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Preface

It is a pleasure to recognize the debt I owe to Isaiah Berlin, who unwittingly set me off on the inquiries of which this book is a belated fruit. In 1948 he invited me—as a young research fellow of King's College, Cambridge—to spend time at our sister college, New College, Oxford, where he was the Philosophy Tutor; there he told me in terms that I never forgot that, for Anglo-American philosophers, the History of Ideas was a *non*-subject. Since that time he has shown that a historical grasp of social, political, and scientific ideas is indispensable if we are to make sense, either of Modernity in general, or of Modern Philosophy in particular.

Isaiah's insights remained with me during the years when I was working in the history and philosophy of science, and underlie my concerns in the present book. This work extends into the social, economic, and practical realms the critique of theory to which I was led by a Wittgensteinian approach to the physical or biological sciences, and by the historical reinterpretation of the seventeenth-century Scientific Revolution that is the core of my book *Cosmopolis*. To sum up the central themes of the present book, let me quote a radio talk on "Political Judgement," which Isaiah gave for the BBC Third Programme on June 19, 1957. The idea that political science rests on laws and experiments like those of physics, he said, "was the notion, either concealed or open, of both Hobbes and Spinoza, each in his own fashion—and of their followers—a notion that grew more powerful in the eighteenth and nineteenth centuries, when the natural sciences acquired enormous prestige, and attempts were made to maintain that anything not capable of being reduced to a natural science could not properly be called knowledge at all."[1] Quite the reverse, he continued:

"The arts of life—not least of politics—as well as some among the human studies turn out to possess their own special methods and techniques, their own criteria of success and failure . . . Bad judgement here consists not in failing to apply the methods of natural science, but, on the contrary, in over-applying them . . . To be rational in any sphere, to apply good judgement in it, is to apply those methods which have turned out to work best in it . . . [To demand anything else] is mere irrationalism."[2]

In the fifty years since my visit to Oxford, I have incurred plenty of other debts—more than I can redeem here. Aside from Ludwig Wittgenstein's classes and R. G. Collingwood's writings, I was directed to questions about practical wisdom by the urgent concerns of my friends in the fields of Medicine—especially Mark Siegler and Christine Cassell—in Engineering—notably Albert Danielsson in Stockholm and Yoichi Arai in Tokyo—and in Action Research—particularly Björn Gustavsen and his colleagues at the Swedish National Institute for Working Life. In all of these areas, I recognize an intellectual kinship with Hans van Beinum in Sweden, Claude Faucheux in France, and Richard Ennals of Kingston University in England, as well as the benefit of continuing exchanges with my colleagues and co-authors, Allan Janik, Albert Jonsen, and Richard Rieke. Parts of this book have been published previously in a different form: the chapter on Method, for instance, revisits an essay in the book *Beyond Theory*, from the John Benjamins series, *Dialogues on Work and Innovation*.

Working with the National Commission for the Protection of Human Research Subjects in Bethesda, Maryland, taught me a lot about case methods in Medicine and Ethics, and led me to reread Aristotle's *Nicomachean Ethics* in a new, clinical light. This reading was reinforced by reflection on the essays of William Gass. A dispute with Harry Johnson in the 1960s about the justification of government science policy, in Edward Shils's journal *Minerva*, led me to despair over current economic theory; but later I regained enough confidence to revive my criticism of the methods of economic theory, with the help first of BDO Groningen, for which I lectured in 1995, next of Kenneth Mischel, Joseph Heilbronner, and colleagues at a symposium held at Baruch College, New York, and chiefly from my daughter Camilla, of the International Institute for Environment and Development, who does what she can to rescue me from my worst

confusions about applied economics and the work of nongovernmental organizations.

For several years, I have been meeting with Ton Meijknecht of the Delft Institute of Technology to discuss the moral problems facing engineers and technologists in their professional life; his deep questions about the standards involved in these disciplines are a constant challenge. In revising this book, I had constructive comments from Steve Fuller, Nancey Murphy, and notably Steve Shapin, who urged me—not wholly successfully—to soften my criticism of the risks of the disciplinary rigor that shapes the departmental life of our universities.

Academics who criticize the Academy, of course, put themselves at risk. Few people are as helpful to us as the students we encounter in our colleges or universities, whose graduate work we oversee: here, let me mention Nancy Baker, Jim Block, Daniel Herwitz, Mike Hickey, Robert Nelsen, Lisa Raphals, and above all Richard Schmitt, who has been a continual source of useful references. At the last moment, too, my good friend Jack Bemporad saved me from some foolish errors.

Institutionally, I have in recent years received important support from the Henry R. Luce Foundation and the National Endowment for the Humanities, which chose me as Jefferson Lecturer for 1997; also from Clare Hall, Cambridge, and the Tanner Trustees, as well as more colleagues than I can enumerate at the University of Chicago, Northwestern University, and the University of Southern California. Without the help of Morty Schapiro and his fellow Deans in the College of Letters, Arts, and Sciences at U.S.C., little of what I do here would have been achieved.

Gerald Holton made possible my move to the United States in the 1960s, and our like-mindedness has always been an encouragement. Marx Wartofsky and Robert Cohen, with their Boston Colloquium for the Philosophy of Science—always the wild card in an over-formal scene—have provided friendly and collegial support to all those of us who were willing to cross academic boundaries in the pursuit of a broadly-based understanding of the varied enterprises of science. Their friendships are something the rest of us never forget.

Joyce Seltzer, my editor at the Free Press and later at Harvard University Press, knows how far she is responsible for what merits this book—like *Cosmopolis*—may prove to have: at every stage in its conception and development her keen intelligence and tact have helped me to write a book

that technical readers should take to heart, yet non-technical ones can enjoy. Not least, Holly Hebert of WordsWorth at Los Feliz, Los Angeles, took on the work of producing a publishable text: her enthusiastic response to this task made me feel that I had succeeded in doing what we had all aimed to do.

They will get it straight one day at the Sorbonne.
We shall return at twilight from the lecture,
Pleased that the irrational is rational.

Wallace Stevens, *Notes toward a Supreme Fiction*,
X, ll. 16–18

I

Introduction: Rationality and Certainty

Intellectuals in the year 2000—philosophers or social scientists, literary critics or economists—have inherited a family of problems about the idea of Rationality and its relations to those of necessity and certainty. But they tend to ignore the more practical, complementary idea of Reasonableness, or the possibility of living, as in pre-modern times, without any absolute necessities or certainties.

In the twentieth century, scholars in the universities of Europe, North America, and their zone of influence have been preoccupied with the concept of *rationality:* preoccupied, at times, to the point of obsession. This is true in academic philosophy, the behavioral and social sciences, and even—more recently—across the whole spectrum of academic fields, from the physical sciences at one pole to the humanities at the other. As a result, subjects like comparative literature, linguistics, and aesthetics have refocused on methodological questions about the legitimacy of ideas and ways of thought whose validity they had previously taken for granted.

Eighty or ninety years ago, scholars and critics, as much as natural scientists, shared a common confidence in their established procedures. The term "scientific method" embraced, for them, all the methods of observation, deduction, generalization, and the rest that had been found appropriate to the problems and issues preoccupying those subjects. How little of that confidence remains today! Among some humanists, the phrase "scientific method" is even pronounced with a sarcastic or ironic tone; and one even hears it argued that the concept of rationality itself is no more

than a by-product of Western or Eurocentric ways of thinking. From its earlier dominance, through a period of doubts and difficulties between the two world wars, to the downright skepticism of contemporary debate, the claims of rationality have been progressively challenged, to the point of being sidelined.

In focusing attention on *rationality*, however, whether to praise it or to challenge it, academic writers have neglected to analyze the complementary concept of *reasonableness*. In the World Academy (it seems) the term "rationality" can amount to anything, only if it amounts to everything: otherwise, it will amount to nothing, and the claims made on its behalf will become absurd. Only in the last few years, in this respect, has the tide turned. In medical ethics, ecology, and other practical fields, the years since the 1960s have seen a revived interest in questions about values that for a while had come to appear foreign even to philosophy. This turning of the tide points to a future in which the rational demands of scientific technique will be balanced by attention to the demands of the human situations in which intellectual or practical skills can reasonably be put to use.

For now, however, the spotlight remains on the intellectual validity of Rationality itself: the human values of Reasonableness are expected to justify themselves in the Court of Rationality. The question has not yet been generally accepted in the Academy—let alone any answer agreed upon—whether the twin concepts of "rationality" and "reasonableness" are not interdependent ideas, of comparable authority and philosophical interest. Indeed, it is not always recognized that the two ideas can be distinguished. Some European languages use only one word for both concepts. In German, for instance, a single word (*Vernünftigkeit*) serves as a translation of both English words; you may hear the word *Rationalität* uttered in seminars devoted to the discussion of Anglo-American philosophy, but it does not have any lexicographical standing except as a technical barbarism.[1]

How do these two concepts differ from, and relate to, each other? And how did we reach a point at which they came to be at cross-purposes with each other? On its face, this is a historical problem, to be answered in historical terms. Yet on what level, and in what kind of terms? In his noted book *A History of Civilizations*, Fernand Braudel has distinguished three different levels of narrative and analysis. On the day-to-day level of *events*, the traditional historian "hovers from one event to the next like a chroni-

cler of old or a reporter [but] too often leaves us unsatisfied, unable to judge or to understand." On a second level of *episodes,* which typically last "ten, twenty or fifty years," facts are grouped, interpreted, or explained as forming (say) the French Revolution, the rise of Romanticism, or the First World War. These may still include "events of long duration," but they are "stripped of superfluous detail" and so given explanatory force.

Still, there is another, much longer term perspective:

> At this level, the movement of history is slow and covers vast reaches of time: to cross it requires seven-league boots. On this scale, the French Revolution is no more than a moment, however essential, in the long history of the revolutionary, liberal and violent destiny of the West. Voltaire, likewise, is only a stage in the evolution of free thought.

On this final level—what Braudel calls *la longue durée*—civilizations are distinct from the accidents and vicissitudes that mark their development. Any historian who embarks on this kind of analysis launches himself into "blue-water cruising on the high seas of time, rather than prudent coastal navigation never losing sight of land." This adventure is only to be undertaken circumspectly. To this day, professional historians do not agree on whether Arnold Toynbee's *Study of History* successfully avoided the same weaknesses and risks as those of Braudel, or whether, like Oswald Spengler's *Decline of the West,* it was uncritically over-enthusiastic. If we are to put out onto the ocean here, we will not go too far beyond the horizon, but will remain, like Braudel, within the reach of the coastal lights by which we can check our navigation.[2]

The sudden loss of confidence in our traditional ideas about rationality in the last twenty or thirty years is marked enough, and widespread enough, to constitute (in Braudel's terms) an episode, not just a collection of contemporary events: many writers today refer to it as the End of Modernity. Dagmar Barnouw, for instance, has referred to this change as the development of a "post-culture":

> The 20th century has been the age of the aftermath: post-modern equals post-war, post-holocaust, post-colonial, post-gender, post-history, and, most important for the cultural critic's enterprise, post-'master narrative.'[3]

To write about the change in such terms is at once to relate it to the historical development we know as Modernity itself: as to that, the question is still arguable whether Modernity is part of the long march of human destiny, or "only a stage in the evolution of free thought." Here I shall be primarily concerned with the relations between changes in our twentieth-century ways of thinking and the longer-term episode of Modernity, from the late sixteenth century to the present day. But I shall also look, from time to time, at some "deep water" questions, with a time scale closer to three thousand years. How far, then, do the leading themes of Modern Thought and Practice harmonize with the longer-term histories of philosophy and human self-understanding generally? And in what ways have European thought and action over the last four hundred years been at odds with that longer journey?

Looking at the phases which our confidence in rationality and scientific method has passed through during the twentieth century, we can identify several stages. Trust in the procedures of intellectual inquiry went hand in hand with a view of language and meaning as embodied in "propositions" that represent "facts" in the world. Colloquially, this idea is captured in the everyday statement, "The cat is on the mat," which reports a situation that we can instantly visualize; technically, it is captured by Ludwig Wittgenstein's statement, *Wir machen uns Bilder der Tatsachen* ("We create representations of facts for ourselves").[4] Seen from this point of view, language is an enterprise in which, among other things, we fashion representations of situations, or states-of-affairs, and rational inquiry helps us find the truth about these situations by examining the relations between such observations and the hypotheses to which investigation leads us.

The key word in that sentence is "relations"; and the stages through which the idea of rationality has passed reflect changes in our assumptions about those relations. For the Vienna Circle philosophers of the 1920s and 1930s, and the logical empiricists who continued their work in the United States after World War II, they were *logical* relations in a narrowly formal sense of the term. Scientists advanced their speculations as hypotheses, and these could be accepted as established truths if and only if they were supported by sufficient evidence. Both hypotheses and evidence were presented in propositions, and the task for philosophers was one of "inductive

logic": analyzing the formal links between hypotheses on the one hand, and reports of evidence on the other. Very varied accounts were given of the formal relations required to show that a hypothesis was rationally adequate; the terms "verification," "falsification," and "corroboration" (inter alia) were used to mark the differences between these accounts.[5]

The formal relation between evidence and hypotheses was, however, only one of the central issues for the new Inductive Logic. In addition, there were questions about the relations between the propositions within any scientific theory. As to that, there was little disagreement among the Viennese philosophers of science or their post-war successors. Initially, they all took it for granted that scientific theories can be formulated as axiomatic systems, on the model of Euclid's *Elements of Geometry*, with statements of principle serving as axioms, and factual observations being interpreted as deductions from those principles in the given situation. Logicians thus had the authority both to judge the validity of theoretical systems and to measure their evidential support; and the solutions to both sets of problems were to be given in the formal Euclidean style.

One aim of the present book (let me say right away) is to show the error of both these views. Despite Newton's reliance on a Euclidean model in his mathematical theory of dynamics, Euclid's geometry was never a good model for scientific theories in general; nor can one give a good general account of the relations among observations and theories by treating them as formal relationships between different propositions. On the contrary, we can establish formal relations between observations and the hypotheses they support only after those observations are massaged into theoretical terms. As for the cult of axiomatics, which was popular among American social and behavioral scientists up to the 1950s, this was ill-adapted to the needs of such disciplines, and they are now learning to cultivate their links with the Biological Sciences rather than with Newtonian Physics alone.

The major break with this approach—what is widely referred to as the "positivist" approach—came with the success of Thomas Kuhn's widely admired book, *The Structure of Scientific Revolutions*. Not that Kuhn was by any means the first to present a serious critique of the positivist philosophy of science. The Polish pathologist Ludvik Fleck and Wittgenstein's pupil W. H. Watson both presented strongly worded alternatives in the years before the Second World War, but there was little audience for their criticisms until the 1950s. Even so, Kuhn's attack on the standard approach did

not go very far. His book would have best been called *Revolutions in the Structure of Science*. He did not seek to undercut the Euclidean assumption that theories should have a logical structure; he argued only that they are from time to time subject to drastic reconstructions, after which they take on different axiomatic structures. (The modesty of his argument became clear in the second edition of his book, where he explained that he had only meant to underline the fact that there are no purely deductive relations between pre- and post-revolutionary theories: after all, he added, is this not the difference between "deduction" and "induction"?)[6]

It took a second step to break more effectively with the positivist approach to the natural sciences. This came with the philosophers' growing realization that changes in our basic scientific concepts involve more than changes in the logical structures of theories. During much of the 1960s, the central issue under discussion was the problem of conceptual change. How can we offer a "rational" account of this process, if we give up the formal methods of Viennese inductive logic? The underlying motto was Hilaire Belloc's maxim, "always to keep hold of Nurse / for fear of meeting something worse": formal logic gave philosophers of science the reassurance that irrationalism might yet be avoided, though many, even R. G. Collingwood, concluded that on a deep level conceptual changes must be explained in *causal*, not *rational*, terms. (Being ahead of the game, Collingwood advanced powerful arguments to this effect in his *Essay on Metaphysics* before the Second World War, and was attacked for being a Marxist!)[7]

As a result of fresh interactions between philosophers and historians of science in the 1960s, another stream joined with this one. So long as George Sarton at Harvard ruled over academic History of Science in the United States, collaborating with philosophers was taboo. This separation of the disciplines suited those logicians who were anxious to defend the historical immutability of Reason and Rationality. They were only too happy to follow Gottlob Frege's injunctions to avoid the "historicist" fallacy: it took a level head to keep the conceptual change debate in the middle of the road between formal logic and historical relativism.[8]

At this point, the argument was already on the verge of the skepticism I noted at the outset. Perhaps it was always an illusion to believe that people from different cultures can understand one another's scientific theories any more than can people from different historical periods. If that were so,

is it even clear that people from the same culture at the same time are capable of reaching intellectual consensus? In this way the idea of rationality became as open to idiosyncrasy as those of justice or morality. (Alasdair MacIntyre's book title, *Whose Justice, What Rationality?*, says it all.)[9]

This is my reading of the historical phases by which the rationality debate reached definitive form in the years from the 1920s to the 1990s. My aim in the chapters that follow will be to steer a middle way, and to show how the idea of Reasonableness lets us keep on an even keel. Yet one preliminary question must be addressed: what kind of evidence or testimony can we rely on in these chapters? Here let me forestall the objection that I am falling into a foreseeable trap, of substituting autobiographical recollections for an analysis of the changes involved in eighty years of intellectual history. Such a criticism misses the point. By now, the challenges to the concept of rationality are so extreme that a theoretical analysis of the period will carry "rational" conviction for only a small cadre of readers. The only way to proceed, therefore, is to go behind all the rival theoretical positions and present a narrative with a personal perspective. Yet what can such a personal narrative do for us? Will not my personal background and standpoint inevitably slant it? So how can I claim to be throwing light on the history of twentieth-century thought "as it really happened"?

This objection can be undercut at the outset in philosophical terms. The view that each of us has of the events through which we have lived is inevitably *incomplete*, but that is not the same as being *slanted:* that is, biased to the point of actual distortion. So the claim that there is no way to avoid bias or distortion—that a man can never appreciate a woman's point of view, a Christian a Buddhist's, an Albanian a Serb's—elevates a practical problem to the level of an outright impossibility. Instead, we may state the point in anthropological terms: if the following account relies, as it sometimes will, on my memory of events and changes, it does so for ethnographic, not egotistical, reasons. I shall treat myself here as a "native informant" whose testimony is sufficiently reliable for present purposes, even if it is not supported by the costly data collection and analysis that some sociologists would prefer. Let the resulting narrative stand on its own, for what it is. Others will tell the same story differently, and these differences may be illuminating; but, under the circumstances, a vast amount of statistically backed documentation would at best increase the bulk of

the argument, without adding to its weight. If the general outlines of the story are sound, that is all that our present purposes demand.

It remains to make explicit the angle from which my story is told; this will answer the question that reentered intellectual debate in the 1960s, "Where are you coming from?" It was no accident that the question of alternative perspectives emerged out of the world of colloquial conversations and personal exchanges of opinion, not in the formal realm of the Academy: in academic debates, we are always challenged to frame our arguments in terms appropriate to one discipline or one forum of argument rather than another, not in general nondisciplinary terms or in ways open to a lay public. Like individuals, academic disciplines have their chosen perspectives, and this selectivity may have the effect of needlessly limiting our chosen arguments.

Very well then: I shall not adopt the standpoint of any one particular discipline. When my friend Marx Wartofsky wrote an essay on my work, he said—out of affection, rather than as a criticism—"Toulmin is an odd duck"; and this description was wryly apt.[10] Long before entering the professional world of philosophy or social science, I was exposed to two influences whose effects were too powerful to ignore. On the one hand, I came to academic philosophy at a time when its arguments were unusually *ahistorical*. The most influential philosopher active at Cambridge in 1945 was Wittgenstein, and his only known comment on History is the solipsistic question, "What is History to me? Mine is the first and only World."[11] Like his colleagues C. D. Broad and R. B. Braithwaite, Wittgenstein's predecessor in the Chair of Philosophy, G. E. Moore, displayed a little more knowledge of his forerunners' views than Ludwig Wittgenstein himself, but he too gave no sign of believing that the soundness of philosophical arguments depended at all on the situation in which they were presented. On the contrary, Moore attacked John Stuart Mill's discussion of the relations between the "desirable" and the "desired" in a way that treated it as a matter of rival dictionary definitions, and completely ignored the role that Mill's *Utilitarianism* had played in nineteenth-century British social history.[12]

By contrast, I was born into a family where History was a matter for dinner table conversation. If my father had come of age after instead of before the First World War, he would himself have been an economic historian;

as it was, before I went to Cambridge, he introduced me to the varieties of History, from Arnold Toynbee's *Study of History* to J. L. Motley's *Rise of the Dutch Republic*. With this background, it was a relief, later on, to discover the books of R. G. Collingwood, who was a philosopher and historian at the same time. (Collingwood was the "odd duck" in 1930s Oxford.) Most significant of all was the fact that my family lived in the shadow of the Nobel Peace Laureate Norman Angell, with whom my father worked before the First World War, and we continued to see N. A. throughout the 1920s and 1930s. (If any book could have prevented the outbreak of the 1914–1918 War, it would have been Angell's *The Great Illusion*, published in 1910, which argued that such a war would leave all the great powers of Europe equally as losers.)

On the other hand, my own interests took me in the direction of theoretical physics, notably cosmology, rather than academic philosophy. As a teenager in the mid-1930s I would sit in bed reading books with titles like *The Restless Universe* or *The Infinite Universe:* the idea of a single theory that could grasp the whole World of Nature had for me a charm that was as much aesthetic as intellectual, and the question how to tell if any particular theory was "correct" did not for the time being strike me as urgent.[13] In the 1930s the idea of a *chaotic* Universe was not yet taken seriously. Physicists still took it for granted that the World of Nature operated "regularly"; and, as the Greeks supposed, the heavens formed a *cosmos*—well ordered or "cosmetic."[14] Cosmological speculation also appeared intellectually "pure" and unaffected by technological concerns or commitments. Theoretical physics in the 1930s was still—as the seventeenth-century founders of modern science had called it—"natural philosophy"; engineering and manufacture, in which scientific ideas were applied to human needs and problems, were seen as separate and largely inferior activities.

I was not alone in this intellectual snobbery. A sharp distinction between the pure and applied sciences was a feature of scientific culture right up to the Second World War. In January 1939, the Irish Marxist crystallographer John Desmond Bernal published his book *The Social Function of Science*, and his colleagues rejected it as politically radical: though Bernal's arguments quoted the works of Francis Bacon from the early seventeenth century, he was pilloried by Michael Polanyi and John Baker as an enemy of democracy, and they set up a Society for Freedom in Science to defend

phers can aim at an all-embracing "master narrative" about the nature of things. Now the eddies began. By 1992, Bruno Latour had denied that the episode of Modernity had ever depended on such an all-embracing narrative. "We were never really Modern," he declared: indeed, before the twentieth century we never really mounted any serious claim to being modern. Finally, the Danish writer Bent Flyvbjerg's *Rationality and Power* shows how, to this day, differences of "clout" affect the ability of competing arguments to carry weight in politically loaded situations.[18]

All in all, a skepticism that at first rested on doubts about the historical permanence of criteria of rationality widened to become—in effect—universal. From now on, permanent validity must be set aside as illusory, and our idea of rationality related to specific functions of the human reason. For students of rhetoric and argumentation, such skepticism toward the claim that rationality has a permanent validity is a commonplace. For philosophers in search of formal proofs, by contrast, this skepticism is catastrophic. For me personally, the outcome of forty years of philosophical critique was thus a new vision of—so to speak—the *rhetoric* of philosophy.

The rhetoric of philosophy? Reflecting on that phrase, I hesitate. The initial attack on my *Uses of Argument,* as an "anti-logic" book, assumed that Rhetoric and Logic were inescapably at odds. Logic is the formal demonstration of truths; Rhetoric is the deceptive peddling of falsehoods. Yet those years of critique were not without an effect. For many years, the University of Pittsburgh's Center for Philosophy of Science was the Vatican of the subject, protecting and preserving its formal principles against the mirages of its rivals. But in November 1992, the Pittsburgh Center organized a symposium on the relation of Reason to Rhetoric in the physical sciences themselves. After all, it turned out, my own position in *The Uses of Argument* still had merit, and the Cambridge University Press tells me—as I write—that, for all the objections from philosophers, the book remains in print, after a life of more than forty years.

Up to a point, then, Bruno Latour is right: the intellectual program of Modernity, with its assumptions about the universal and permanent character of Rationality, achieved full expression only in the twentieth century. Still, the current imbalance between our ideas of "rationality" and "reasonableness" sprang from seeds planted as early as the seventeenth century. Intellectually and institutionally alike, we can understand the current transition in our theoretical and practical lives only by taking such

a longer-term historical perspective. Then we can see to what extent the changes going on today are *undoing* things that were originally done in the 1630s and after, and represent a recovery of commitments that sixteenth-century humanists took for granted. Nor is this imbalance a feature of intellectual history alone, or of institutional history alone: any redressing of the imbalance requires us to correct both over-intellectualized ideas and over-bureaucratized institutions at the same time.

In some ways, this is already happening. Philosophy and social science are sharing the experience of music. Little now remains of the twelve-tone music of Berg and Webern, which seemed in the 1920s and 1930s to be laying down the road into the musical future. Only the "conservative revolutionary" Arnold Schoenberg went on arguing that twelve-tone music had all along been just another step on the highway marked out from Palestrina to Bach, and on to Haydn and Mozart, Beethoven and Brahms.[19] As in music, so in philosophy and the human sciences, the price of intellectualism has been too great, and we are now having to work our way back to broader modes of self-expression.

Seventeenth-century natural scientists (we shall see) dreamed of uniting the ideas of rationality, necessity, and certainty into a single mathematical package, and the effect of that dream was to inflict on Human Reason a wound that remained unhealed for three hundred years—a wound from which we are only recently beginning to recover. The chief task of this book is to show what is needed if we are to treat that injury, and reestablish the proper balance between Theory and Practice, Logic and Rhetoric, Rationality and Reasonableness.

goals of social policy, the factors responsible for successes or failures, the biological and physical causes of effects or phenomena, the striking features of an art object, the style or delivery of a speech, and a dozen other things. And, for more than two thousand years, all such activities were given equal consideration. No field of investigation or speculation was dismissed as intrinsically unphilosophical. A few, like astrology, might prove to be ineffective, but that was another matter.

From the mid-seventeenth century on, however, an imbalance began to develop. Certain methods of inquiry and subjects were seen as philosophically serious or "rational" in a way that others were not. As a result, authority came to attach particularly to scientific and technical inquiries that put those methods to use. Instead of a free-for-all of ideas and speculations—a competition for attention across all realms of inquiry—there was a hierarchy of prestige, so that investigations and activities were ordered with an eye to certain intellectual demands. Beside the *rationality* of astronomy and geometry, the *reasonableness* of narratives came to seem a soft-centered notion, lacking a solid basis in philosophical theory, let alone substantive scientific support. Issues of formal consistency and deductive proof thus came to have a special prestige, and achieved a kind of *certainty* that other kinds of opinions could never claim. So, as time went on, academic philosophers came to see literary authors like Michel de Montaigne—an essayist who had little use for "disciplines" and put equally little reliance on formal logic—as not being philosophers at all, let alone scientists.

It had not always been so. In mapping the reach of philosophy and human reason, the contrast between the reasonable and the rational is only one of half a dozen differences in our methods of inquiry. The contrast between the reasonableness of narratives and the rigor of formal proofs, between autobiography and geometry, is the contrast between the "soundness" of substantive *argumentation*, which has the body and force needed to carry conviction, and the "validity" of formal *arguments*, whose conclusions are determined by the starting points from which they are deduced. There is a parallel contrast between our local knowledge of the patterns we find in concrete events, and the universal, abstract understanding embodied in purely theoretical points of view. The substance of everyday experience refers always to a "where and when": a "here and now" or a "there and then." General theoretical abstractions, by contrast, claim to

apply *always and everywhere*, and so—as Tom Nagel points out—hold good *nowhere-in-particular*.

We need first to look more carefully at the contrast between *formal arguments* and *substantive argumentation*, and the relations between them, beginning with some samples of each kind. These must, if possible, be clear type examples, which can serve as templates in judging whether other examples are "purely formal" or "truly substantial"; if possible, too, they should be neither sophomorically simple nor excessively technical.

Consider, for a start, the eighteenth-century story of the Count and the Abbé:

> Two old ladies are receiving visitors, and the first to arrive is a bigwig, who happens to be a Count. The three of them discuss the Confessional, and the Count remarks, "Well, Mesdames, I can tell you this much—I was the Abbé's first penitent." He soon leaves, and the Abbé himself comes in. The conversation goes on and, under pressure, the Abbé clears his throat and says, "Without violating my duty of secrecy, Mesdames, let me simply tell you this: My first penitent was a murderer."

We have only to hear this story to jump to the conclusion: "The Count was a murderer"; and truly, if we take the two statements at face value—"The Count was the Abbé's first penitent" and "The Abbé's first penitent was a murderer"—they lead as they stand, by a *formal argument*, to the conclusion: "The Count was a murderer."

Yet the same story can be parsed, instead, as a piece of *substantive argumentation*. What guarantee have we that either the Count or the Abbé is telling the truth? The ladies are not likely to challenge them, so either or both of them may be grandstanding. Leaving open the possibility of such doubts, we may qualify our conclusion and say: "It looks as though the Count may, quite possibly, be a murderer." This change situates the formal argument in a human situation, so that it becomes a component in a substantive exchange of views. If we jump to a premature conclusion, we put both statements in a single mouth or mind, and the inference that the Count is *necessarily* a murderer overshoots the mark. Because the statements came from different mouths, the exchange has a different

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