

*The Idol of Stability*

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Twenty years ago Hans Küng, the German theologian, gave an evening lecture in the University of Chicago's Rockefeller Chapel: its title was *Science and the Problem of God*. Some of his hearers were surprised that a man of the cloth chose a title implying that the nature of God was problematic, while the nature of science was not. Might one not expect him to prefer, as his title, *God and the Problem of Science*? Yet a similar difficulty arises if we ask about the nature and standing of human values. On the one hand, the concept of *values* is in practice as little open to question among Europeans or Americans today as was that of *God* in Medieval Europe. We may disagree about cases; but we understand claims about the value of saving life, or building happy families, or respecting personal autonomy. All of these claims have clearly recognizable meanings. On the other hand, if we look at concepts and theories in the human sciences — whether concerned with individual behavior or with institutions and social relations — we find great weight placed on the need to confine ourselves to the *facts* and steer clear of *values*, which must (it is said) introduce damaging biases into our inquiries. Human scientists, as much as natural scientists, are exhorted to treat the difference between facts and values, not just as a *distinction*, but as a downright *separation*. How, then, can we do “factual” work in our scientific theorizing, while we continue to recognize “values” in all our practical activities and relations? That is the central issue for this Tanner Lecture.

For a start, I shall avoid debating the fact/value contrast as an issue in epistemology: we can return to this later. Instead, I shall examine the historical situation in which the founders of the human sciences became convinced of a need to frame their inquiries in “factual” and “value-free” terms. This will point us to the tasks we must tackle, if we are to reconcile the demands of thought and

practice —theoretical and practical life —and rescue our everyday understanding of human values from the critiques of the behavioral and social sciences. Where, then, are we to begin? As so often, one good starting point is to look at the things that “went without saying” during the Scientific Revolution. In his *Essay on Metaphysics*, R. G. Collingwood showed us, first and lucidly, that the thought of any place and age rests on certain beliefs that are so basic and familiar that the people of the time have no occasion to articulate them: assumptions that are so unquestioned that they may go without saying, Collingwood called these “absolute” presuppositions, in contrast to the “relative” ideas and hypotheses of everyday science. (This was before all Kuhnian “paradigms” or *mentalités*.) So let us start by asking what “absolute presuppositions” underlay the image of nature and science on which the founders of the human sciences relied. When they set out to build up these novel sciences, what questions did they *not* ask?<sup>1</sup>

My initial question, then, will be one that teases me whenever I leave the evolution of the natural sciences for the methodology of the human sciences: namely, “Why was the type example of serious Science —to be emulated by economists, sociologists and psychologists no less than by physiologists and biochemists —Newtonian Dynamics? Why were the first human scientists so determined to be the *Newtons* of social theory?” Surely, the activities of human beings are not like the motions of planets in their orbits, or rigid spheres rolling down inclined planes? Surely, they are far more like the behaviors of living creatures? So why did the initial creators of the human sciences not rely on models from biology in their theory-building, rather than on implausible analogies with physics? No work in the natural sciences had greater influence on the idea of “theory” in the social sciences than Isaac Newton’s *Principia* —at least as these are interpreted in our universities —yet no work (I shall argue here) has been more deeply misunderstood.

<sup>1</sup> R. G. Collingwood, *An Essay on Metaphysics* (Oxford, 1940), esp. chaps. 4 and 5.

## 2

Newtonian physics came to be seen as a model for the truly “hard” sciences, not least, because of its supposed success as an instrument of prediction and control. Yet those who hold dynamics up as an example to the human sciences in this respect have (I shall claim) failed to study carefully enough the conditions on which it played this role *even in physics*. Pierre-Simon de Laplace dreamed of an Omnipotent Calculator who, given the positions and velocities of every atom in the universe at Creation, could use Newton’s equations to compute the entire subsequent history of physical nature. This (as I shall argue here) was never more than a fantasy *even at the outset*; and, if we ask how this fantasy reflected the original claims for theoretical physics, we shall find that the human sciences —not least theoretical economics —based their research programs, not on realistic ideas about the *actual* methods of Physics, but on their vision of a physics that never was.

So much for my agenda: How can I back up these claims? I shall focus on the aspects of Newtonian dynamics that led Gottfried Wilhelm Leibniz to dismiss Newton’s *Principia* as metaphysically impossible: in particular, the puzzle mathematicians have called the *Three-Body Problem*. This is the subject of a monograph published by Henri Poincaré in 1889, deploying all the resources of nineteenth-century mathematics to resolve this problem without success. Now, a century later, Poincaré’s monograph — instead of rescuing the solar system from the threat of instability — serves, instead, as a starting point for today’s chaos theory.

Notice my phrase “rescuing the solar system from the threat of instability”: it brings to the surface an assumption that “went without saying” from the early seventeenth century up to the first years of the twentieth century. From Hugo Grotius and René Descartes until the First World War, the ideals of intellectual order and rational intelligibility current among European intellectuals emphasized *regularity*, *uniformity*, and above all *stability*. From this standpoint, the merit of Newton’s *Principia* was to show that the

solar system of which the earth is a member is a “demonstration” —a *paradeigma*, in the Classical Greek —of an *intrinsically stable* system. This assumed success for Newton’s theory convinced the “Mathematical and Experimental Natural Philosophers” (the theoretical physicists of the seventeenth century who took a lead from Galileo Galilei, Johannes Kepler, and Descartes) that their use of Euclid’s *Elements of Geometry* as a model for a new physics —or, for Thomas Hobbes, a political theory —was not a dream born of Platonist epistemology alone, but a realistic program for scientific research.

In the 1630s, Descartes’s *Discourse* set out philosophical reasons for seeing Euclid’s *Geometry* as an intellectual model for theories in other areas of inquiry. Fifty years later, Newton showed that this model was not just formally rigorous, but empirically powerful: i.e., it resolved problems that had plagued European thinkers ever since the publication of Nicolaus Copernicus’s *de Revolutionibus* (1543). If this could be done in astronomy, they asked, was the same not possible in other fields, too? Once taken up, this challenge engaged the intellectual imaginations of talented mathematicians and scholars for more than 200 years. This was the change that most recommended the Newtonian model of a “hard science” to the intellectuals of Europe, and its fiercest opponent was Newton’s old enemy, Leibniz.

What exactly, then, “went without saying” in planetary theory in Newton’s last years? And what continued to go without saying in philosophy and social science for much longer, even until after the First World War? At the heart of an answer lay this belief that the solar system is the prime example of a “rationally intelligible” system in nature; but we must take great care just how we state this belief. It was not just the *geometrical* move from the Ptolemaic to the Copernican picture of the solar system that was at issue: rather, it was a deeper assumption, about the *dynamic stability* of the whole system.

As Newton declared in a Scholium to the second edition of the *Principia*, the Stability of the Sun and Planets shows that the World of Nature displays the Creator's Rationality. This belief was consistent with any geometrical or dynamical account of the solar system. Tycho Brahe took the *stability* of the solar system for granted as surely as Copernicus or Galileo did; nor did it matter whether Descartes's "vortices" or Newton's "gravitation" were used to explain its operation; finally, it was compatible with all current ideas about *how far* and *in just what respects* the system was "rationally intelligible." At this point, then, we must make a more careful study of the things in Newton's theory that Leibniz found offensive. For here begins a bifurcation in physics and philosophy that shows up for 150 years in the budding human sciences, too —not least, in economic theory.

The central issue dividing Leibniz and Newton was the question "How is the Rationality of the Creation —more specifically, the Rational Design of the Solar System —to be *demonstrated*?" Newton was content to explain the regularities Kepler had found in the planetary orbits *empirically*, by appeal to his inverse-square Law of Universal Gravitation: in this way —he argued —they displayed just the kind of mathematical pattern a rational Creator might be expected to prefer. Leibniz, by contrast, was not content with an empirical demonstration: he would not accept anything but a *formal proof* that the planetary system *must* display the regularities we do in fact observe. By that measure —Leibniz argued —Newton had failed. And when Samuel Clarke, as Newton's amanuensis, replied, "Evidently, this is how God *chose* to create it,"<sup>2</sup> that only sharpened the antagonism. To show what God *in fact* chose was not enough: you must also show that it was *right and just* for God to have chosen as He did. For Leibniz, Newton's theory was incomplete, because it included no Theodicy: no demonstration that the way in fact things *are* in God's World is also *for*

<sup>2</sup> Cf. *The Leibniz-Clarke Correspondence*, ed. H. G. Alexander (Manchester, 1956).

*the best*. So, Leibniz provided the model for Dr. Pangloss in Voltaire's *Candide*, who could continue to argue that "Everything Is for the Best in the Best of All Possible Worlds," even after the catastrophic Lisbon Earthquake of 1755.

When it came to a formal proof of the Divine Rationality, the Three-Body Problem was (to Leibniz) a fatal blow to Newton's theory. The theorems in dynamics that Newton relied on to explain the elliptical forms of the planets' orbits, or their speeds of motion round the orbits — both established empirically by Kepler — were highly *oversimplified*: they showed only that the Law of Gravitation fitted the motions of one small planet at a time around a much more massive center of gravitational attraction, viz. the sun. On this simplification, the equations of motion for a single planet were easily solved, and we could obtain general solutions having the same forms as Kepler's empirical laws. But as soon as we introduced any third body into the case (e.g., a second planet), the equations were no longer soluble in algebraic terms: the best a Newtonian can do is to calculate the third body's influence from moment to moment, arithmetically, as a "perturbation" of the simplified orbit.

How can one react to this discovery? In practical terms, it may be enough to improve our methods of computation bit by bit, so that the numerical match between the results and the planetary motions recorded by astronomers is increasingly exact: that was the agenda for the eighteenth century, progressively cutting down the "perturbations" in ways that led up to Laplace's *Système du monde*. Laplace was content to show that these perturbations were on a scale explicable by refined Newtonian calculations; so he rejected Newton's hint that God might intervene in the system from time to time, so as to remove all irregularities and so restore the stability of the system ("Je n'avais pas besoin de cette hypothèse").<sup>3</sup> But Leibniz could not take this step: for him, an acceptable theory *must* yield general algebraic solutions for any set of bodies whatever,

<sup>3</sup> See Leibniz's first letter in the *Correspondence* with Clarke.

however multiple or complex — not merely two at a time. Once he recognized that Newton's *Principia* did not provide such solutions, he set it aside as metaphysically inadequate; and it is hard to find evidence in his writings that he read more than the first thirty or so pages of the *Principia*.

From the death of Leibniz in 1715 to the last twenty years of the nineteenth century, then, we find a basic division in the philosophy of physics. On the one hand, starting with Leibniz himself and continuing *via* Leonhard Euler (and Laplace in metaphysical moments) to Pierre Duhem, there is a Continental tradition of *rationalism*: on the other hand, starting with Newton and continuing *via* John Dalton and William Herschel (and Laplace in practical moments) up to James Maxwell and Ernest Rutherford there is a British tradition of *empiricism*. Empiricists saw *any* regularities in the observed phenomena as evidence of God's Rational Order: rationalists still looked for comprehensive mathematical theories, having the full rigor of Euclid's *Elements*.<sup>4</sup> Only Immanuel Kant —keeping to the sidelines in natural philosophy as he did in epistemology and metaphysics —took neither side in the dispute. In the *Allgemeine Naturgeschichte und Theorie des Himmels* (1755), his intellectual imagination reached out beyond the previous limits of the Newtonian system, to hint at a cosmology with an evolutionary history barely glimpsed by his predecessors.

Working physicists —especially in Britain —took the empiricist line: it was enough to balance the books by fitting computations and observations. After 1810, the Three-Body Problem faded into the background and was seen as a metaphysical rather than a scientific issue. By the 1870s, however, the tide had turned. From Charles Lyell's *Principles of Geology* on, the history of the earth was a scientific preoccupation: the debate triggered by Charles Darwin's *Origin of Species* made a historical reinterpretation of

<sup>4</sup> Cf. Pierre Duhem, *La théorie physique: Son but, sa structure* (Paris, 1903), Eng. trans. by P. P. Wiener (Princeton, 1954), chap. 4, "Abstract Theories and Mechanical Models."

the order of nature only the more urgent. The scale of the universe —in both space and time —proved far vaster than was earlier assumed and provoked the anxiety exemplified in Alfred, Lord Tennyson's widely read poem *In Memoriam*. Soon, the debate about the history of nature became the focus of religious and intellectual discussion, and it is the background against which we can look at Poincaré's monograph "Sur le problème des trois corps et les équations de la dynamique."<sup>5</sup>

The circumstances of this monograph are themselves of some interest. It appeared as a special supplement to *Acta Mathematica*, one of the leading journals of pure mathematics. From 1882 on, this was edited by the Swedish mathematician Göran Mittag-Loeffler of Uppsala, with an editorial board that included two of the finest mathematicians in Europe, Karl Wilhelm Theodor Weierstrass from Germany and Charles Hermite from France. (Poincaré was one of Hermite's most talented pupils.) From the start, *Acta Mathematica* concentrated on "pure mathematics," as that subject was then understood. Georg Cantor, Heinrich Hertz, and David Hilbert all published in it, and in 1885 King Oscar II of Sweden lent his name to a competition for the best essay on a subject in pure mathematics.

In announcing the competition, Weierstrass, Hermite, and Mittag-Loeffler chose four areas for special attention. Three of the problem areas lay in theory of functions or other subjects still recognized today as belonging to "pure" mathematics; but the first topic was *la stabilité de notre système planétaire* (the stability of "our" planetary system). For a question in pure mathematics, this problem was framed in oddly singular terms. It was not stated as having to do with general "stability conditions" for *any* planetary system: instead, it referred precisely to *our particular* planetary system. Shortly before his death in 1859 — it was said — Gustav L. Dirichlet claimed to found a proof of this stability; but he never explained it, and entrants to the competition were invited to recon-

<sup>5</sup> *Acta Mathematica*, vol. 13 (1889), pp. 1–270.

struct his feat. They were to send entries to Sweden, marked with an epigraph, their names given only in sealed envelopes bearing the same epigraph, to ensure that the judging was anonymous. Twelve entries arrived, and five of these tackled the stability problem. Two prizes were awarded, one for a monograph bearing the epigraph *Nunquam praescriptos transibunt sidera fines*: “Never will heavenly bodies transgress their prescribed bounds.” This was Poincaré’s meticulous reanalysis of the Three-Body Problem, set out in 23 chapters and 270 pages.<sup>6</sup>

Poincaré’s epigraph recalls the antiquity of the belief in celestial stability; but the choice was also ironical, since the question “Do planetary motions have any ‘prescribed limits’? Can one *prove* that the planetary system must in fact be stable?” was just the point at issue. By the time one reached the last page of Poincaré’s monograph, there was clearly no more hope in 1889 than in 1715 of finding general methods of solving the equations of motion for two or more planets moving round the sun at the same time. Another result was even more damaging for philosophical debate: by the end of Poincaré’s analysis it appeared that, when numerous objects move freely under mutual gravitational attraction, critical collisions (*chocs*) may take place whose outcomes are radically unpredictable. Instead of Laplace’s dream of a world whose history was computable in Newtonian terms, a new picture began to emerge of a world in which—aside from the artificial case of the sun and a single planet—complete predictability was out of the question.

The world of physical determinism that was a nightmare for nineteenth-century thinkers thus gave way to the world we know today as the world of chaos. Far from proving that “our” planetary system is dynamically stable, Poincaré ended by laying a basis for chaos theory. True, he did not at once appreciate the full effects of his work. In the 1880s, his painful analysis of the Three-Body Problem only ended by reinforcing the difficulty and gave us no

<sup>6</sup> See n. 5, above.

new way of solving the equations of motion for three bodies or more: without some radically new kind of mathematics (e.g., our “nonlinear” mathematics) there seemed no prospect of overcoming the difficulty. In the 1890s, Poincaré developed a three-volume book on *New Methods in Celestial Mechanics*, with different conclusions from Laplace’s. Only in his philosophical essays at the turn of the century did he open up clearly the issues of chaos and complexity that preoccupy physical scientists today.<sup>7</sup>

Poincaré’s interest in the Three-Body Problem was never purely personal. Questions in many-body dynamics still fascinated mathematicians, from Sweden to Italy, Germany to North America: these questions crop up in *Acta Mathematica* throughout the 1880s and 1890s, up to 1906. Nor was this a purely technical issue for mathematicians: the year 1906 saw publication of H. G. Wells’s novel *In the Days of the Comet*, in which the earth faces annihilation by a massive comet, and humans are moved to reorder their affairs. Even in the 1990s, the dynamics of our planetary system are a matter of public concern. In 1994, there occurred “by far the most spectacular event in the Solar System ever witnessed by the human race”: a collision with the planet Jupiter of fragments of Comet Shoemaker-Levy 9. In mid-March 1998, again, astronomers foretold, for October 26, 2028, the possible impact of an asteroid on the earth violent enough to be a catastrophe for the human species, as an earlier one apparently destroyed the dinosaurs.

Far from the Euclidean model being the standard pattern for any “hard” science, then, physics itself never fully exemplified that form. Leibniz had been right: the Three-Body Problem raised insuperable difficulties for any strict reading of Newtonian theory, if not for a purely pragmatic reading of his theories. From the outset, a strong case might have been made for radical unpredictability and complexity, chaos theory and nonlinear mathematics: failing that, the model that for so long held center stage as the

<sup>7</sup> Notably *La science et l’hypothèse* (Paris, 1902) and *Science et méthode* (Paris, 1908).

ideal form of theory in any would-be science was the model of a physics that never was.

## 3

What did this episode in the history of physics have to do with the actual development of the human sciences? Was the goal of eighteenth-century social theorists to be the Newton of the human sciences any more than agreeable rhetoric? In order to recognize the influence of this assumption about the stability of the planetary system on human thought and practice, we must look more closely at the people who laid the foundations of the social sciences. For this purpose, let us focus directly on those who tried hardest to model their work on mathematical physics: the creators of mathematical economics. Of all human scientists, the ones most confident of the rigor of their methods and the superiority of their results are the economists who develop abstract, universal mathematical systems. The formality of their arguments carries an air of theoretical rigor; the generality of their concepts gives them the appearance of practical universality; and, as a result, the ideas of “neo-classical equilibrium analysis” have a special prestige among academic economists.

There are two ways to write a history of economic theory. We can start where we are now and look back at earlier writers who already used mathematical methods of analysis like those used by academic economists today: in this way, we can establish an honor roll of the *precursors* of modern economics. (This is a recipe for surprise and disappointment: surprise at the foresight of a few imaginative individuals, disappointment that their example took so long to be followed up.) Or we can begin at the beginning: asking what personal projects these creative individuals were engaged in and how their mathematical excursions into economics contributed to those projects. Depending which of the two roads we take, we end with a different story about the birth of economic theory. On the first, the creation of economic theory was delayed

by the failure of readers to pursue the lines of argument of their creative precursors: on the second, theoretical economics — as we know it today — is a product of conceptual abstractions that became available only during the twentieth century, and the work of the “precursors” reflects quite different intellectual ideas and ambitions.

Until recently, most historians of economic theory chose the first road. For instance, the classic *History of Economic Analysis* assembled after his death from the *Nachlass* of J. A. Schumpeter took as heroes Adam Smith (1723–90) and Antoine Augustin Cournot (1801–77), William Stanley Jevons (1835–82) and Léon Walras (1834–1910). All of these writers were concerned in their own ways with connections between economics and physics: taking them in turn, we find close parallels between their ideas about *equilibrium* in economics and Newtonian ideas about the dynamics of the planetary system,

All biographers of Adam Smith remark on the unusual scope of his writings: from the uses of rhetoric to the theory of the moral sentiments, from the wealth of nations to the history of astronomy. His intellectual versatility they put down, in part, to the range of academic discussion and education in the eighteenth-century Scottish Enlightenment, in part, to the variety of his own interests and the width of reading he had accumulated in the course of his bachelor life. Yet it is evident that, for many years, his own personal project was to develop an overall vision of the universe — we might even call it a “cosmology” — of which he fully completed only this history of astronomy, and he abandoned this ambition only when he saw that it was too vast to finish in his lifetime. The essay on astronomy stands as testimony to his ideas about the proper method for any intellectual system; but he never pursued the parallels between economics and physics into more substantial fields.

In Britain, then, we find an empirical tradition in economics — linking Adam Smith and David Ricardo, by way of James Mill and John Stuart Mill, to Alfred Marshall — that parallels the empiri-

cist tradition in physics, from Newton to Maxwell and Rutherford. In Continental Europe, by contrast, we find in the precursors of economic theory a rationalist tradition like that in natural philosophers from Leibniz to Duhem. At times, this analogy was stronger. Augustin Cournot's use of mathematics in his *Recherches sur les principes mathématiques de la théorie des richesses* (1838) was somewhat elementary, and for most of his career he set aside economics for broader issues in cosmology and epistemology: to judge the place of economics in his personal enterprise, we may look at the *Traite de l'enchaînement des idées fondamentales dans les sciences et dans l'histoire* (1861). Of the 707 pages of this, his major work, only 28 are devoted to topics in economics.

The rationalist thrust of Cournot's cosmology is clear in his own intellectual evolution, as recorded in his *Souvenirs*. He came from a conservative royalist family and did not at first go to the university, but spent four "largely wasted" years in a law office. However, he read widely, and four authors specially caught his imagination: Bernard de Fontenelle's *Eloges* and *Pluralité des mondes*, Laplace's *Système du monde*, the Port-Royal *Logic*, and — of all things — the *Leibniz-Clarke Correspondence*. This last work had defined the rival, empiricist and rationalist, methodologies for physics, and Cournot's loyalties are clear. In his *Considérations sur la marche des idées* (1872), he comments that the true successors to Newton were not British empiricists, but Continental mathematicians like the Bernouillis, Euler and Johann Heinrich Lambert, Alexis-Claude Clairaut and Jean d'Alembert. The final pages of his *Revue sommaire des doctrines économiques* (1877) are pure epistemology: they discuss the proper method for any theory of human transactions and compare this to the theories of planetary astronomy. In both fields — he concludes — we must distinguish the general laws that define the *essential* form of the phenomena involved from "perturbations" arising from the *accidental* influence of other intervening bodies or agents. At the very end of Cournot's life, the power of the astronomical model was thus undiminished.

Jevons, too, is given a historical role among the precursors of economic theory that too easily exaggerates the centrality of economics to his thinking. Invited to speak to the British Association for the Advancement of Science in 1862, he prepared a “brief account of a general mathematical theory of political economy,” followed by a book on *The Theory of Political Economy* (1871); but most of his papers on economics were edited only after his death.<sup>8</sup> Taken in isolation, these works may give us the impression of a writer for whom economics was a topic of central importance; yet Jevons’s total *oeuvre* belies this. Increasingly, his concern was with the power of logic as an instrument in scientific theory of any kind; so, when he brought his general ideas together for publication in his 600-page *Principles of Science*, he did not give one page to economics, and the word “economics” is not in the index. His excursions into mathematical economics seem, as much as anything, to be making a methodological point.<sup>9</sup>

The most revealing case is that of Léon Walras, a French academic who (to his regret) spent his career at Lausanne in Switzerland, where his colleagues included Vilfredo Pareto. Walras was a more single-minded economist than Cournot or Jevons; yet, like them, he is preoccupied with *method*, notably with analogies between “equilibrium” in planetary theory and economic affairs. During his last ten years, he kept writing to Poincaré, hoping to win the great mathematician’s approval for a parallel he thought he had established, between the laws of economic equilibrium and those that supposedly ensured the stability of the planets: Walras’s last paper, in fact, was entitled *Economique et mécanique*. By this time, Poincaré himself, of course, no longer believed that the planetary orbits had any essential stability —let alone that Newtonian

<sup>8</sup> *Journal of the Royal Statistical Society* (London), 29 (June 1866): 282–87. For a more detailed and sympathetic view of Jevons as an economist as well as logician, see Margaret Schabas, *A World Ruled by Number: William Stanley Jevons and the Rise of Mathematical Economics* (Princeton, 1990).

<sup>9</sup> *The Principles of Science: A Treatise on Logic and Scientific Method* (New York, 1892, repr. with a preface by Ernest Nagel, New York, 1958).

dynamics give a mechanical guarantee of that stability — so it was embarrassing for Poincaré to answer Walras's pressing letters candidly, and the letter that is printed as an annex to Walras's paper reads, in retrospect, more like a diplomatic brushoff than an endorsement.<sup>10</sup>

Even after dreams of equilibrium and stability faded within physics itself, then, they remained alive in economics; and to this day many economists' ideal of a theory still rests on parallels with Newton's *Principia*. As attention in physics itself shifted to the relativity and quantum theory, however, the debate in economics began to change its tone. In his *History*, for instance, Joseph Schumpeter captured this shift in a footnote on the work of the founder of the Cambridge school of economics, Alfred Marshall: "The truth that economic theory is nothing but an engine of analysis was little understood all along, and the theorists themselves, then as now, obscured it by dilettantic excursions into the realm of practical questions. But it was emphasized by Marshall who, in his inaugural lecture at Cambridge [1885], coined the famous phrase that economic theory is not universal truth, but 'machinery of universal application in the discovery of a certain class of truths.'" <sup>11</sup> Two phrases shine out from this comment — Schumpeter's judgment on economists who apply theories to practical issues as "dilettantic"; and Marshall's argument that economic analysis may still have "universal application," even if it has abandoned all pretensions to "universal truth." For Marshall, concepts like "equilibrium" thus remained of universal *relevance*, even if we stopped reading them as accounts of *reality*.

<sup>10</sup> Walras's paper is in the *Bulletin de la Société Vaudoise des Sciences Naturelles (5e série)*, 45, no. 166 (June 1909): 1–15; for Poincaré's letter, see pp. 14–15. As to the other correspondence between the two men, cf. *Correspondence of Léon Walras and Related Papers*, vol. 3 (1898–1909), ed. W. Jaffé, esp. letters 1492 and 1495.

<sup>11</sup> Joseph A. Schumpeter, *History of Economic Analysis* (New York, 1954), part IV, chap. 7, p. 954, n. 2.

## 4

We live today in a time when public life is dominated by applications of economics to “the realm of the practical”; and we need to ask both how these applications can escape Schumpeter’s charge of dilettantism and how far Marshall’s claims of “universality” still hold good. My aim is to question the “universal” relevance of neoclassical theory. So let me here change gear and report two practical examples. In the first, Marshall’s assurance of the “universal applicability” of abstract economic theory had disastrous consequences: in the second, admirable results flowed from abandoning that assurance. In practical cases, then, the distortions introduced by this assumption may be more serious than is commonly admitted, and we can escape them only if we “de-universalize” the application of economic analysis to practical problems —treating these as specific aspects of the actual social, cultural, and historical situations in which they arise.

My first vignette combines several of the difficulties that afflict contemporary economic analysis.<sup>12</sup>

*I have an anthropologist friend, with a Dutch wife, who does field work on Bali. His research has been on the system of “water temples” whose priests —by tradition —controlled the schedule by which irrigation water was shared between the rice fields of different farmers or communes. For 800 years, these temples were a central feature of Balinese society; yet the central government of Indonesia —the former Dutch colonial administration as much as the Indonesian government today —treated the water temples as “cultural monuments” and never saw them as having any economic significance.*

<sup>12</sup> J. Stephen Lansing, *Priests and Programmers: Technologies of Power in the Engineered Landscape of Bali* (Princeton, N.J., 1991), pp. 113–15. See also the report by Lucas Horst (Wageningen Agricultural University), “Intervention in Irrigation Water Division in Bali, Indonesia.” Dr. Horst gives a very fair picture of the mistakes made in the Bali Irrigation Project. Notice Horst’s comment, “The Italian and Korean consultants had no or little knowledge of the specific Bali-Subak irrigation” —even describing the traditional procedures as making an *arbitrary* allocation of water to the farmers!

*In the late 1960s and early 1970s the Indonesian national government decided to introduce to Bali, on a massive scale, the new strains of “miracle rice” developed at the International Rice Research Institute in the Philippines. So — my friend points out — “Balinese farmers were forbidden to plant native varieties . . . Instead, double-cropping or triple-cropping of IR-36 [or similar varieties] was legally mandated. Farmers were instructed to abandon the traditional cropping patterns and to plant high-yielding varieties as often as possible.” With this policy went an engineering project launched by the Asian Development Bank in 1979, based on a report from consultants in Milan, Italy, and Seoul, South Korea. From a purely technical and economic point of view, this project was a strictly rational recipe to increase rice production and help make Indonesia self-sufficient in rice, which was the central aim of the government policy.*

*What happened? For two or three years the policy succeeded as forecast: the rice crop soared and farmers put money in the bank. But, in the 1980s, the local authorities recorded “explosions” of insect pests and infestations of funguses, both old and new. Soon, all the biblical plagues of Egypt were afflicting the farmers of Bali: “By the mid-1980s, Balinese farmers had become locked into a struggle to stay one step ahead of the next rice pest by planting the latest resistant variety of Green Revolution rice. Despite the cash profits from the new rice, many farmers were pressing for a return to irrigation scheduling by the water temples, to bring down the pest populations. Foreign consultants at the Irrigation Project interpreted any proposal to return control of irrigation to the water temples as a sign of religious conservatism and resistance to change. The answer to pests was pesticide, not the prayers of priests. Or as one frustrated American irrigation engineer said, ‘These people don’t need a high priest, they need a hydrologist!’ ” Until matters reached crisis point, economists at the Asian Development Bank found it hard to admit that the traditional irrigation schedules operated by the water temples were*

*functional: what “worked” was not prayers of priests, but the centuries of experience embodied in the schedules. As they saw it, as “religious” institutions, temples must be economically irrelevant; so this was a hard lesson to learn.*

Don’t misunderstand me. I tell this story not out of a dislike for technology —I am not a machine breaker —but to illustrate another point. We too easily assume that economic and technical issues can be *abstracted from* a situation: that economists and engineers can know in advance what things are or are not economically or technically relevant to our decisions. If engineering and economics are *scientific* (we assume) their principles *must be* universal; so that the theoretical view from Milan, Italy, or Seoul, South Korea, may be not less but more clear-sighted than the view on the ground, in Bali itself.

The decision, prompted by the Asian Development Bank, to replace traditional planting and irrigation schedules by uncoordinated multiple cropping had the effect of destroying, at a stroke, both the *material* infrastructure of Balinese *culture* —waterways and practices developed through the history of the island, to minimize the exposure of crops to insects, diseases, drought, flood, and other natural enemies —and the *moral* infrastructure of local *society* —the institutions that embodied the people’s respect for the traditional procedures. At the same time that the crops were blasted, the loyalties of the people were undermined.

My other example shows how economic analysis, applied more perceptively, can have equally constructive results. Let us leave Bali for Bangladesh.<sup>13</sup>

*The key figure in this story is a young graduate student called Muhammad Yunus, who took a Ph.D. in economics at Vanderbilt University, in Nashville, Tennessee. There he was taught the economic principles of banking and finance, in a form that supposedly*

<sup>13</sup> The work of Dr. Yunus and the Grameen Bank has recently been widely discussed in the *Economist* and elsewhere, notably in connection with the Microcredit Summit at Washington, D.C., in February 1997.

*applied in the same way in all countries. Returning home to Chittagong, he ran into difficulties. In class he handed on the "laws of the market" as he had been taught at Vanderbilt; but every day after class he walked home via the local market, and found it hard to square the transactions going on there with the theoretical principles he had just been teaching.*

*Stopping at a stall where a poor woman made sandals, he asked how she ran her business. She bought raw material for the sandals (she said) from a moneylender, who lent her the cost of the materials and took her output at a price he himself set. Having nothing to offer as "collateral" for the initial loan —no house or car, nor even a sewing machine —she could not build up a surplus, but was trapped in dependence on the moneylender.*

*Young Muhammad Yunus gave her a very small loan —\$15, I think it was —so that she could sell sandals to the public at her own price, and at a profit. Then he went home and asked how the concept of collateral might be extended, to cover productive loans to the poorest of the poor. Instead of material collateral (he decided) one might experiment with a kind of "social" collateral, by which a group of individuals together ensured repayment of a small loan to their poorest member, on the understanding that this would qualify the other members for loans, in turn.*

*Three years later, Yunus started his own Grameen Bank, which now operates in 30,000 Bangladeshi villages, making loan to local groups, chiefly of women, who keep up a repayment rate of 97 or 98 percent —any commercial bank would of course be very happy with such a rate —and, by now, similar "microcredit" schemes are to be found in more than fifty countries across the globe —even in the United States itself.*

To repeat: I am not attacking economics: I argue only that, in real life, economic analysis yields just or fruitful human outcomes *only* if economists take into account all the relevant social, cultural, and historical features of a human situation. Muhammad Yunus understood the culture of his homeland well enough to see that it

is no good equating a local market in Chittagong with the idealized “market” of economic theory. The lack of material collateral of kinds familiar in mature economies was not a reason to penalize the poor even further: rather, it was a call to extend the application of the term “collateral” to fit the local culture and society better. Economic theories of universalistic kinds had too often led economists to overlook social, cultural, or historical factors that seemed to them “noneconomic”; so the new kind of “social collateral” had to be recognized, if you were to match the theories of banking and finance to the actual situation on the ground.

A briefer vignette underlines this point. It has to do with an anthropologist from San Francisco who went to work in Japan.

*Why did he go to Japan, and what did he do there? He went because one of the top Japanese auto makers wanted to break into the California market and invited him to run their strategic planning unit.*

That’s the whole story. Someone at the headquarters recognized that all *economic* problems are, in practice, *cultural and social* problems, too, and that strategic planning that fails to take this fact into account is likely to prove shortsighted and unproductive.

I speak here about economics, but I might equally well have chosen other disciplines. A Japanese colleague of mine in civil engineering, Yoichi Arai, supervised construction of an artificial island in Osaka Bay for Kansai International Airport, to serve central Japan. He was impressed by the range of questions arising during construction that he could not answer by straightforward technical calculations: even questions about the effects of the new island on the fish population of the bay. So now he argues for a radical revision of the syllabus for educating engineers and technologists. What he calls for is a “humanized” technology, in which mathematical methods are taught always with an eye to their practical application in particular human situations.<sup>14</sup>

It is a far cry from the time when the US. Army Corps of Engineers could set in train construction of large-scale dams or

<sup>14</sup> See, e.g., the introduction to Y. Arai, *The World Airports* (Tokyo, 1996).

canals, without considering the interests of the people in the valleys inundated by their work. It is a far cry, too, from the time when nuclear power stations were built, without even a public inquiry into the effects of their construction on the neighborhood. Nor is it only the Japanese who react in this way: similar discussions about the education of engineers are going on, to my personal knowledge, in Sweden and the Netherlands. All over the world, the political debates about the human consequences and environmental impact of large-scale engineering works are, thus, reflecting back on the discussion of technical disciplines.

If real life problems in economics and engineering are historical, cultural, and social problems, too, the same is true of human problems more generally. None of the issues that affect the practical interests of human beings can be fully resolved in abstract, theoretical terms alone. This is not to question the intellectual value of well-established theories, or to deny their practical fruitfulness as applied to the human needs evident in real-life situations. It is only to comment on a tendency in Western thought, to focus on the core concepts or techniques of one single, abstractly defined discipline at a time, while failing to consider in concretely described terms the human effects of putting those same concepts and techniques to work in particular practical cases.

If we renounce that tendency, one immediate outcome is to challenge those dreams of universality and timeliness — what I called the *Idol of Stability* — that played a central part in the history of the human sciences in general, particularly of social and economic theory. We live in a world of flux as much as fixity, specificity as much as generality, particularity as much as universality. Nothing in human affairs is in total flux, let alone in total chaos. There are general similarities to be explored among human societies and organizations, as well as among the thoughts and feelings of different individuals. But, if we assume from the outset that all these things are governed by universal timeless laws, we lay

up trouble for ourselves; and we do better to set the dreams of stability and equilibrium aside.<sup>15</sup>

## 5

At this point my road is at a fork, and there are two alternative ways ahead. On the one hand, we can stay in the world of theory and reformulate our ideas for the human sciences in subtler, less simplified terms, paralleling the newer physics that was made possible by twentieth-century critiques of Newtonian ideas. On the other hand, we can question the primacy claimed for theory in the modern era and reconsider the merits of a practical (even clinical) view of these subjects. May it not be better to view the social sciences as concerned, not with “value-free” facts, but *precisely* with human values and practices: with coming to understand how human lives go *well or badly, better or worse*, and how we can help them to fulfil their potential? That, of course, will mean turning our backs not just on the Idol of Stability but on the “fact/value dichotomy” as well.

To begin with, notice how the grip of equilibrium analysis is starting to loosen, even in economic theory itself. Analysing “increasing returns and path dependence” in economics, for instance, W. Brian Arthur quotes Schumpeter’s *History*:<sup>16</sup> “Multiple equilibria are not necessarily useless, but from the standpoint of *any* exact science the existence of a uniquely determined equilibrium is,

<sup>15</sup> For detailed studies demonstrating the need to treat equilibrium theories with caution, see such recent books as *Range Ecology at Disequilibrium*, ed. R. H. Behnke, Jr., Ian Scoones, and Carol Kerven (London, 1993), and *Sustaining the Soil*, ed. Chris Reij, Ian Scoones, and Camilla Toulmin (London, 1996).

<sup>16</sup> W. Brian Arthur, *Increasing Returns and Path Dependence in the Economy* (Ann Arbor, 1994), p. 4. Arthur writes of this passage as having been *written* by Schumpeter in 1954, but by then Schumpeter had been dead for four years. The words quoted must in fact have been written half a dozen years earlier. Note also a comment Arthur quotes from J. R. Hicks about the danger of taking increasing returns seriously: “The threatened wreckage is that of the greater part of economic theory.” On the use of equilibrium theories in economics, see Bruna Ingraio and Giorgio Israel, *The Invisible Hand: Economic Equilibrium in the History of Science* (Cambridge, Mass., 1990).

of course, of the utmost importance . . . without any possibility of proving the existence of [a] uniquely determined equilibrium — or at all events, of a small number of possible equilibria — . . . a field of phenomena is really a chaos that is not under analytical control.”<sup>17</sup> Writing in the late 1940s, Schumpeter was not (of course) using the term “chaos” in the new, “chaos theory” sense. But the idea of *equilibria* was still in his view indispensable, if any economic theory was to have analytical control over its subject matter. So understood, his ideas about the nature of any “exact science” retain a *universality* familiar from the writings of the Vienna Circle philosophers.

By contrast, Brian Arthur argues that economists must look carefully at the historical situation in which any economic fact occurs, since it may make that fact an exception to the hitherto universal rules. Familiar examples are the commercial success of the VHS video system, despite the technical superiority of the Betamax system; the success of gasoline-powered automobiles, despite the absence of pollution from steam-powered cars; and the general adoption of the *qwertyuiop* keyboard in typewriters. In each case, the success of an inferior product was “locked in” because it won its market position before there was any direct competition with its rivals.

Arthur’s work extends the reach of economics, but stays clearly on the side of theory rather than practice. If he writes about cars, videotapes, and typewriter keyboards, it is to explain the general phenomenon of historical lock-in, not to promote the less successful rival products as objects of practical concern. It is as though Muhammad Yunus thought up “social collateral” simply to improve the economic theory of banking and finance, not to help the poorest of the Bangladeshi poor. That would certainly have been an advance in economic theory; but Yunus’s later pursuit of other ways to tackle poverty and destitution in his home country showed that his core concern was practice, rather than theory.

<sup>17</sup> Schumpeter, *History*, p. 969.

Consider the alternative: I call this not just *practical* but *clinical* for a reason. Fifteen years with physicians, studying the practice of clinical medicine, have taught me to ignore any claim that medicine is merely “applied” scientific biology: that is a twentieth-century view, fostered to win support for better scientific training of doctors.<sup>18</sup> Advances in the natural sciences of physiology and biochemistry certainly contribute to our inventory of clinical procedures, but the day-to-day art of handling the problems of patients is both closer to the heart of medicine and also older than any of the natural sciences.

Early in the *Nicomachean Ethics*, Aristotle refers to the “timeliness” of all our practical understanding: the need to recognize how the changing “occasions” on which problems confront us affect our ways of handling them. He cites two activities in particular as timely and circumstantial —helmsmanship and medicine. As he himself was a doctor, and the son of a doctor, Aristotle well understood how medical problems arise and run their course, and how a doctor’s actions are adapted to changes in that course. A doctor (we may say) “steers a way” through the shoals of illness and changes the direction of treatment as a patient’s condition develops: if something unexpected comes up, the doctor may have to go off on a new tack. Increasingly, the range of diagnoses and treatments available may be supplemented by new scientific work; but the demands of practice still rule, and the value of scientific theory to clinical medicine must still be measured against those demands.<sup>19</sup>

There is a moment in medical training when a young student faces for the first time a key task of clinical practice: *taking a patient’s history*. “How far is the patient’s condition explained by his or her earlier life, diseases, and experience? And where in that condition are the pointers we require to see what is wrong now,

<sup>18</sup> The key document in this campaign was the Flexner Report of 1913.

<sup>19</sup> Aristotle, *Nicomachean Ethics*, bk. II, ch. ii, 1104a 4–5.

and how can it best be remedied?" These are the crucial questions for what I am here calling *clinical knowledge*.

If I am right, "clinical" understanding plays a part in all the problems of life, moral and technical alike. When Muhammad Yunus invented *social collateral* as a method of securing loans, and founded the Grameen Bank to put this idea to work, his insight was to see that the general theory of banking could be applied to the particular local market in Chittagong only if you made allowances for the social situation in Bangladesh, where there were not enough *material* possessions as the collateral for small business loans. Correspondingly, when economists from the Asian Development Bank ignored traditional irrigation methods in Bali, what was wrong was their failure to understand how these methods fit into the social, cultural, and historical fabric of Bali. Bangladesh was a success, and Bali a failure, in *clinical* economics, and it is no accident that development economists are the ones who best understand the "clinical" aspects of their discipline.<sup>20</sup> Similarly, if my engineer friend in Japan calls for a *humanized* technology, and argues that students of engineering should be taught to judge the human effects of their projects, he too is offering a "clinical" view of engineering in which all *general* computations of structural stresses, quantities, margins of safety, and the rest are evaluated by their effect on the *particular* humans and other living creatures affected by the projects concerned.

This view of practice holds for all the human sciences. The attempt by the behavioral scientists in the academy to keep the human sciences "factual" and "value free" rested all along on misplaced analogies with physics rather than biology. Issues of human value (no doubt) raise methodological problems, but the human sciences are no less *scientific* for all that. The great nineteenth-century physiologist Claude Bernard called his work "experimental medicine": the topic for physiology was the difference between

<sup>20</sup> I have in mind (e.g.) Partha Dasgupta and Amartya Sen. See, especially, Partha Dasgupta, *An Inquiry into Well-being and Destitution* (Oxford, 1993).

(e.g.) a *well* functioning and a malfunctioning heart; and, if that is not a “value” difference, it is hard to say what is! Rather than view sociology or psychology as narrowly factual, value-free disciplines, we may therefore think of them as asking, “On what conditions —social or cultural, intellectual or emotional, collective or individual —do we find human affairs going *well or badly* in practice? And how can we intervene in those conditions to help them go *better rather than worse*?” This suggestion can in no way be condemned as *unscientific*; so human scientists need no longer be shy about discussing the difference between *well* functioning and *malfunctioning* societies and cultures, organizations, and personalities. Indeed, that is just what the rest of us can legitimately ask them to do.

Two postscripts are in order. The first has to do with the contrast between rationality and reasonableness —between the “rational” methods of the *explanatory* sciences and the “reasonable” decisions of *clinical* scientists. Not that an Aristotelian (“clinical”) approach requires us to abandon all hope of establishing “universal” truths: on the contrary, the term “universal” won a place in philosophical usage in an Aristotelian context; however, the force of the term, in that context, reflects its etymology. A “universal” was *katholou* —or rather, *kat’ holou* —and in Classical Greek *kat’ holou* meant the same as the corresponding English phrase “on the whole” (or “generally”), and still means this on the streets of Athens today. It would be odd for a doctor like Aristotle to argue that universals and universal truths are what they are “invariably and without possible exception”: in many situations, a universal is what holds good generally *as distinct from* quite invariably. In the human sciences as in medicine, then, we should keep in mind the difference between the formal deductions that figure “rationally” in mathematical theories and the factual assumptions that “reasonably” underlie medical and other practical arguments.

My other postscript concerns the history of the contrast between an Aristotelean and a Platonist approach to epistemology or phi-

losophy of science. Professional philosophers may argue in principle that the validity of philosophical theories does not depend on the life stories of their supporters; but it can do more harm than good to emphasize this principle without allowing any exceptions. Aristotle (I said) was a doctor and the son of a doctor; and I used this fact to expound the difference between clinical and explanatory disciplines, implying that his firsthand experience of the nitty-gritty timeliness of medical judgments saved him from looking for universality or timelessness —let alone abstraction — where it was not to be found. Plato, by contrast, was preoccupied with fields like geometry and planetary astronomy, where mathematical abstractions had more part to play; so his “ideas” could be conceived apart from all down-to-earth instances, in a way neither Aristotle’s interests in botany and zoology nor his concern for medical problems admitted.

Since seventeenth- and eighteenth-century natural philosophers took their Platonist ambitions from Galileo and Descartes, it is understandable that the role of universal, timeless mathematical theories could be exaggerated in all disciplines. From the start, formal systems modeled on Euclid had a charm that carried people’s imagination over into fresh fields: if the world of nature exemplified in Newton’s dynamics had a timeless order, this could presumably be extended to the world of humanity as well —hence, their readiness to use Newtonian physics as a source of analogies for human affairs. Very soon, indeed, in the Battle of the Ancients and Moderns, the name of Aristotle came to be equated with highly conservative —chiefly medieval —modes of thought. Among academic philosophers, the clinical nature of practical thinking dropped out of sight, and the resulting hostility to Aristotelianism and Aristotle himself lasted up to John Dewey’s time. Formal logic put rhetoric in the shade, philosophically significant theories were expected to make timeless, universal claims, and the contingent world of experience lost prestige, as compared with the eternal truths of abstract reflection. So what other direction could the human sciences initially take?

At the end of the twentieth century, our own position is very different. Rather than jumping from the exact inferences of abstract economic theory (say) to practical recipes for solving concrete, real-life problems, we must keep in mind all the interpretative steps involved in applying any formal theorem to a specific social, cultural, or historical occasion. When he criticized economists' "diletanttic excursions into the realm of practical questions," Joseph Schumpeter had a point. It may be *rational* for us to have intellectual trust in the results of mathematical deductions; but it is *reasonable* to put our trust in the substantive recipes of a clinical science only if these rest on an understanding of the whole situation to which they are meant to apply. In the end, that is how the consideration of human values will most effectively be reintroduced into the practical work of the human sciences.