beliefs identified in the citation of a primary reason could explain an action *only* under the description in terms of which an agent would have regarded the appealing action when they formed the relevant desires and beliefs. In 'Actions, reasons, and causes', this reflection served to underscore the importance of explaining actions in the terms that the agent saw it: by flipping a switch, I may inadvertently alert a prowler to the fact that I am home, but I performed the action because I wanted to turn on the light and believed that flipping the switch would satisfy that desire.[22]

   Davidson's essay was timely. Although the topic is neglected in the historical literature on analytic philosophy, debates about the ontological status and explanation of action – with the explanatory successes of the natural sciences taken as given – brought together several strands of 'analytic' philosophy and bound them together into a recognizable form of Anglo-American philosophy. When we bear these developments in mind, the early reception of logical empiricism during the 1930s and 1940s, in which philosophers such as W. V. Quine, Nelson Goodman, and Charles W. Morris focused on issues in philosophical logic and the unity of science, appears at most the prelude to the establishment of analytic philosophy. The late 1950s and early 1960s saw issues over action, intention, and causation joined by a much wider constituency: theorists of scientific explanation like Carl Gustav Hempel and Ernest Nagel, neo-Wittgensteinians like Anscombe and Melden, ordinary language philosophers such as Austin, Ryle, and Hampshire, and a new wave of American analysts like Arthur Danto and Roderick Chisholm.[23] This was the context in which Davidson intervened. Ostensibly a defence of 'the ancient – and common-sense – position that rationalization is a species of causal explanation', Davidson's paper displayed an extensive roster of analytic-philosophical topics: scientific explanation, logical form, causation, and the theory of decision.[24] After an initially muted reception, 'Actions, reasons, and causes' was quickly recognized on both sides of the Atlantic as a tour de force.[25]

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[22] Ibid., pp. 685–90.
[23] In addition to the works by Anscombe, Melden, Hampshire and others cited above, a sign of the renewed interest in the philosophy of action can be found in the *Journal of Philosophy*'s publication of a special issue on the topic in 1963 (before the appearance of 'Actions, reasons, and causes'). See essays by G. E. M. Anscombe, Richard Brandt and Jaegwon Kim, Arthur Danto, Keith S. Donnellan, Stuart Hampshire, Brian O'Shaughnessy, and Sidney Morgenbesser in *Journal of Philosophy*, 60, nos. 14 and 15 (1963). See also Roderick M. Chisholm and Richard Taylor, 'Making things to have happened', *Analysis*, 20 (1960), pp. 73–8; Roderick M. Chisholm, 'The descriptive element in the concept of action', *Journal of Philosophy*, 61 (1964), pp. 613–25.   [24] Davidson, 'Actions', p. 685.
[25] Ernest Lepore, 'An interview with Donald Davidson', in Davidson, *Problems of rationality* (Oxford, 2005), pp. 252, 260.

But who was Donald Davidson? He is today recognized as one of the major philosophers of the last third of the twentieth century, but in 1963 this question would not have been rhetorical. When 'Actions, reasons, and causes' appeared in print, Davidson was in his mid-forties and a largely unpublished associate professor at Stanford University. He described his presentation of the paper at a meeting of the American Philosophical Association as 'in many ways my first real public performance'.[26] We must next ask what were the intellectual commitments and interests that led Davidson to write his paper, and to present his argument as he did.

II

In a relatively straightforward sense, Davidson can be thought of as a neo-Aristotelian – at least in regard to the theory of action. Davidson placed considerable emphasis on the ancient sources of his argument. In 'Actions, reasons, and causes', he engaged two issues raised by Aristotle: first, the nature of practical reasoning, which in Aristotle was treated in terms of the practical syllogism; and, second, the motivational sources of voluntary action. Aristotle's texts became especially important to Davidson when he turned in later essays to consider the problems posed for his causal theory of action by the phenomenon of *akrasia*, when agents acted 'incontinently' – that is, intentionally but against their own best judgments as to the right course of action.[27] It was in Aristotle that Davidson found the 'ancient view' of the causal relation between reasons and actions that he sought to defend.[28] Especially appealing to Davidson was the assessment provided in *De Anima* of the sources of voluntary actions in the conjunction of appetite and thought. In Davidson's Aristotle, it was always an appetite or aversion that initiated the causal chain, with the faculty of reason cast in the role of calculating the means to the desired end.[29] Aristotle's description of the nature of practical reasoning rendered the causal connections between desire, belief, and action particularly strong: it proposed that the conclusion of a piece of practical reasoning, modelled in part on the logical syllogism, was not a decision or a belief but an *action*. Given an appetite or evaluative attitude toward a certain end, and the rational discovery of an

[26] Donald Davidson, 'Intellectual autobiography', in Lewis Edwin Hahn, ed., *The philosophy of Donald Davidson* (La Salle, IL, 1999), p. 37.
[27] Davidson, 'How is weakness of the will possible?' (1969), in *Essays on action and events*, pp. 21–42.
[28] The Aristotelian roots of Davidson's theory of action are often neglected in the contemporary commentary on the 'standard' Davidsonian account of action. His position is often described as principally Humean in inspiration. See, e.g., Michael Smith, 'The structure of orthonomy', *Royal Institute of Philosophy Supplement*, 55 (2004), pp. 165–6; idem, 'The Humean theory of motivation', *Mind*, 96 (1987), pp. 36–61; Kathrin Glüer, *Donald Davidson: a short introduction* (New York, NY, 2011).
[29] See Davidson, 'Aristotle's action' (2001), in Davidson, *Truth, language, and history* (Oxford, 2005), pp. 278–80.

available means to that end, an agent, concluding the practical syllogism, would 'straightaway' act.[30] These classical ideas were revived by the Wittgensteinians as debates about the explanation of action took shape during the 1960s.[31]

Crucially, however, in 'Actions, reasons, and causes' Davidson was firm about the need to 'redeploy' Aristotle's claims. In subsequent essays, he made explicit his view of the limitations of Aristotle's account of voluntary action when it came to handling conflicting values, decisions among alternatives, and the weakness of the will.[32] In the 1963 paper, these difficulties arose most starkly in connection with the question of how useful Aristotelian accounts of action could be in predicting future actions. A significant shortcoming of the Aristotelian practical syllogism, Davidson observed, was that 'it exhaust[ed] its role in displaying an action as falling under one reason'. Yet, to Davidson, it seemed obvious that what emerged 'in the *ex post facto* atmosphere of explanation and justification, as *the* reason frequently was, to the agent at the time of the action, one consideration among many, *a* reason'. Consequently, '[a]ny serious theory for predicting action on the basis of reasons must find a way of evaluating the relative force of various desires and beliefs in the matrix of decision'. This was why the practical syllogism could not be 'subtilized into a reconstruction of practical reasoning, which involves the weighing of competing reasons'. It offered, concluded Davidson, 'a model neither for a predictive science of action nor for a normative account of evaluative reasoning'.[33]

Here, Davidson indicated the kind of redeployment of Aristotle he had in mind. Although the commitment goes largely unstated in 'Actions, reasons, and, causes', Davidson's criteria for a powerful predictive science of action and a normative account of rationality were drawn from his work in the field of decision theory. His discussion of primary reasons also mapped onto his research in this area. In order fully to understand the kind of intervention Davidson was making in his famous 1963 essay, it is necessary to uncover the sources of these commitments.

Davidson did his graduate training at Harvard University, but not purely in philosophy. Between 1939 and 1941, and then again for a brief spell in 1945, Davidson studied in the combined field of classics and philosophy, albeit with growing ambivalence. Upon graduation from Harvard College in 1939, Davidson had been offered a studentship in classical philosophy on the

[30] For Aristotle's account, see Martha Nussbaum, *Aristotle's De Motu Animalium*, §7 (Princeton, NJ, 1978); Aristotle, *De Anima*, trans. Hugh Lawson-Tancred (London, 1986), pp. 210–16.
[31] Anscombe, *Intention*, pp. 57–66; idem, 'Thought and action in Aristotle', in Renford Bamborough, ed., *New essays on Plato and Aristotle* (London, 1965), pp. 143–58; Alexander Broadie, 'The practical syllogism', *Analysis*, 29 (1968), pp. 26–8; A Kenny, 'The practical syllogism and incontinence', *Phronesis*, 11 (1966), pp. 163–84; Mary Mothersill, 'Anscombe's account of the practical syllogism', *Philosophical Review*, 71 (1962), pp. 448–61.
[32] Davidson, 'How is weakness of the will possible?' and 'Intending' (1978), in *Essays on actions and events*. See also Davidson, 'Aristotle's action'.   [33] Davidson, 'Actions', p. 697.

strength of his senior honours thesis, which explored Plato's concept of human excellence as described in the *Philebus*.34 Soon after he began graduate studies, however, Davidson found himself 'turned around' by W. V. Quine's course on logical positivism.35 Billed as a 'critical survey of the views of the Vienna Circle and related authors', Quine's course taught Davidson 'that it was possible to be serious about getting things right in philosophy – or at least not getting things wrong'.36 By the time exiled members of the Vienna Circle began gathering in Cambridge, MA, in 1940, Davidson was moving in their direction philosophically, but was obliged to write a dissertation on Plato.

The war intervened to make Davidson's uneasy transition from classicist to analytic philosopher yet more protracted. From 1942 until the end of the war, Davidson served in the US navy. After a brief return to Harvard upon demobilization, he took up a teaching post at Queens College in New York. Although Davidson eventually completed a dissertation on the *Philebus* in 1947, he admitted his aim by that point was 'to write about Plato in as analytic and contemporary a mode as I could'.37 When, in December 1950, Davidson accepted an invitation from his old department chair at Queens to join the faculty at Stanford University, he retained only a teaching interest in classical philosophy, and had as yet no special field or project to call his own.

Because of a remarkable transformation in the political economy of California and its universities, Stanford would play a very active role in shaping Davidson's philosophical ideas. Understanding why requires some regional history. During the Second World War, California had benefited from a bonanza of federal spending, which targeted aeronautics, shipbuilding, basic metals, and oil, but also encompassed the construction and maintenance of large military facilities across the state. California had been the marshalling ground for America's war in the Pacific; the de facto nationalization of the Golden State's hitherto underdeveloped economy entailed not just the short-term expansion of employment opportunities in the military sector but also the government-subsidized expansion of industries like petroleum, metals, and aviation, and the emerging high-tech field of electronics. After a brief economic slump immediately following the end of the war, the population of California exploded. Manufacturing employment boomed as the military, the federal government, and the state's captains of industry worked out a modus vivendi centred on sustained federal patronage and public sector markets for aviation, shipbuilding, construction, and high-tech R&D.38

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34 Davidson, 'The concept of Aretē and the two lives in the *Philebus*' (Honors thesis, Harvard, 1939). Copy in carton 12, Donald Davidson papers, BANC MSS 2005/167, Bancroft Library, University of California at Berkeley (DDP).

35 Davidson, 'Intellectual autobiography', p. 22.

36 *Official register of Harvard University*, 36 (22 Sept. 1939), p. 177; Davidson, 'Intellectual autobiography', p. 23.   37 Davidson, 'Intellectual autobiography', p. 27.

38 Gerald Nash, *The American West transformed: the impact of the Second World War* (Bloomington, IN, 1985); idem, *World War II and the West: reshaping the economy* (Lincoln, NE,

Universities, and the scientists and scholars who staffed them, were compelled rapidly to adapt to the transformed political economy of the American West. A select band of institutions were primed to thrive in the new climate. During the war, the Office of Scientific Research and Development (OSRD), which oversaw all government-sponsored research, had contracted out vital work to a small subset of universities under extraordinarily favourable terms: the contracting institution received, in addition to direct costs incurred, an overhead payment at a flat rate of 50 per cent. These payments for indirect costs were used, in most cases, as unrestricted funds by grateful university administrators. A small number of institutions received the lion's share of federal largesse, which was doled out by OSRD director Vannevar Bush and his colleagues with little congressional oversight. In the east, Harvard and the Massachusetts Institute of Technology (MIT) dramatically expanded their research operations on the back of government contracts. In the west, meanwhile, the University of California (UC) and Caltech were given crucial contracts relating to the Manhattan Project and jet propulsion technology, respectively. Thanks to the post-war settlement, both UC and Caltech were asked to handle large research facilities run during the war by the armed services.[39]

As the Second World War drew to a close, Stanford was in a more precarious situation than its California neighbours. It had missed out on the big wartime contracts, in part because the leaders of the scientific establishment were interested in hot-housing a small number of favoured institutions, and in part because of the Stanford trustees' deep hostility to the idea of public funding for what was a private university. Some of this attitude persisted into the post-war years, when Stanford president Donald Tressider had attempted to stay true to trustee Herbert Hoover's anti-statist principles and attract money for the university from the private sector.[40] However, another important Stanford figure was prescient enough to see that a much larger game was afoot, and that if Stanford was to adapt to the new regime it would have to revise its principles. Frederick Terman was chair of the department of electrical engineering when, in 1942, he was called by his dissertation adviser Vannevar Bush to direct the Radio Research Laboratory (RLL) at Harvard. Although he shared Tressider's and Hoover's view of the natural relationship between private universities and the private sector, Terman's stewardship of the RRL, which was Harvard's largest state-sponsored lab, showed him which way the wind was blowing. Terman realized two things: that electronics, along with other technologies developed in military labs during the war, would have potentially lucrative commercial applications; and that the state, and especially military patrons,

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1990); Roger W. Lotchin, *The bad city in the good war: San Francisco, Los Angeles, Oakland, and San Diego* (Bloomington, IN, 2003); Kevin Starr, *Embattled dreams: California in war and peace, 1940–1950* (New York, NY, 2002).

[39] See Rebecca Lowen, *Creating the Cold War university: the transformation of Stanford* (Berkeley, CA, 1997), pp. 67–82, 97–102.   [40] Ibid., pp. 99–102.

would fuel the R&D economy after the war. Those universities with closest relations to government patrons would grow in wealth and reputation; any private sector alliances would flourish only once that patronage relationship was secure. Upon his return to Stanford in 1945, Terman was determined to ensure that the university would follow the path of Harvard, MIT, and Caltech. Stanford would either become the Harvard of the West, he wrote, or it faced the ignominy of being its Dartmouth.[41]

First as dean of the school of engineering, and then as provost, Terman rebuilt Stanford's faculty and research infrastructure in ways that would allow it to attract funding from civilian and military patrons. In the late 1940s, the most important source of funding came from the military, and especially from the Office of Naval Research (ONR), which paid contract overhead in the same generous fashion as had Bush's OSRD during the war. Terman knew that most of this funding was going to defence-related research in physics, electronics, and applied mathematics, and so he made it his objective to appoint men in these areas who were reliably able to secure contracts.[42] Many, like Terman himself, had held contracts during the war in such areas as radar counter-measures, Operations Research, computing, and nuclear physics. Because he often appointed researchers who were already sponsored by agencies like the ONR, he could split their salaries between Stanford's operating budget and the funds gathered from contract overhead; these savings meant he could make further appointments, and once again salary-split by appointing scientists capable of gathering contracts. Through the 1950s, Terman built in just this manner nationally recognized 'steeples of excellence' at Stanford in physics, engineering, and statistics.[43] Moreover, the contract research these burgeoning departments did for the military was soon fed to the commercial sector, as new businesses in electronics, aeronautics, and related industries began to gather in Palo Alto and to hire Stanford faculty as consultants. Terman encouraged these links by establishing in 1951 the Stanford Industrial Park, now the centre of Silicon Valley. Corporations like Varian Associates and Packard-Bell benefited from expertise developed under government contracts at Stanford, and then sold the resulting commercial technologies to the US military, among other customers.[44] A growth strategy of this scale and complexity required an especially flexible administrative structure, and Terman did not hesitate to

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[41] C. Stewart Gilmor, *Fred Terman at Stanford: building a discipline, a university, and Silicon Valley* (Stanford, CA, 2004), pp. 265–93, 300–47; Lowen, *Creating the Cold War university*, pp. 103–19.

[42] On the ONR and post-war military research agencies, see Amy Shell-Gellasch, 'Mina Rees and the funding of the mathematical sciences', *American Mathematical Monthly*, 109 (2002), pp. 873–89; S. S. Schweber, 'The mutual embrace of science and the military: ONR and the growth of physics in the United States after World War II', in Everett Mendelsohn, Merritt Roe Smith, and Peter Weingart, eds., *Science, technology and the military* (Dordrecht, 1988), pp. 3–45.

[43] Gilmor, *Fred Terman*, pp. 348–419; Lowen, *Creating the Cold War university*, pp. 147–90.

[44] Christophe Lécuyer, *Making Silicon Valley: innovation and the growth of high tech, 1930–1970* (Cambridge, MA, 2005).

downgrade the priorities of department chairs and deans and make appoint-
ments and accept contracts that fitted with his general growth plan.

Working in the fluid but increasingly patronage-focused environment of Cold
War Stanford had two significant consequences for Donald Davidson's nascent
career in philosophy. First, Davidson enjoyed the blessing and the burden of
working in an understaffed but increasingly well-resourced department. He was
obliged to teach across, and thereby train himself in, a broad range of subject
areas. He taught Plato and Aristotle, an ethics survey course, epistemology, logic
at introductory and advanced levels, philosophy of language, the aesthetics
of music, and value theory.[45] At the same time, Davidson had a free hand to
bring in visiting professors and guest speakers from the East Coast and from
Britain. Davidson's guests at Stanford during the 1950s included Gilbert Ryle,
W. V. Quine, Peter Strawson, J. L. Austin, Elizabeth Anscombe, Michael
Dummett, David Pears, Peter Geach, Paul Grice, and David Wiggins.[46] This
provided a stimulus for Davidson to think about neo-Wittgensteinian and
ordinary language doctrines in the theory of action and the philosophy of
language.

Davidson was thus able to give himself an extensive education in the main
currents of post-war philosophy. But the influence of Stanford on his
philosophical development did not end there. Fred Terman followed Harvard
president James Bryant Conant's demand for the self-sustaining financing of
the departments of a university: 'every tub on its own bottom' was Conant's
maxim, and at Stanford this required the collection of soft money and the
practice of salary splitting. Terman insisted that humanities departments stand
on their own two feet. Philosophy department chair John Goheen did his bit by
negotiating contracts with the Group Psychology Branch of the Office of Naval
Research and the Office of Ordnance Research of the US army for work on the
theory of decisions involving risk or uncertain consequences.[47] In 1953, he also
won a grant from the Ford Foundation's Behavioral Sciences Division to
conduct a group study on 'Values, decisions, and rationality' – an enterprise that
eventually became the Stanford Value Theory Project.[48] Each of these grants
and contracts involved interdisciplinary research and cross-faculty collabor-
ation, and they changed, inexorably, the profile of the philosophy department.
One faculty member in particular did more than Goheen to attract and sustain
external funding streams. Patrick Suppes was a philosopher of science and a
methodologist who had been hired fresh from Columbia's graduate pro-
gramme in 1950. Under Suppes's influence, and with the ONR and Army
Ordnance contracts increasingly crucial to the department's viability, the

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[45] Davidson, 'Intellectual autobiography', pp. 30–1; Lepore, 'An interview', p. 250. See also
the teaching materials contained in carton 8, DDP.

[46] Davidson, 'Intellectual autobiography', p. 30.

[47] Lowen, *Creating the Cold War university*, p. 152.

[48] Ford Foundation Grant File 53–82, Ford Foundation Archives, Ford Foundation.

intellectual orientation of the philosophy programme was drawn more toward logic, applied mathematics, and problems in econometrics and psychological scaling. Davidson was drawn into Suppes's orbit, and in the process encountered formal methods in philosophy in the context of applied, interdisciplinary research.

III

To see how this connection with Suppes conditioned Davidson's reception of the tools and techniques of scientific philosophy, we need to look at the latter's work during the 1950s in more detail. In 1952, the Stanford philosophy department hired J. C. C. McKinsey, a leading game theorist and logician who had been working at the RAND Corporation in Santa Monica. Soon, McKinsey, Suppes, and Davidson were collaborating on a set of experimental and theoretical studies in decision theory. As Davidson put the matter, Suppes and McKinsey took him 'under their wing'. Their initial plan was to 'write a paper on the implications for ethical theory of decision theory'.[49] This duly appeared in the journal *Philosophy of Science* in 1955 as 'Outlines of a formal theory of value, I'.[50] Meanwhile, Suppes and McKinsey set out to 'improve [Davidson's] education'– specifically, as Davidson later recalled, by giving him 'as an exercise the simple task of axiomatizing decision theory on the basis of a new primitive concept'. For Davidson, the exercise was 'totally unlike anything I was prepared for' but 'very much in line with what [Alfred] Tarski was training his students to do'.[51] Both Suppes and McKinsey were close to Tarski, who was by now a professor across the Bay at Berkeley.[52] Although McKinsey died unexpectedly in 1953, Suppes had set a course for the department that centred on a programme of axiomatization in the mathematical sciences, and related work in psychological metrics and value theory.[53] Ever the entrepreneur, Suppes began in the early 1950s to split his time and salary between philosophy and Stanford's laboratory of applied mathematics and statistics, a magnet for soft money. Suppes picked up the first of many ONR contracts in the stats lab, and was rapidly promoted up the ranks, becoming a close confidant of Terman's in the

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[49] Ibid., 30.

[50] Donald Davidson, J. C. C. McKinsey and Patrick Suppes, 'Outlines of a formal theory of value, I', *Philosophy of Science*, 22 (1955), pp. 140–60. A further two parts were slated to appear, but did not appear in the form originally intended, perhaps because of McKinsey's death.

[51] Lepore, 'An interview', p. 252; Davidson, 'Intellectual autobiography', p. 31.

[52] Soloman Fefferman and Anita Burdman Fefferman, *Alfred Tarski: life and logic* (Cambridge, 2005), pp. 160–1, 216.

[53] The main concerns of Suppes's work during this period are captured in Patrick Suppes and Muriel Winet, *Axiomatization and representation of difference structures*, Stanford Value Theory Project, Report No. 2 (Stanford University, Stanford, CA, 29 Mar. 1954); Patrick Suppes, 'Some remarks on problems and methods in the philosophy of science', *Philosophy of Science*, 21 (1954), pp. 242–8; Patrick Suppes and Muriel Winet, 'An axiomatization of utility based on the notion of utility differences', *Management Science*, 1 (1955), pp. 259–70.

process. By the late 1950s, Suppes had become, in his own words, a 'conduit' for Terman to make 'good appointments' in the faculty of arts and sciences.[54] Suppes's vision for philosophy, according to Davidson, was clear: to hire 'logicians who knew something about other subjects'.[55] Around the time that 'Actions, reasons, and causes' was published, Davidson was chair of a department that included its own 'Logic Division', staffed by Tarski students or peers George Kreisel, Dana Scott, Solomon Feferman, and William Tait. The general staff of the department had many of its own experts in logic and axiomatic methods, including Jaako Hintikka, Richard Jeffrey, David Nivison, and Suppes and Davidson.[56] Here was a department reconstructed in line with Terman's principles.

A neophyte in the formal programme being put together by Suppes, Davidson completed his homework assignments and around 1953 hit upon an empirical solution to a central conceptual problem in decision theory. Decision theorists were interested in the calculations an agent carried out in deciding between alternatives, one or more of which were gambles, that is, choices whose outcomes depended on a chance event or unknown state of nature. It was evident that an agent's decision between alternatives whose outcome was uncertain would be governed by two factors: their calculation of the probable outcome of the gambles between which they were being asked to choose, their 'subjective probability', and the value they attached to the possible outcomes of their choice, their 'subjective utility'. The difficulty faced by those, in the early 1950s, who wanted to make decision theory a powerful empirical theory of risky decisions – and this, after all, was what the navy and army were paying for – was that subjective probabilities and subjective utilities were hard to disentangle. When Davidson began working on the topic, a number of economists, psychologists, and statisticians had produced empirically testable models for measuring expected utilities given certain patterns of preference revealed in choice behaviour.[57] The problem was that the results of experiments carried out with these models were chronically ambiguous, in so far as the scaling of probabilities usually relied on making assumptions about an agent's utilities or vice versa. As a result, empirical choice behaviour could usually be explained in

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[54] Transcript of interview with Patrick Suppes, Faculty Staff Oral History Project, Stanford University, box 1, SC0932, Stanford University Archives.

[55] Lepore, 'An interview', p. 261.

[56] 'Philosophy 1964–1965' and 'Logic at Stanford' (promotional materials), papers of the Department of Philosophy, Stanford University, series 3, 4430, Stanford University Archives.

[57] William Vickrey, 'Measuring marginal utility by reactions to risk', *Econometrica*, 13 (1945), pp. 319–33; Milton Friedman and L. J. Savage, 'The utility analysis of choices involving risk', *Journal of Political Economy*, 56 (1948), pp. 279–304; Jacob Marschak, 'Rational behavior, uncertain prospects, and measurable utility', *Econometrica*, 18 (1950), pp. 111–41; Kenneth J. Arrow, 'Alternative approaches to the theory of choice in risk-taking situations', *Econometrica*, 19 (1951), pp. 404–37; L. J. Savage, 'The theory of statistical decision', *Journal of the American Statistical Association*, 46 (1951), pp. 55–67; Frederick Mosteller and Philip Nogee, 'An experimental measurement of utility', *Journal of Political Economy*, 59 (1951), pp. 371–404.

two incompatible ways. The key was to find a way of gauging one element without making assumptions about the other. Davidson was delighted to find a way of structuring a choice between gambles such that an agent's indifference between them would show two things: first, that they thought the events on which the outcome of the gambles depended – say, a fair coin's chances of coming up heads or tails – were equally likely; and, second, that the combined outcomes of each gamble were of equal value to the agent. Given this information about the set up of the gambles, and about the agent's choices, one could measure their utilities and probabilities without making assumptions about one to determine the other.[58]

Davidson's discovery was not, in the event, epoch-making. Frank Ramsey had outlined a similar method for assigning subjective probabilities and utilities on the basis of choice behaviour in his remarkable but, in the early 1950s, little-known paper 'Truth and probability', written in 1926.[59] Nevertheless, Davidson and Suppes were able to outline a new set of axioms for choice involving risk that could be tested in controlled conditions. Davidson's experiments, conducted with Suppes and others, produced a stream of articles and technical reports in a field of study that united statisticians, psychologists, economists, and philosophers. Davidson co-authored a paper in *Econometrica* and reported, with the economist Jacob Marschak, on a method for measuring utilities in experimental settings somewhat different from those described by Ramsey.[60] In 1957, he published with Suppes the book *Decision making: an experimental approach*, a founding text in the field of experimental economics.[61] Although these studies were not explicitly philosophical, Davidson considered the techniques he had picked up in the broad field of decision theory and axiomatic methodology an invaluable toolkit for sharpening issues in the philosophy of action, the theory of scientific explanation, and related fields in analytic philosophy.[62] By 1960, while still addressing formal problems in decision theory, Davidson was already speaking in terms that would resonate in 'Actions,

[58] For one of Davidson's expositions of this problem, see Donald Davidson, 'Belief and the basis of meaning', *Synthese*, 27 (1974), pp. 309–23.

[59] Frank Plumpton Ramsey, 'Truth and probability' (1926), in R. B. Braithwaite, ed., *The foundations of mathematics and other logical essays* (London, 1931), pp. 156–98.

[60] Donald Davidson and Patrick Suppes, 'A finitistic axiomatization of subjective probability and utility', *Econometrica*, 24 (1956), pp. 264–75; Donald Davidson and Jacob Marschak, 'Experimental tests of the stochastic decision theory', in C. West Churchman and Philburn Ratoosh, eds., *Measurement: definitions and theories* (New York, NY, 1959), pp. 233–69. See also Suppes and Winet, *Axiomatization and representation*; Donald Davidson and Patrick Suppes, *Finitistic rational choice structures*, Stanford Value Theory Project, Report No. 3 (Stanford University, Stanford, CA, 4 Mar. 1955); Donald Davidson, Sidney Siegel and Patrick Suppes, *Some experiments and related theory on the measurement of utility and subjective probability*, Stanford Value Theory Project, Report No. 4 (Stanford University, Stanford, CA, 15 Aug. 1955).

[61] Donald Davidson and Patrick Suppes, with Sidney Siegel, *Decision making: an experimental approach* (Stanford, CA, 1957).

[62] See, most explicitly, Donald Davidson, 'A new basis for decision theory', *Theory and Decision*, 18 (1985), pp. 87–98.

reasons, and causes'. In a paper for the International Congress for Logic, Methodology and Philosophy of Science, Davidson argued that formal theories of decision dealing with voluntary actions had to interpret 'the set of alternatives or actions between which choices or decisions are to be made' in 'the same way it is interpreted by the subject'. This was the language of sub-jective probabilities and subjective utilities, but also a methodological stricture Davidson would invoke in his discussion of primary reasons. Modern theories of decision, Davidson also claimed in the paper, 'explain and predict decisions in the same way theories in other branches of science explain and predict the phenomena with which they deal'.[63] Actions or decisions were not to be explained outside of the conceptual resources of the sciences generally; again, this was a conviction that found voice in Davidson's 1963 paper.

Davidson's work on 'Actions, reasons, and causes' began in earnest in the spring of 1963, when Sidney Morgenbesser, the editor of the *Journal of Philosophy*, solicited from Davidson a review of one of the Wittgensteinian red books, Anthony Kenny's *Action, emotion, and will* (1963). Morgenbesser en-couraged Davidson to 'take the opportunity to discuss your own ideas on utility, game theory, etc., using [the Kenny] book as [a] springboard'.[64] And Davidson, naturally enough, did draw on some of the concepts and theoretical com-mitments he found in decision theory and related areas of the mathematical and behavioural sciences.

In seeking to make sense of the connections between Davidson's commit-ments to decision theory and axiomatics, on the one hand, and his defence of a causal approach to the explanation of action, on the other, we must tread care-fully. Readers of 'Actions, reasons, and causes' and related essays of Davidson's on the nature of agency, the interpretation of behaviour, and the philosophy of mind will know that, despite their author's frequent gestures toward his experimental work in the 1950s, much of the art of Davidson's inquiries lies in his exploration of the limits of strict, formal theories.[65] We do not simply add Cold War-inspired theories of risk-taking to Aristotle and arrive at the critique of anticausalism presented by Davidson in his seminal 1963 paper. One extra element that would have to be added to this equation is Davidson's engagement with theories of scientific explanation, especially as these theories turned on the standards that any authentically causal explanation would have to meet. Nevertheless, it is reasonable to conclude that Davidson was sensitive to the causal force of reasons (or, more accurately, beliefs and desires) precisely because his work in decision theory had taught him that beliefs and desires could be measured independently of one another and treated as separate

[63] *International Congress for Logic, Methodology, and Philosophy of Science: Abstracts of Contributed Papers* (Stanford University, Stanford, CA, 24 Aug. – 2 Sept. 1960), Folder – box 4440–2: Logic, Papers of the Department of Philosophy, Stanford University, Stanford University Archives.

[64] Morgenbesser to Donald Davidson, 15 May 1963, carton 3, DDP.

[65] A point urged in Davidson, 'A unified theory of thought, meaning, and action', in *Problems of rationality*, pp. 151–66.

elements to be employed in the explanation of the decisions and other actions of agents.

Accustomed to the rigorous conception of theory construction championed by Tarski and Suppes and exemplified by work in decision theory, Davidson came to the work of the philosophical psychologists like Kenny and Anscombe with a sceptical eye. The Wittgensteinian observation that explanations of action involved elements of redescription was from Davidson's perspective not on its own enough to force a departure from the scheme of scientific explanation he had learned to use during the 1950s. In 'Actions, reasons, and causes', Davidson defiantly reasserted this scheme and showed how it could treat practical reasoning in ways far more subtle than the narrowly Aristotelian or Wittgensteinian approach to practical reason in terms of the practical syllogism or the 'logic' of mental concepts. The anticausalists, for Davidson, had shown far less about the structure of practical reasoning and human action than they thought they had. The modern decision sciences, and the axiomatic treatment of scientific theories, laid bare just what they had missed.

IV

Stepping back from my case-study of the early Davidson, I want to close by identifying two core characteristics of the interpretive framework I am recommending for the history of post-war analytic philosophy. The first concerns the emphasis on decision-theoretic accounts of practical reasoning and the role that the Second World War played in this shift. It also brings logical empiricism back into the picture, but on more complex terms than historians have typically invoked. What I want to suggest here is that the Bayesian turn in post-war American philosophy was undoubtedly catalysed by the imperatives of war research, and by the subsequent rise of the Cold War military-industrial-academic complex; nevertheless, we have to see this transformation as an acceleration of a probabilistic revolution in semantics and epistemology that began within scientific philosophy during the interwar years. During the decade leading up to American intervention in the war, a number of former members of the Vienna Circle had recanted their commitment to the doctrine of verificationism and instead began both to address the logic of inductive reasoning, and to speak of 'degrees of confirmation' in the theories of knowledge and meaning.[66] These debates about probability and induction formed a crucial part of the heritage of logical empiricism as it was received in the United States. But the war transformed formal concerns with the structure of scientific languages

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[66] Herbert Feigl, 'The logical character of the principle of induction', *Philosophy of Science*, 1 (1934), pp. 20–9; Hans Reichenbach, 'Induction and probability', *Philosophy of Science*, 3 (1936), pp. 124–6; Rudolf Carnap, 'Testability and meaning', *Philosophy of Science*, 3 (1936), pp. 419–71; idem, 'Testability and meaning – continued', *Philosophy of Science*, 4 (1937), pp. 1–40; Hans Reichenbach, 'On the justification of induction', *Journal of Philosophy*, 37 (1940), pp. 97–103.

and the epistemological problem of confirmation into problems of practical reason and resource allocation. In this new context, formal methods in scientific philosophy were encountered as tools used in the assessment of decision-making and complex operations. Such an emphasis marked the later work of Davidson, Suppes, and others in the fields of decision theory and measurement.

Some historical background will help to fill out this claim. Following the lead of the British, who employed civilian scientists like P. M. S. Blackett to plan and assess military operations during the war, Vannevar Bush created the Applied Mathematics Panel as a division of the wartime Office of Scientific Research and Development under the leadership of Warren Weaver.[67] Weaver, in turn, contracted out to Columbia, Princeton, and a handful of other universities for applied work on the effectiveness of bombing strategies, anti-aircraft fire patterns, and general calculations for the 'improvement' or 'best use' of weapons technologies.[68] These groups of civilian scientists were typically directed and staffed by statisticians, but they often included experts in mathematical logic. Columbia's Statistical Research Group, for example, which was the largest of the groups under contract with Weaver's Panel, employed as researchers or associates Leonard Savage, George Stigler, Frederick Mosteller, and Milton Friedman.[69] It is often correctly noted that the Second World War, for all of its expansion of the field of statistics and probability theory, did not produce a revolution in this area as it had in computing or electronics. But it had two other notable effects. First, it confirmed among US-based statisticians and theorists of probability a concern for the subjective interpretation of probability – probability conceived as a measure of subjective belief, rather than a measure of frequency. These different interpretations of the theory of probability had been a source of fierce debate before the war, and would become so again, but, for a time, the war stacked the deck in favour of the subjectivists.[70] Second, Operations Research offered some (although by no means all) philosophers of science a model of their discipline. During the war, Operations Research teams in the US military

[67] Erik P. Rau, 'The adoption of Operations Research in the United States during World War II', in Thomas P. Hughes and Agatha C. Hughes, eds., *Systems, experts, and computers: the systems approach in management and engineering, World War II and after* (Cambridge, MA, 2000), pp. 57–92; Erik P. Rau, 'Technological systems, expertise, and policy making: the British origins of operational research', in Michael Thad Allen and Gabrielle Hecht, eds., *Technologies of power: essays in honor of Thomas Parke Hughes and Agatha Chipley Hughes* (Cambridge, MA, 2001), pp. 215–52; William Thomas, 'The heuristics of war: scientific method and the founders of operations research', *British Journal for the History of Science*, 40 (2007), pp. 251–74.
[68] Mina Rees, 'The mathematical sciences and World War II', *American Mathematical Monthly* 87 (1980), p. 611.
[69] Ibid.; Shell-Gellasch, 'Mina Rees', p. 878; Mirowski, 'Cyborg agonistes', pp. 699–703.
[70] See, e.g., Friedman and Savage, 'Utility analysis'; Savage, 'Theory'; Milton Friedman and L. J. Savage, 'The expected-utility hypothesis and the measurability of utility', *Journal of Political Economy*, 60 (1952), pp. 463–74; L. J. Savage et al., 'On the foundations of statistical inference: discussion', *Journal of the American Statistical Association*, 57 (1962), pp. 307–26.

were not limited to studies of ordnance, and were asked to examine battlefield operations, the organization of supply chains for the armed services, and R&D in the wartime weapons labs.[71]

After war, this kind of expertise among logicians and statisticians was parlayed into consultancy work for the armed services. The RAND Corporation, which began life as an air force-funded research-and-development division of the Douglas Aircraft Company, employed several philosophers and logicians. These included two philosophers whom Bertrand Russell had taught at the University of Chicago – Abraham Kaplan and Norman Dalkey – along with Olaf Helmer, J. C. C. McKinsey, and Hans Reichenbach. Even W. V. Quine landed a summer gig at RAND, where he tinkered with Kenneth Arrow's impossibility theorem, another RAND product.[72] For some, philosophy of science was no longer a metamathematical or linguistic exercise in the 'logic of science', as Carnap imagined it to be: it was an applied discipline that dealt with methodological issues across the sciences, from problems in measurement to questions of decision, strategy, and general value. In the decade after 1945, C. West Churchman, Patrick Suppes, and Russell Ackoff devoted articles in the journal *Philosophy of Science* to sketches for a new applied approach in the discipline, one closely modelled on the Operations Research tradition.[73]

All of which is to say that Davidson and his peers were not simply picking and choosing at random among formal methods when they hit upon significant seeming problems in theories of decision or probability theory. These interests had been prepared by the exigencies of applied research in the war and the influence this had on philosophical methods in the Cold War system of research patronage.

The second, closely related characteristic of the framework I seek to endorse for the history of the analytic revolution concerns the concept of value. This once again takes us back to the revival of the theory of practical reasoning and the philosophy of action. As we have seen, the central feature of decision theory was its combination of assessments of subjective probability with measures of subjective utility, a twin concern that led Savage, Friedman, and Mosteller from their research for the Applied Mathematics Panel to applied work in expected utility theory after the war.[74] It is important here not to be misled by the economic terminology and to recognize that so-called 'utility theory' in the

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[71] M. Fortun and S. S. Schweber, 'Scientists and the legacy of World War II: the case of operations research (OR)', *Social Studies of Science*, 23 (1993), pp. 595–642; Stephen P. Waring, 'Cold calculus: the Cold War and Operations Research', *Radical History Review*, 63 (1995), pp. 29–51; Mirowski, 'Cyborg agonistes'; Rau, 'Adoption'; Shell-Gellasch, 'Mina Rees'; Thomas, 'Heuristics'.

[72] Mirowski, 'Cyborg agonistes', p. 705; Mirowski, 'Scientific dimensions', pp. 309–10; W. V. Quine, *The time of my life* (Cambridge, MA, 1985), p. 217.

[73] C. West Churchman and Russell L. Ackoff, 'Varieties of unification', *Philosophy of Science*, 13 (1946), pp. 287–300; Russell L. Ackoff, 'An educational program for the philosophy of science', *Philosophy of Science*, 16 (1949), pp. 154–7; Suppes, 'Some remarks'.

[74] See, e.g., the references in nn. 57 and 70.

immediate post-war period was not the sole provenance of economics professors; it was connected to an interdisciplinary sector of the behavioural sciences that focused on practical reasoning. Statisticians, psychologists, philosophers, and economists were interested in connecting probability theory, inductive logic, and decision-making to the theory of value. Although this was in many respects a heady conjunction of ideas forged by applied research during war, it also belonged to a venerable strand of American philosophy. Through the 1940s and early 1950s, philosophers like John Dewey, Clarence Irving Lewis, and Ralph Barton Perry sought to make a case for what they called an 'empirical ethics': a theory of value judgments guiding actions that was consonant with the account of predictive empirical judgments developed in the philosophy of the experimental sciences. Economic and psychological investigations of utility were not foreign to these major thinkers of the American philosophical tradition.[75]

Although normative political and moral theory may have fared poorly in the new regime of behavioural and mathematical sciences, concepts of value and ethics did not disappear from the scene as the analytic tradition in America took shape. Far from it. Davidson's work on decision theory was especially ecumenical in its treatment of the problem of value. His 1955 paper with Suppes and McKinsey, 'Outlines of a formal theory of value', took care to address, if in a critical fashion, the accounts of evaluative reasoning presented by Dewey, Lewis, and Perry. Davidson's notes for the paper indicate that he was the author pushing for this wider philosophical orientation.[76] In addition, the formal limits placed in the essay on what could count as a rational pattern of preferences were deliberately designed to be weak; the intent of Davidson and his co-authors was simply to present a definition of a rational ordering among a set of options and to establish the possibility of measuring utilities. In the years after he published 'Outlines', Davidson offered a seminar in formal value theory, in which he addressed a remarkable array of texts from across the disciplines.[77] His work for this seminar encompassed studies in game theory by Anatol Rapoport, R. M. Hare's *The language of morals* (1952), Kenneth Arrow's *Social choice and individual values* (1951), Stephen Toulmin's early treatise on ethics, John Rawls's 'Two concepts of rules' (1955), Felix Kaufman's *Methodology of the social sciences* (1944) – the list goes on. As he was beginning this research,

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[75] Ralph Barton Perry, 'The definition of value', *Journal of Philosophy, Psychology and Scientific Methods*, 11 (1914), pp. 141–62; idem, 'Economic value and moral value', *Quarterly Journal of Economics*, 30 (1916), pp. 443–85; idem, 'Value as election and satisfaction', *International Journal of Ethics*, 41 (1931), pp. 429–42; John Dewey, *Theory of valuation* (Chicago, IL, 1939); idem, 'Valuation judgments and immediate quality', *Journal of Philosophy*, 40 (1943), pp. 309–17; idem, 'Some questions about value', *Journal of Philosophy*, 41 (1944), pp. 449–55; Clarence Irving Lewis, *An analysis of knowledge and valuation* (La Salle, IL, 1947); Mosteller and Nogee, 'Experimental measurement'.
[76] See the notes in folder marked '2/61: 'Outlines of a formal theory of value', carton 2, DDP.
[77] See folder marked '2/62: 'Outlines of a formal theory of value (seminar)', carton 2, DDP.

Davidson described his general research programme as the '[s]tudy of the character of inference, evidence, postulation, and concept formation in ethics and other modes of value'.[78] A few years later, he was promising the American Council of Learned Societies that he would 'write a book on the logic and psychology of moral reasoning, with [an] emphasis on the relations between philosophical problems and decision making as studied by economists, psychologists, and other social scientists'.[79] In the 1950s, Davidson saw his project as relevant to moral philosophy, ethics, and decision theory, among other fields – both the war and the philosophical culture of Stanford itself meant that he did not expressly need to choose between these options.

So Davidson, like many post-war philosophers, was operating in a remarkably open field, in which the methods and programmes of logicians, philosophers of science, economists, statisticians, and psychologists were inextricably entangled. Counterintuitively, perhaps, the theoretical framework that pulled all of these disparate elements together was that of practical reasoning – and not, as might be expected, formal epistemology and semantics. In 'Actions, reasons, and causes', Davidson managed to put these influences to work in a revisionist account of the explanation of action. This account called upon, but in important ways went beyond, his training in the mathematical and behavioural sciences.

With further specification of the interactions between decision theory, utility theory, statistics, and scientific philosophy, we could shed more light on cognate projects in American analytic philosophy of the 1950s and 1960s. What we need, in the study of the analytic tradition in the United States, is not a story of declension or triumph revolving around the émigré logical empiricists and their interlocutors. We need to know about the array of formalisms emerging from upheavals across the disciplines in the middle decades of the twentieth century, and their integration into the projects that defined post-war American philosophy. The questions of practical philosophy – of value, action, and ethics – will be central to such an enterprise. Moving forward, we need both institutional studies and detailed textual exegesis. This should also bring the history of American philosophy into contact with the history of science, and especially of the human sciences. Indeed, I would suggest that, to achieve a comprehensive understanding of the emergence of an analytic mainstream in American philosophy after 1945, we must attend to philosophy's place among the behavioural and mathematical sciences.

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[78] Donald Davidson, fellowship application to the Fund for the Advancement of Education, c. 1952, carton 2, DDP.
[79] Davidson, fellowship application to the ACLS, 14 Oct. 1957, carton 2, DDP.