CHAPTER 6

Rigor and practicality: rival ideals of quantification in nineteenth-century economics

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in Philip Mirowski (ed.)
Natural images in economic thought
Cambridge University Press, 1994

Neoclassical economics, now dominant in the English-language world, emerged out of the so-called marginal revolution beginning in the 1870s. In retrospect, and in the eyes of some of the leading protagonists as well, it seems clear that the crucial change here was nothing so limited as a new theory of value. It was the serious introduction of mathematical reasoning to economics. It is only a slight exaggeration to say that mathematical methods constituted economics as an academic discipline.[1] This conquest of economics by mathematics has become the most lively and exciting area of research in the current history of economics. On the whole, historians of this episode have come to agree with the actors themselves, that the model of the natural sciences contributed crucially to the reformulation of economics. To say this is by no means necessarily to praise neoclassical economics. While economists generally consider their ties to physics a matter to celebrate, historians often have not. Thus, many are inclined to blame inappropriate copying of physics for the willingness of neoclassicals to tolerate bizarrely unrealistic assumptions and to place everything historical, cultural, institutional, and even psychological outside the framework of economic analysis. One of the least sympathetic portraits, by Philip Mirowski (1989), indicts neoclassical economics precisely for its unimaginative copying of energy physics. If true, it is easy to understand why economic assumptions and models might seem to caricature the motives and behavior of real, flesh-and-blood human actors.

I take it as well established now that the model of natural science played a key generative role in the creation of mathematical economics. Indeed, it is not too strong to speak of deliberate imitation, at least for some of the pioneer neoclassicals. But we cannot explain the shape assumed by neoclassical economics so simply. Successful imitation is anything but straightforward. The most indiscriminate copying will not suffice to create a perfect correspondence. What begins as imitation, if it succeeds, must inevitably take on a life of its own. I have argued this point elsewhere in regard to the Belgian astronomer and statistician Adolphe Quetelet. His fanatical commitment to the model of celestial mechanics did not suffice to create a successful "social physics," but rather introduced subtle changes in the way the mathematics he sought to apply was interpreted, changes that subsequently were imported back into the natural sciences (Porter 1985). Mathematical economics, too, has become an important resource for the biological sciences, and even occasionally for the physical ones.

I am concerned here with a different obstacle to unimaginative imitation: that the natural disciplines present nothing like a single model of scientific theory or method. This is not simply a matter of the very different resonances of physics and biology, which since Alfred Marshall at least have been familiar, perhaps to the point of stereotype, among economists. Biology, after all, was a loser in the battle for the soul of economics.[2] Here I will ignore biology and consider economics in the context of its relations to the so-called exact sciences, meaning mathematics, physics, and closely related areas of engineering. There is already within the notion of "exact" science a major ambiguity, crucial for much of the modern history of economics, between what we may call quantification and mathematization. Mathematization implies theoretical formulation in the language of mathematics, emphasizing derivations involving the manipulation of terms to reach new results. Quantification, as used here, refers first of all to purely or partly empirical operations, such as measurement, counting, and statistical analysis. High neoclassical economics assigns a distinctly subordinate place to these forms of quantification and reveres deductive mathematics. Physics and engineering are, to say the least, far more ambivalent about the priority of theoretical mathematics. Of course, economists and physicists alike prefer not to dwell on the distinction, aspiring instead to a fruitful union of mathematical theory and empirical or experimental data. The experimental tradition in physics, though, has been consistently strong, whereas the collection and analysis of empirical information have in the last century become increasingly peripheral to academic economics. To the extent that economists have aimed to pattern their discipline after physics, their principal model has been theoretical physics, not experimentation. This choice was a highly consequential one. There were, I will show, other alternatives, which if anything were closer to the physics model.

Certainly an infatuation with physics never required the hypertrophy of mathematical theory. Until late in the nineteenth century, theoretical physics was not even an acceptable specialty of physics (Jungnickel and McCormmach 1986). Of course, physicists regarded theory as important, but almost never in isolation from experiment, and their customary rhetoric emphasized experimental fact, not mathematical rigor. This is not to say that quantification was of secondary importance, though. A culture of experimental and observational quantification became dominant in physics during the first half of the nineteenth century, so that by 1850 reports without measurements could scarcely be taken seriously.[3] Even then, the purely theoretical paper remained exceptional and was likely to be viewed as vaguely subversive.[4] Meanwhile, vast efforts were devoted to the collection of quantitative data, ranging from stellar coordinates to thermal and electrical conductivities to tide levels. Although in some cases the quantification of measurements was necessary to make them commensurable with mathematical theory, in others there was not even a gesture at theoretical modeling or prediction. It does not at all follow from this that physicists were unwilling ever to let theory run ahead of measurement. Nor can we infer that they were uniformly or even typically scornful of economic abstractions. But they were unlikely to be struck dumb by the appearance of deductive rigor in economic science. If they were not well disposed to classical political economy for other reasons, it was easy to find justification within their own disciplinary traditions to join lay critics and denounce it as baseless theorizing.

None of these scientist-critics aimed to deny the legitimacy of theory, not even in political economy. Nor did they commonly denounce a premature use of mathematics. They objected, rather, to "loose" theorizing. The precision and rigor of quantitative methods were held up as a cure for this looseness. The cure might be a matter simply of deflating excessive pretensions. This was the aim of William Whewell, who despised Ricardian economics and who tried to recast economic reasoning in mathematical form in order to show that its more objectionable conclusions could not stand up to exact investigation. More commonly, physical scientists interested in economics looked to reconstruct it on an empirical basis, to displace abstract theory or at least supplement it with a healthy infusion of measurement and statistics. They were, to follow the distinction already proposed, committed first of all to quantification, and only secondarily to mathematization.

Such a scheme for economics was by no means predestined to failure. The strength of the quantifying impulse in economics in the nineteenth century is attested to by the burgeoning field of statistics. As I will discuss later, an alternative political economy based on an alliance of statistics, physical measurement, and thermodynamics was pursued on more than one occasion by physicists and engineers as well as economists. Marginal economics in the form that was introduced in the 1870s, in contrast, was very much a program of mathematization, one that did not condemn quantification, but was willing to defer it indefinitely. Though patterned in important ways after physical statics, this was not the economics of choice for physicists, and it permitted theory a degree of autonomy from measurement that went well beyond what is normally condoned even in twentieth-century physics.

Mathematical discipline for theorists

Pure theory was never so dominant in classical political economy as the standard image purveyed by commentators and historians would suggest. Even within the apostolic succession of Adam Smith, Thomas Robert Malthus, David Ricardo, and John Stuart Mill, we find a huge amount of empirical and sometimes historical material mixed up with theoretical deductions in the main works of all but Ricardo. The same holds for Marx. Perhaps a few French authors, such as Say and Bastiat, can be categorized with Ricardo. Against them one should place a whole host of economic authors concerned with the statistics of production and trade, monetary history, the condition of the poor, the general advance of prosperity, and public health. Still, the basic theoretical doctrines of political economy had wide currency, appearing sometimes as catechisms. They were easily mobilized for public debate, where they provided ready answers to hot issues of public policy. They upheld an ethic of individualism: Free exchange increased everyone's utility; trade unions could not help the working classes; poor laws aggravated the problem of pauperism; agricultural tariffs enhanced the ability of parasitic landlords to suck up the surplus production of the industrious classes. These policy doctrines were not universally admired. Neither was the idealization of an atomistic world of self-interested economic actors. But no theoretical tradition of comparable elegance, simplicity, or rigor was developed by the opponents of classical political economy. Instead, critics learned to attack its abstraction, its indifference to empirical fact, and its blindness to history, institutions, and legal structures.

It took some time for this opposition to form its own intellectual traditions. When it did, in the 1860s and 1870s, it was under the banner of historicism. Historicism was strong in England, France, and the United States, but almost everyone recognized that its intellectual center was Germany. The historical school became a hotbed of German antimodernism. It was organicist, holistic, antiliberal, and more than a little antiscientific. At least it opposed strenuously the idea that natural science could be a model for historical and humanistic studies. Similar, though generally more moderate, views were characteristic also of historical economics in the United Kingdom and the United States (Kadish 1982; Koot 1987). One naturally infers from this that the classical economists stood for the ideal of science, though perhaps in an exaggerated form. Thus, we would expect to see mathematical and quantitative reasoning deployed by the allies of classical political economy and opposed by its critics.

This is wrong. The central propositions of classical political economy were not expressed mathematically, much less used to predict quantities that could be measured statistically. Jean-Baptiste Say explained why. He insisted, naturally, that political economy must be based on fact. The alternative was the lamentable *esprit de système* that had made it possible to believe gravity was caused by *tourbillons* of invisible matter rather than simple, mathematical forces. But as with every other science, not just any fact would do. A heavy object may be suspended in air by the jet of a fountain, without defying the law of gravity. In the same way, interest rates may for a time diverge from risk, though the law of their equality prevails just the same. The problem is perturbing causes, which conceal the simple laws governing phenomena in economics and mechanics alike. Economics cannot be based on mere statistics, any more than physics can rest on casual observations of carts and fountains. The facts that support economic reasoning must be like the experiments of physics, well grounded and carefully isolated. A mass of indiscriminate observations, all mixed together, is worthless. Perturbing causes cannot prevent economics from attaining general principles, but they make economic prediction impossible. To test economic theory against statistics is invalid and otiose. And if exact predictions cannot be made, there is little reason to try to make economics mathematical.[5]

Statistics provided an ideal of social and economic knowledge that was often placed in radical opposition to the deductions of Ricardo, Say, and Marx. The German historical school was at least as dedicated to statistics as to economic history, and it, in alliance with official agencies, provided the main support for public statistics in late-nineteenth-century Germany. German social reformers pointedly contrasted empirical, factual statistics with the baseless deductions and blind dogmas of Manchester liberals and revolutionary socialists. In Victorian England, statistical writing was deployed in support of the political ambitions and liberal presuppositions of the economists, though even there statistics were most often assembled to endorse paternalistic or state-directed reform, not laissez-faire.[6] And in England too, statistical factuality was sometimes held up as an alternative to the theoretical excesses of the economists.

The great British advocate of statistics in opposition to political economy was Richard Jones. Significantly, Jones was largely responsible for the organization of Section F, Statistics, of the British Association for the Advancement of Science (Goldman 1983; see also Henderson, Chapter 18, this volume). Section F, in turn, formed the kernel of the Statistical Society of London, ancestor of the modern Royal Statistical Society. Jones did not succeed in turning London statistics into a bastion of opposition to Ricardian economics. Certainly, though, he had allies. The one who concerns us here is William Whewell, Jones's lifelong friend and literary executor, and himself an early member of the Statistical Society's governing council. Whewell was not an active social statistician. Nor did he perform original work in historical economics. Instead, he contributed to Jones's cause by writing a mathematical exposition of Ricardian economics. This may seem an improbable alliance: Why should the great enemy of deduction in economics have been supportive of its mathematization? Whewell claimed that mathematics, with its high standard of rigor, could bring out the doubtful assumptions and errors of reasoning in Ricardo's argument. Mathematics would impose discipline on theoretical political economy and block its indiscriminate application.

Economics was by no means Whewell's major intellectual concern. He was a polymath – a leading scientific organizer; master of Trinity College, Cambridge, and thinker and writer on educational subjects; an astronomer, physicist, geologist, and mineralogist. He devoted much of his scientific effort to "tidology," the science of tidal movement, involving the collection of enormous amounts of quantitative data, which he hoped could be brought into accord with mathematical predictions. He is best known now as the author of the three-volume *History of the Inductive Sciences* followed by the two-volume *Philosophy of the Inductive Sciences* and a last one, *On the Philosophy of Discovery*.

Whewell's philosophical outlook is the obvious place to begin seeking an understanding of his critical approach to political economy (Hollander 1983). We find, to begin, that political economy is not a topic of Whewell's history or philosophy. This was, after all, history teaching by example, and its author found nothing in political economy that could fit it to be a model for other scientific investigations. On the contrary, he thought political economists had much to learn from the example of the more successful disciplines, meaning the natural sciences. So Whewell criticized Ricardian economics not because he thought the model of natural science inappropriate for political economy, but because political economists had departed too far from the historical pattern of successful scientific investigation.

That pattern involved, first of all, induction. Whewell professed admiration for Francis Bacon, and we find him arguing over and over that science should proceed by induction to successively broader generalizations. The temptation must be resisted to leap from a few casually observed facts to vast, all-embracing principles and proceed thereafter by the easy path of deduction. This last is, of course, what he thought Ricardo had done. His mathematical Ricardianism was intended mainly as a destructive project, to join political economy to mathematics and thereby to "make nonsense of it."[7]

For the more positive task of reconstructing political economy, he had a close ally. This was Jones, a friend since their undergraduate days together at Cambridge. Whewell wrote often to Jones, encouraging his research and complaining about his opponents, from the late 1820s until Jones's death in 1855. He wrote in 1828, for example, that if the political economists "will not understand common sense because their heads are full of extravagant theory, they will be trampled down and passed over; and it will be the height of indolence and bad management if you allow other heels to take the *pas* of yours in this most meritorious procession" (Todhunter 1876, 2: 94). As Whewell's remark implies, Jones was somewhat remiss in finishing his work and publishing; although his comparative study of rent came out in 1831, the projected succeeding volumes never appeared, and his next major publication was in 1858, three years after he died. This was due to Whewell, a prolific author, who had become his literary executor. Whewell (1859) praised Jones's reliance on induction and cited with approval his doctrine that Ricardo's theory of rent could apply at most to "farmers' rents," which were to be found almost nowhere outside Britain and the Netherlands. Mere deduction applies to nothing at all unless it takes customs and legal arrangements into account.

Whewell's commitment to induction was anything but pure, and it is probably a mistake to make this the crucial factor in his opposition to Ricardo. To be sure, he emphasized its importance throughout his life, especially whenever he had occasion to discuss political economy. That science, he argued in 1860, violates "the precepts that we must classify our facts before we generalize, and seek for narrower generalizations and inductions before we aim at the widest" (Whewell 1860, 298). As a member of the council of the London Statistical Society, though, he quickly became disillusioned with its radical commitment to facts, to the exclusion of all expressions of opinion. "I am afraid you will think me heterodox," he wrote the Belgian statistician Quetelet in 1835, but investigation depends on working theories if it is to get beyond unconnected facts. "Theories are not very dangerous, even when they are false (except when they are applied *to practice*)" (Whewell 1835). He insisted, against Mill, that induction can never be mechanical, that it is meaningless to talk abstractly of causes A, B, C, and effects a, b, c. Induction should be based on facts, but there is an irreducible element of intuition involved in any discovery of causes or laws, and Whewell believed that hypotheses are invaluable for guiding investigation even if the end result might be to discard them for some other explanation. Jones, for one, came to believe that Whewell's philosophical writings departed too far from a proper inductivism (see de Marchi and Sturges 1973).

We should not think of Whewell's views on method as abstract and monolithic. Political economy he regarded as something distinctive, deserving of his sharpest barbs. Clearly it was not immaterial that Ricardo had reached conclusions the reverend master of Trinity College found thoroughly unappealing. He complained repeatedly of the premature application of political economy to practice. In particular, he opposed Ricardo's notion of class conflict, that the landed classes were tending to absorb an ever increasing fraction of production in the form of rent, at the expense of the productive members of society.[8] Still, his remarks on method were no mere disguise for naked political antipathies. His great objection to Ricardo the theorist was not simply the rigidity of his deductions, but also their looseness. Ricardo's methods seemed to him weak. Verbal reasoning is too slippery. It does not require that the premises be made clear and permits auxiliary hypotheses to slip in unnoticed. It provides no clear checks against errors of reasoning. Verbal methods, in short, are too weak to guarantee correct reasoning and too imprecise for their results to be tested against those uncompromising judges, experiment and observation. Mathematical economics could overcome these defects. The result, of course, might often be to show that we are not yet able to succeed at deductive reasoning, that our premises are not sufficiently in accord with the world. But this, too, is valuable to know. Exact results, even if faulty, are to be preferred to imprecise, sweeping conclusions, to "the statements which we perpetually receive from the economists, of that which must necessarily be but yet is not, and to general 'truths,' to which each particular case is an exception" (Whewell 1831, 61).

Whewell's professed goal in his mathematical writings on Ricardian political economy was to eliminate this looseness. He did not expect important practical results from the enterprise: "Mathematical calculations," he conceded, cannot "supply the place of moral reasoning." One can no more reduce the business of the world to mathematics than mechanics can be used to understand the working of machines when we ignore friction, resistance, and the imperfection of materials. But Ricardo and others had based their reasoning on so few principles that mathematical solutions were readily available and, indeed, "might have been done in a few pages." In this way, the reasonings would have been made "almost infallible," and the mathematical results "could be compared with practice so as to show whether the problem was approximately solved or not" (Whewell 1829).

Given all this, it is hard to be surprised at Whewell's conclusions. Ricardo had allowed dubious tacit assumptions to creep into his argument. Once exposed and made explicit, Ricardo's qualitative assumptions could be judged against historical and empirical work of men such as Jones. Whewell did not himself work out theory to the point of quantitative predictions that could be compared with statistics, but he seemed not to anticipate its total vindication. He claimed also to find mistakes in Ricardo's abstract verbal reasoning. Ricardo erred, for example, in his inference of the effect on rent or profits of growing English prosperity, and of the sector upon which taxes of various descriptions would ultimately fall. Not that Whewell believed the mathematician could reach decisive, exact conclusions on these points. His purposes were more critical than constructive: to show "of what kind and how many are the data on which the exact solution of such problems may depend" (Whewell 1829, 1831). Mathematics should not supplant empirical investigation but could clear the ground for it by revealing the weakness of verbal deductions.

Specific grievances also lay behind several other economic efforts by men trained in natural science and economics in the late nineteenth century. The most common whipping boy in the 1860s and 1870s was the wages fund doctrine. This was an old doctrine of imprecise meaning – from one standpoint, it amounts to little more than a balancing of accounts. But it also provided an opportunity, or pitfall, for those infected by the Ricardian vice. All other things being equal, this fund is a limit on wages, and though in reality the other things are never equal, and though even if they were, wages might not have reached that limit, this doctrine did provide language of some use to those who were unfavorably disposed to trade unions. Collective bargaining, it was sometimes argued, is useless, since it cannot expand the fund available for wages. Or if one group of workers, through effective organization, gets more, it must come from the pockets of their less greedy fellows.

Fleeming Jenkin, who achieved some note in the history of mathematical economics for his papers on the graphical representation of the laws of supply and demand, was moved to this effort by a desire to clear up the wages fund doctrine. Jenkin wrote his papers in 1868 and 1870, while a professor of engineering at the University of Edinburgh, and he may be counted with James Clerk Maxwell, William Thomson (Lord Kelvin), and P. G. Tait among the notable Scottish mathematical physicists of the nineteenth century. He was a classmate of Tait and junior of Maxwell at the Edinburgh Academy, and he became very close to Thomson, when the two joined in planning and laying submarine telegraph cables. He also had known physical labor, having worked his way up from an apprenticeship as an engineer, and as Robert Louis Stevenson put it, he knew the working classes too well to regard them "in a lump" (1887, 1:xlix). He was not, however, an opponent of political economy, and in particular he spoke repeatedly in favor of free trade. No devout enemy of the economists could end a paper as Jenkin did: "Whatever school of religion or philosophy we belong to, we cannot deny that each man, acting rationally for his own advantage, will conduce to the good of all" (Jenkin 1870, 2:105).[9]

The verbal argument from the wages fund principle against the possibility of workers benefiting by trade unions has a certain plausibility, he allowed. Certainly there will be a tendency for whatever reduces profits to reduce also the fund available for wages. But there is a fallacy here: "The motion of a body is not determined by one force only" (Jenkin 1868, 2:9). The problem with the wages fund argument is that it does not tell us how this fund is determined; it is in fact affected by a myriad of circumstances, all of which can affect the rate of wages. How is the fund determined precisely? We don't know, said Jenkin: "No economist has hitherto stated the law of demand and supply so as to allow this calculation to be made" (2:15). Here was an obvious desideratum. To work out the interaction of causes required, if not an abstract mathematical formulation, at least generalizable quantitative techniques. So Jenkin, like Whewell, took to mathematics out of frustration with verbal reasoning that was, perhaps inherently, too vague to permit understanding in detail. Unlike Whewell, Jenkin thought his mathematics adequate to make a real contribution to an understanding of the problem, not mainly an agent of debunking. But it is significant that his conclusion was to pronounce the solution indeterminate, at least without a considerable improvement in the empirical data.

The task was to find the equilibrium between supply and demand. These are, of course, functions of price – or, in the particular problem here addressed, of the wage rate. But the shape of these curves is not given timelessly by nature. They depend, as Jenkin put it, on states of mind – of the capitalist and of the workers. "The laws of prices are as immutable as the laws of mechanics, but to assume that the rate of wages is not under man's control would be as absurd as to suppose that men cannot improve the construction of machinery" (Jenkin 1870, 93). Hence, so-called "laws" of demand and supply "afford little help, or no help, in determining what the price of any object will be in the long run" (Jenkin 1870, 87). Unorganized laborers are at a great disadvantage; those who do not bargain collectively are like goods to be unloaded in a bankruptcy sale. Hence, organization into trade unions most certainly can improve the worker's lot. How much? In a subsequent paper on the incidence of taxes, Jenkin suggested empirical measurement of supply and demand schedules to resolve the effects of taxation experimentally, and the same methods would apply to wage studies (Jenkin 1871–2). But given the mental component that he emphasized so heavily in the determination of wage rates, prediction here might well be beyond the capability of the political economist's art.

Quantitative programs for political economy

Whewell's anti-Ricardian campaign is suggestive of the ways in which mathematical reasoning could be turned against deductive political economy. It did not offer a positive program of quantification. Neither Whewell nor Jenkin wrote mathematical theory in a form that would yield predictions of statistical results. And despite Whewell's warm embrace of induction, he made almost no effort to gather the economic facts he so piously defended. Jones of course did. His ideal economics was to be thoroughly statistical and untheoretical. Whewell wondered if this might be going a bit too far, though he clearly preferred it to the opposite extreme. So did Charles Babbage, Whewell's contemporary, best known even in his own day for his "calculating engine." Babbage was a founding member of the Statistical Society of London and the author of a very successful book on the machinery question. No more than Whewell or Jones did he admire classical political economy: The "closet philosopher," he wrote, is too little acquainted "with the admirable arrangements of the factory" (Babbage 1833, 156). On these matters, Babbage practiced while Whewell preached. The effects of machinery were arguably the greatest economic issue of the time, a major concern of much early-nineteenth-century empirical work on political economy (Berg 1980). Babbage allied himself unambiguously with those who would measure and count, not with theorists. This included many "practical men," such as members of parliament, who allowed that Ricardo might be right in theory but insisted that such abstractions could never be adequate for a legislator facing a complex world (de Marchi 1974). It was also the prevalent view among natural philosophers who wrote on political economy.

One may be tempted to regard this empirical attitude as characteristically British, especially in the time of Whewell and Babbage. In a way it was, but the greatest success of statistical economics came in imperial Germany. There, the mathematical approach to political economy was in sharp opposition to the individualism of the classicals. There also, the historicist revolt was so strong as almost to extinguish deductive economics. In just one German-language university did it thrive – in the Vienna of Carl Menger and his students. Menger is often grouped with Jevons and Walras because of his marginal utility theory, but unlike them he made no use of mathematics. His economics was not only nonmathematical, but also largely nonquantitative. It is curious but revealing that in the great *Methodenstreit* between followers of Menger and Gustav Schmoller, we find mathematics mainly on the side of *Historismus,* not with the deductivists. Of course, the mathematics involved consisted not mainly of deductive models, but rather statistics. Still, there were a few prominent figures who went beyond presenting numbers and sought to develop higher methods to analyze them. G. F. Knapp and Wilhelm Lexis, in particular, saw themselves as champions of mathematical precision *and* faithfulness to the complexities of experience, as against the indefinite generalizations of the verbal deductivists in Vienna, Paris, and Manchester.

The historical school economists, even more than Whewell, were opposed to the classicals and neoclassicals on moral grounds. They objected particularly to the individualism of traditional political economy, to its assumption that principles regulating the behavior of individuals could be posited independently of the larger community to which these individuals belonged. This was, they thought, to place humanity in the realm of nature and of mechanical law. Humans belong to the domain of history and of progress – to free communities that gradually change, along with the individuals who make them up. In place of selfish utilitarianism, the historicists called for social responsibility, to be expressed partly through free associations such as worker cooperatives and partly through the activity of the state.

But here, as almost always, intellectual convictions cannot be reduced to mere ideology, even if ideology is often an important component of economic views and approaches. That Knapp and Lexis were not prisoners of anti-Enlightenment, antiliberal dogmas is strongly evidenced by their devoted pursuit of mathematical social science. Knapp, in his much later autobiography, reports equal disgust dating back to his student years in the early 1860s with unimaginative statistical compilations and deductive political economy. The former seemed to him vacuous. The latter he called a useless *Gymnastik,* a mere student exercise without scientific value and inapplicable to real problems. To be sure, he conceded, political economists have often written intelligently about practical matters. But they do so in their examples, and for this purpose the dogmatics are put aside (Knapp 1927). As a doctoral student at Göttingen, Knapp was put to work on the wages fund doctrine. He concluded that it was fallacious. There are, he argued, too many variables for a rigorous solution to the problem of distribution, even in Thünen's "isolated state." Thünen, he held, was forced to treat some quantities as independent variables that in fact were dependent ones. Hence, the "general, absolute validity, that Thünen ascribes to [his equation] . . . , is lacking, and it most certainly does not hold in the real world" (Knapp 1865, 12). Knapp would eventually make his mark as an economic historian in studies of peasants and agriculture, but his first serious social studies were statistical. He worked for a time as director of a government statistical office, in Leipzig. He also wrote mathematical works on demography – the one demonstrably practical area of exact social science, since those methods were used to calculate life insurance and annuity premiums.

Wilhelm Lexis criticized Menger for his failure to incorporate mathematics into economics, but he also was skeptical of the mathematical marginalist theory of Walras. These abstract propositions are valuable, he conceded, but they show no more than tendencies. They do not give a "reliable predetermination of actual events, and cannot by themselves decide the measures to be taken in pursuit of goals in economics" (Lexis 1881, 427). His response to the gap between economic theory and practical concerns was to emphasize the study of disturbing forces, which can be identified and estimated only through empirical research. In particular, he pursued something rather like what we know in the twentieth century as mathematical statistics. Neither Lexis nor Knapp was the patient, disinterested observer of society that their critique of the theoretical excesses of classical economics might seem to demand. Lexis aimed throughout to demonstrate that humanity was not subject to natural laws, independent of time and place. Using statistical methods he aimed to demonstrate, with the conclusiveness of mathematics, that moral and social behavior vary greatly over time and place and that societies cannot be reduced to a sum of autonomous individuals (Porter 1987; Wise 1987). And beyond social metaphysics, his statistical research also supported the gentle interventionism of the "academic socialists" in the Verein für Sozialpolitik. Effective state activity, he believed, presupposed adequate expertise. The test of this expertise was empirical adequacy, and mathematical reasonings had to be held to this standard if they were to be usefully applied to practical questions. Of course, he and Knapp did not reach a perfect accord between theoretical understanding and quantitative measures either. Their statistical methods, though, were calculated to manage the economy, while classical deductions showed mainly why political authorities should leave it alone.

The economics of engineers and physicists

Engineers are often required by their profession to practice economics. Physicists, at least in their familiar capacity as researchers, generally are not. But the line between physics and engineering has not always been very sharp. The gap was kept narrow through most of the nineteenth century as a result of the great importance in physics and engineering first of steam engines, and then of electricity. Especially in the early part of the century, relations between thermodynamic and economic ideas were extremely close. Each made use of ideas from the other. By no means was economics simply parasitic on physics; economic and physical ideas grew up together, sharing a common context. An economic point of view formed the root of thermodynamics. But this was not mainly a matter of physicists depending on the work of Ricardo or Say. The economic mentality at issue here was associated more closely with accounting than with high theory. And this economic conception itself already integrated a labor theory of value with a set of analogies involving engines (Wise 1989–90).

This fruitful confrontation of physics with engineering and economics first took place in France. It was closely associated with the culture of the École Polytechnique, created during the French revolution to enlist science in the service of the French state, especially in view of its pressing military needs. It was the great French scientific and engineering school, the first institution to make science and mathematics central to the education of engineers. Its raison d'être was to produce knowledge that was at once mathematically elegant and useful. After 1815, the French found themselves decades behind the British in the technology of the steam engine, and engines became an important topic of scientific as well as engineering inquiry (Fox 1986). These engineers were not content to approach steam engines as a problem of craft skill and merely technical ingenuity. They were scientists, and they sought an adequate scientific vocabulary for talking about the effectiveness of engines. An adequate vocabulary, naturally, presupposed the possibility of measurement. In this context was introduced the crucial physical notion of work.

For physicists and engineers like J. V. Poncelet, Charles Dupin, and Louis Navier, work referred to something more physical and more readily quantified than labor. It came to be defined as a product of weight and the height to which it was raised, the action of a force through a distance. But this was not merely a physical unit. It was also a measure of labor power, of work in the colloquial and economic sense. With it one could compare machines with humans or animals. One could talk about efficiency and productivity. The transmutation of heat and electricity into work became conceivable, indeed measurable. This was an important ingredient in the formulation of the doctrine of energy conservation (Grattan-Guinness 1984, 1990; Mirowski 1989).[10]

With the transfer of French physics to Britain, the rich concept of work was introduced as well. There, as Norton Wise has shown, work, meaning energy, became the basis for an alternative economics. The economics of energy was ideally suited to measurement, for it permitted the productivity of labor to be assessed against an absolute, physical standard. The champions of energy economics were not generally hostile to free trade, laissez-faire, or the other leading doctrines of classical political economy. Neither, though, were they content with an economic science that was mainly theoretical. Here was a form of economic reasoning and, more crucially, a system of economic practice that would permit scientists to judge the productivity of machines and labor, as well as to improve them. In this economics, statistics of factories, workers, and production meant something. Quantification could aid administration, could guide the improving activities of engineers and reformers (Wise 1989–90).

In Britain, the most important early champion of the new French physics of work was Whewell, author of an 1841 textbook entitled *Mechanics of Engineering*. Whewell wanted to raise engineering above mere craftsmanship, to introduce physical theory in alliance with physical measurement. His book made the foot-pound the common unit of laboring force. This had many advantages. Crucial among them was that it could readily be expressed in quantitative terms, to compare the labor of machines, animals, and humans. The advantage of machines could thus be expressed in familiar terms. James Thompson, brother of the famous physicist William and himself a distinguished engineer, gave a typical calculation in 1852. His pump, he found, could lift water at the rate of 22,700 foot-pounds per minute. A man can lift only 1,700 foot-pounds per minute, and that only for eight hours in a day, so that the pump was doing the work of forty men. Physical work, as Wise remarks, was here literally labor value (Wise 1989–90).

Even more crucially, this formulation permitted a clear distinction between useful work and waste, and indeed gave a quantitative expression of efficiency. This was invaluable to the industrial engineer and also to the reformer and philanthropist. Calculation could be used to determine an optimal mix of machine labor with human labor. James Thomson calculated to decide whether it was energetically advantageous to boil urine as fertilizer, thereby producing an increase in food for human workers, or to employ the coal fire directly for productive work (Wise 1989–90). At about the same time, William Thomson showed how energetic and monetary calculations could be combined to reach an optimum in telegraphy. Once he had learned how to measure the retardation of signals in a wire, it became "an economical problem, easily solved . . . to determine the dimensions of wire and covering which, with stated prices of copper, gutta-percha, and iron, will give a stated rapidity of action with the smallest initial expense" (quoted in Wise and Smith 1987, 326). And with this we begin to discover the benefits of energetic calculations for friends of the poor and working classes, especially those hailing from the Gradgrind school. R. D. Thomson, of the Glasgow Philosophical Society, looked forward to the day "when the light of science will enable the guardians of the poor to manage our poverty-stricken fellow men by precise and definite rules" (quoted in Wise 1989–90, 224). To this end, the Glaswegians were pleased to make use of a tabular presentation of the nutritive value of various food items: beans, peas, wheat, rye, oats, cabbage, and turnips. R. D. Thomson presented the nutritive values of various types of bread, in comparison with costs, to aid in minimizing the cost of supplying energy to human labor power. This was, as Wise remarks, rather like measuring the energy content of coal or the efficiency of machines. Lewis Gordon, another Glaswegian and the first professor of engineering in a British university, appreciated that measuring physical labor power and weighing bread yielded comparable numbers. Together they enabled the engineer to design and run factories with a maximum of efficiency.

The economics of energy here implied no rejection of the more customary medium of economic quantification, money. Its crucial feature was the search for standard, comparable units. This was a form of economics patterned after physics that aimed far less at theoretical elegance than at practical management and efficiency. The contrast with the mathematical economics of the marginalists could scarcely be more vivid. And the economics of quantified energy, unlike that of mathematized utility, won the interest and even enthusiasm of contemporary physicists.

One can find a similar approach, even more coherently developed, in France. "Engineers do economics while others talk about it," argued one twentieth-century French polytechnician (F. Caquot, quoted in Divisia 1951, x). The École Polytechnique and the École des Ponts et Chaussées had long recognized that the business of the engineer required a familiarity with economic ideas. There were, however, enduring doubts about whether the writings of those who called themselves political economists were capable of supplying what the engineers needed. Classical economics was, some argued, too impractical, too qualitative, too dogmatic. The engineers cultivated their own economic tradition, which borrowed sometimes more, but often less, heavily from Say, Rossi, Garnier, and other classical French economists.

One important Polytechnique engineer, whose work overlapped in important ways with Fleeming Jenkin's, was Émile Cheysson. Cheysson was a member of the French civil engineering corps, the Ponts et Chaussées. He was also a pioneer of graphical statistics and an influential social reformer in the tradition of Frédéric Le Play, whose monographic study of selected family budgets Cheysson saw as complementary to statistical method.[11] Cheysson called statistics indispensable for the management of men, for social engineering. He wanted to use them to divert economics from its abstractions, emphasizing instead the "study of the conditions that produce the well-being, the peace and the life of the greatest number" (quoted in Elwitt 1986, 67).

Predictably, Cheysson took physics as his model for political economy. Economics, of course, suffered by the comparison. It lacked, he said, a common unit: The value of money is too changeable, and utility is impossible to measure; unlike many predecessors, he did not pursue energy as an alternative.[12] Hence, he argued, economics can make no pretense to the rank of an exact science. "Despite ingenious attempts," he proclaimed in 1882, "the rigorous procedures of algebra have been proven sterile in application to this order of phenomena, for the equations are incapable of embracing all the facts" (Cheysson 1911b, 2:48). But Cheysson did develop ideas that tended to the mathematization of decision making. His outstanding contribution on these lines was his article on the geometry of statistics, first published in an engineering journal in 1887. It aimed to extend the skills of the engineer to business decisions about products, supplies, markets, and prices. Unlike the political economy of Walras and Jevons, with which he was well acquainted, it was not a mere abstraction, "speculative analysis," but a quantitative tool developed for practical reasons to solve practical problems in public and private affairs. It would permit decisions to be made without groping toward an optimal price or tax rate through trial and error, but instead by solving such problems directly.

Cheysson advocated graphical methods for finding optima of this sort, though he conceded that analytical methods could attain the same results. Analysis, he remarked, required mathematical sophistication and lacked the intuitive appeal of that *langue universelle,* graphical statistics. Also, the graphical method is quite simple. Suppose we want to determine how much to charge for railway travel on some line or network. We must plot two curves, one of demand and one of costs, each as a function of charge per kilometer. From them we can calculate a curve of net revenue, which the company aims to maximize. The highest point on this curve is the solution. Sometimes extrapolation will be required, but only when the company has always charged rates on one side of this optimum. In that case, the potential benefits even of an approximate solution are very great. He gave as an example the Austrian Nordbahn, whose rates had always been far too high to maximize the profitability of the company. Such errors seemed especially egregious given that the public interest demands rates below this point of maximum profits. Cheysson's methods were not limited to transport problems. They could also be used to establish optimal wage rates for workers, and thus provide powerful tools of social betterment. And they could guide investment decisions or the setting of tax and tariff rates (Cheysson 1911a, 1: 185–218).[13]

Cheysson, though a loyal engineer, had a multifarious career, most of it outside the Corps des Ponts et Chaussées. Still, his economic interests were in many ways typical of French state engineers. Nowhere was practical economic quantification more skillfully developed in the nineteenth century. The Corps des Ponts et Chaussées was an administrative agency, not just a team of engineers. As François Etner observes, it was charged with budgeting and choosing among projects, "all in the name of the public interest and in accordance with rules that should be written, public, and non-discriminatory" (1987, 115).[14] In the interest of rationalization, these engineers endeavored to make physical parameters such as mechanical efficiency, friction, and wear commensurable with costs of construction, maintenance, and operation. Choice of materials in a road or the decision about steepness of grades and sharpness of curves on a railroad were economic problems, as was recognized in any number of papers by state engineers on the construction of routes.[15] An outstanding example is the solution to the problem of road maintenance given by Jules Dupuit, the only one of these engineers to gain a lasting international reputation among economists. It was unmistakably an economic solution, in which physical measurements were in the end translated into monetary terms (Dupuit 1842).

Dupuit's reputation survives among economists because he used the principle now known as "diminishing marginal utility." He did not invent it as the basis of a program of mathematical deductions, but rather to attain a satisfactory measure of the public benefits of a railroad or canal. It is significant that Walras, the French-language pioneer of marginal utility theory, disdained to include the engineer Dupuit among his precursors, and in a way Walras was right. For purposes of calculation, though, Dupuit wielded his principle very effectively. It was designed as an improvement on some formulas proposed by Navier, who introduced this form of cost–benefit quantification in an attempt to show that the benefits of a canal would normally far exceed the revenue it brought in. The best measure of benefits, Navier proposed, is not tolls charged, but costs saved – the product of volume of goods moved on a canal by savings per ton-mile over transportation on the roads. Dupuit declared this formula far too generous. Much of the traffic on the canals depends on their low charges and would not move at all if water transport were not available. These shipments do not yield benefits equal to the full difference between road and canal shipping costs, but only the difference between actual costs on the canals and the increase in value resulting from the transportation. As the cost of transport goes down, the volume will go up. Hence, the total benefit due to the canal cannot be the product of volume with cost differential, but must instead be represented as the area under a curve that plots the number of units that would be transported on a rail line or canal as a function of the toll charged (Dupuit 1844, 342; see also Ekelund and Hébert 1978; Smith 1990). Dupuit assumed, with eminent reasonableness, that, as tolls go up, usage will go down. This corresponds with the doctrine of diminishing marginal utility.

Dupuit did not suppose that these curves could be plotted directly from statistics. If, however, rates had varied over time, one could surmise something about the shape of the demand curve. At least his quantities were observable in principle, quite unlike the personal utility of the next generation of economists. And Dupuit's general strategy for calculating the benefit of public works became standard for guiding policy on their construction and pricing. His methods were taught, for example, in the authoritative textbooks on the economics of public works published toward the end of the century by Clément-Léon Colson, also of the Corps des Ponts. Colson was not a man of speculative bent. He complained of those economic authors who are content to reason deductively and mathematically, and thus "have often deviated completely from real facts in their most ingenious theories" (Colson 1907, 39). Engineers, he stressed, are practical men. Their economics should stay close to the facts, to statistics, so that it will be useful in administration. This, indeed, was Corps dogma. François Divisia, in a later celebration of the economics of French engineers, did not conceal his scorn for pure economics:

> How far we are from its resonant controversies that go round and round through the decades or the centuries, from its clever and subtle dissections, the games of mandarins, from its previsions that are just the opposite of reality one time in two, from its experiments that really aren't and that lack even the value of a lesson in facts. Economics! Is it, after all, anything more than a job well done, what all our engineers can do? (1951, 101)

Walras confronts the polytechnicians

The differences in view separating mathematical economists from engineers and physicists are compellingly illustrated by the career of Léon Walras, generally viewed by modern mathematical economists as the most important neoclassical pioneer. Two recent books show to what extent Walras took his mathematics from standard works of physics, particularly from potential theory in statics (Mirowski 1989; Ingrao and Israel 1990). One might have expected a cordial welcome for mathematical economics from those trained in modern physics. Walras certainly hoped for one. Recent studies of Walras and A. A. Cournot, noting their almost complete isolation from the French legal and literary school of political economy, have tried to connect them instead to the mathematical and engineering traditions of the École Polytechnique (Ménard 1978; Dumez 1985). The relations between mathematical economics and French engineering were important ones. They were, however, exceedingly stormy. Their history highlights the differences between economic mathematization and quantification.

Cournot's 1838 book can reasonably be called the first serious work of mathematical economics. It was, in its time, a complete failure, despite the considerable reputation of its author. He was not actually a polytechnician, but a graduate of the École Normale Supérieure. Among his classmates was Walras's father Auguste. The École Normale was a school of science and scholarship that educated teachers and researchers. This may be contrasted with the École Polytechnique, whose mission was, of course, to train engineers. There is considerable ambiguity here, since the curriculum at the Polytechnique was strongly oriented around mathematics. Especially in its first decades, up to the 1820s or 1830s, it was the central institution of French science and mathematics. Pure mathematics helped to maintain its standing as an elite institution in a conservative society. Yet, as Jean Dhombres (1987) has argued, the practical ethos of engineering and management was already strong there in the 1820s, and it became even more dominant as the century advanced.

Cournot, no engineer, aimed to sharpen up economic theory by rewriting it in terms of general mathematical functions. He took care, though, to frame his theory in terms of observable economic quantities – money and prices. He set out by showing how to use the method of least squares to chart the changing value of precious metals, based on an explicit analogy with astronomy. In this way he hoped to establish a fixed unit, to facilitate reliable measurement, and to permit comparisons across time. Thus, one cannot call Cournot indifferent to the investigation of economic quantities, and it is significant that the model of natural science entered his reasoning most explicitly where he was most concerned with measurement. But his books on economics and probability were written mainly from the standpoint of a mathematician, and scarcely more than Whewell did he have a workable vision of a quantitative economics (Cournot 1838, 1843). As Claude Ménard points out, Cournot's strategy of economic mathematization depended on excluding history, with its irrationality and perpetual disequilibrium. Cournot was willing to pay the price of mathematical rationality by excluding the whole domain of *économie sociale,* all the complications that would be as mud to the pellucid waters of pure economic reasoning. The "logical reconstruction" effected by Cournot's mathematical approach was made possible by his willingness to assume pure rationality and not to limit himself to what could be ascertained empirically or applied to policy. Real economic decisions, he conceded, involve so many complex factors that practical sagacity outweighs scientific apprehension (Ménard 1978).

Walras was a great admirer of Cournot. He claimed in his correspondence to have gone beyond Cournot mainly in the purity and rigor of his methods. "You," he wrote, "follow a route that takes immediate advantage of the law of large numbers and leads to numerical applications, while my work remains free from that law on the terrain of rigorous axioms and of pure theory."[16] To be sure, he did not always discuss his work this way. In his letters to Jules Ferry, the French minister of education, he was much more eager to claim practical relevance for his theoretical insights, or even to hold that some pressing problem such as railroad rates could not be solved until economic theory was better developed.[17] And Walras, unlike Cournot, did write on practical issues. He even became active twice in campaigns for economic reform: first, at the beginning of his career, in favor of free trade, and then, near its end, as an advocate of land socialization. But the interpretation of his own work he sketched for Cournot is at least defensible. Cournot framed his theory mainly in terms of macroscopic variables such as the quantity of money. Walras's originality as a theorist owes principally to his deductions from an abstract model of free exchange, leading to an even more abstract theory of general equilibrium. His microeconomic approach, like most, could be used as a language to describe the behavior of a profit-maximizing firm, but this was not why Walras developed it.

Walras was no polytechnician. His mathematics was not good enough to succeed in the competition for entry. He did study as an external student at the École des Mines, which, like the École des Ponts, accepted as ordinary students only the most elite graduates of Polytechnique. He was not entirely indifferent to applications of social mathematics. He served briefly as actuary for a Swiss insurance company. He sent not only letters to Ferry about railroad rates, but also, in 1875, a long memoir. He hoped that pure economics would guide practice in these areas. In 1873 he wrote his colleague at Lausanne, the engineer Antoine Paul Piccard, that "by introducing into pure political economy the precision of definitions and the rigor of deductions that prevails in pure mechanics, . . . most rules of applied political economy" could be demonstrated mathematically.[18] This was, however, by no means direct and simple. Pure political economy, he held, should be constructed on the model of astronomy – "the type to which, sooner or later, the theory of social wealth must converge." It will study "natural facts" of human behavior, which are more basic than social conventions and which "impose themselves on the human will." Such laws can be expressed in abstract mathematics and provide the proper foundation of political economy. Adam Smith and J. B. Say had never gone beyond what he called "applied" political economy. He anticipated that in the future this would be grounded on his more fundamental theory. But we still have not reached the practical rules of economic policy. They were to be given by a third subdiscipline, "social economy," which would connect with the deepest level of theory only through the mediation of the second.[19]

And even this was an expression of youthful enthusiasm, written in 1862 before Walras had any specific vision of mathematical economics. By 1876, when he published his *Eléments d'économie pure,* he had already become more pessimistic. Later he virtually stopped claiming policy applications. Asked for official advice about tariffs in 1881, for example, he answered that he did not command the detailed knowledge of the conditions of Swiss industry to justify a recommendation and that he lacked the interest to devote the needed time to it. "I am a man of pure theory," he explained. He still hoped that others would take the trouble to define a more rational practice on the foundation of his theory, but he saw no reason to be very hopeful.[20]

This remoteness of Walras's theory from practice was recognized also by engineers and seems to account for their lack of interest in his work. Only in retrospect, out of bitteness, did Walras reciprocate their disdain. At first he courted them assiduously, for he had no other supporters. He tried to gain entry to the Institute of Actuaries, a group of Polytechnique graduates dedicated to the quantitative study of economic problems who took insurance mathematics as their model. Walras's general equilibrium theory was too abstract to interest them. Although they were quite able to understand it as pure mathematics, they could never see the point (Dumez 1985).

The history of Walras's relations with them is instructive. In 1873, he presented a paper at a meeting of the Académie des Sciences Morales et Politiques in Paris in hopes of making his work known to the leading French economists. Disappointed, though not completely surprised, by their incomprehension, he was correspondingly pleased to hear afterward from Hippolyte Charlon of the newly formed Circle of Actuaries. Charlon offered its journal as an outlet for the economist's work. Walras, in reply, declared himself pleasantly surprised to discover that he was not so isolated in France as he had thought.[21] He soon sent Charlon a memoir, the crucial chapter of the *Eléments d'économie pure,* for separate publication in the hope of drawing attention to his forthcoming book. After a long delay, caused by internal disagreement among the actuaries, Charlon explained that the *Journal des actuaires français* had decided not to publish his memoir. Although Charlon had found it "very remarkable and abounding in sound ideas," it was also "off the practical and positive course along which we have directed our Journal. There is a crowd of sciences that, more than political economy, employ or could employ mathematical methods. This is no reason for them to be the object of our publication." There seems, he speculated, to be an unfortunate "incompatibility of humor between economists and actuaries."[22]

This incompatibility resurfaced in the correspondence between Walras and Hermann Laurent in 1898. The Circle of Actuaries had fallen into abeyance in 1880; Laurent was the moving force in its revival, in 1890, as the Institute of Actuaries. Like Charlon he had studied at Polytechnique. He was also a distinguished physicist and mathematician, and he took the model of the physical sciences very seriously. In his correspondence with Walras, he wondered whether economic comparisons over time might be facilitated by using a measure of energy, rather than currency or utility, as the standard economic unit. That is, he wanted economics to be based on measurement, and this could not be accomplished with a fluctuating unit like money. His aim was to make economics more practical, which, he explained, required that it be made mathematical.[23]

He was no enemy of Walras. He published in 1902 a short book on political economy "according to the principles of the Lausanne school" of Walras and his successor, Vilifredo Pareto (Laurent 1902). But, while applauding their mathematical turn, he wanted to associate it with something more practical than the abstract laws of exchange. He argued that a proper course in economics should involve four main parts: statistics, "economic facts," theory of financial operations, and theory of insurance. This did not entirely exclude the more abstruse theories of economists, for he included Walrasian pure theory within his category of economic facts. Mathematics could at least elevate economics to a proper science, he held, but only if it was closely linked with the study of empirical reality. This for him implied careful attention to statistics: Economics without statistics would be like physics without experiment. "I consider statistics not merely as an auxiliary to political economy," he wrote, "but as its fundamental base. It is its experimental part. Political economy can never become a true science, genuinely useful, until the day when its reasonings can conduct its premises to well-made observations, and when its conclusions can be verified by other appropriate observations."

Laurent can by no means be said to have achieved this. He did take the goal seriously enough to include a substantial discussion of statistics in his treatise on political economy and then in 1908 to publish another little book on statistics (Laurent 1908). It was not devoted to the collection of useful administrative numbers, but to probabilistic techniques for analyzing data and estimating precision that Laurent regarded as the foundation of statistics. He wanted to see economics and statistics become more like the science of the actuary. Actuaries had succeeded in making probability mathematics indispensable for insurance companies. He looked forward to a day when political economy could boast of a like practical value.

In his exchange of letters with Walras, he explained that an effective economics must be dynamic. To compare measurements across time, one needed a stable unit. His candidate for this was energy. He was deeply skeptical of Walras's ineffable "utility." Walras responded with as much patience as he could manage that energy was a valid economic measure only if it were equivalent to utility at the margin – which he doubted. He then admitted that dynamic formulas had no place in his theory. "In my desire to establish patiently the basis of a new science, I have so far more or less confined myself to the study of the phenomena of economic statics."[24] Laurent was not at all satisfied with this evasion, and subsequent correspondence did nothing to resolve their disagreement. And Laurent was his closest contact in the Institute of Actuaries. The stubborn indifference of the others to his work fed his paranoia. They had deliberately excluded him from their company. The Institute of Actuaries, he told its secretary, was controlled by the same malign influence that had ruined political economy in France. To others, he offered the opinion that there was no "profound knowledge" or intellectual vitality to be found there.[25]

The failure of Walras to win influence in the Circle of Actuaries, or to develop practical economic tools of his own, sheds much light on the relation of marginalist economics to practical calculation. This was largely an autonomous tradition, cultivated by administrators with problems to solve rather than by academic theorists. The highly abstract models from which Walras built a theory of general equilibrium contributed nothing to the decision processes of engineering administrators. The philosopher Renouvier, also a polytechnician, complained to Walras that the gap "between the science and the art of the engineer-economist (if you will permit me this expression)" is much greater than "that between the science and art of the engineer-mathematician."[26]

It was not only among the engineers in France that Walras's theory failed. He won few adherents, and almost no followers. This failure is naturally somewhat disconcerting to neoclassical economists, who view Walras's work as the discovery of an important scientific truth. Accordingly, there have been various attempts to explain his nonreception, with results that are on the whole convincing. Mathematical economics triumphed in Britain and the United States as part of the professionalization of the field, and its success is difficult to explain in other terms (Coats 1967; Stigler 1982).[27] The weak interest it stimulated in France is due in large part to the lack of opportunity for professionalizing economics in the French university system. Political economy was part of the training for civil servants and engineers. It won a place in the universities in the 1880s, but in the law faculty rather than among the sciences. It was, in short, taught mainly for administrative purposes. This was ruinous for Walras. Mathematical political economy was the sort of thing that only an academic economist could love.

Economics, physics, and mathematics

The pioneers of neoclassical economics depended heavily on mathematical physics for the theoretical structure they imposed on their discipline. The rediscovery of these interdisciplinary links is one of the most welcome developments in the recent historiography of economics (Kingsland 1985; Ingrao and Israel 1990). Drawing inspiration from statics and energy physics, economists built up a set of mathematical models as impressive and as demanding as are to be found in any natural science. Yet the story I have told here suggests a generally unenthusiastic reaction to deductive or mathematical economics on the part of physicists. William Whewell applied mathematical reasoning to Ricardo precisely in order to reveal his question-begging assumptions and to display his errors. Physicists and engineers in both Britain and France developed their own economic frameworks, which were thoroughly quantitative and yet quite alien to the mathematics of the early neoclassicals. One should not exaggerate the point. Certainly there were physicists, such as Vito Volterra, who applauded the research of the neoclassicals. But these were rare. More typical is Simon Newcomb, the U.S. astronomer and influential spokesman for "scientific method." Newcomb was an admirer of political economy and highly favorable to the project of making it more scientific. He was a teacher of Irving Fisher. He wrote an introductory treatise on political economy, which is full of mechanical analogies to economic processes. Yet, although the works of Walras and Jevons had been out for a decade, he did not even employ the calculus, the indispensable mathematical basis for marginal economics. He insisted that a fruitful economics must be closely linked with statistics. And he criticized Jevons, arguing that it was useless to make subjective feelings the foundation for economics. One must instead focus on actions, human behavior, which alone can be properly quantified (Newcomb 1885; Moyer 1992).

Walras was perpetually frustrated by this attitude. His desperate search for allies included appeals to such giants of theoretical physics as Poincaré. In Poincaré's philosophy he found inspiration, or rather justification. "One of the masters of modern science," he rhapsodized, "has concluded that masses are nothing but coefficients which are conveniently introduced into the calculations." Is it not the same, he continued, with the crucial economic concepts of utility and scarcity (*rareté*)? (in Mirowski and Cook 1990, 213). With this inspiration, Walras approached Poincaré for his approval. And he received in reply an ambivalent letter, favorable enough that Walras thereafter quoted from it on every possible occasion. But Poincaré was devoutly committed to applied mathematics and did not fail to notice that utility is a nonmeasurable magnitude. While it may legitimately be introduced as an arbitrary function in the premises, he allowed, it must disappear from the conclusions or these will be devoid of sense and interest. He also wondered about the premises of Walras's mathematics: It might be reasonable, as a first approximation, to regard men as completely self-interested, but the assumption of perfect foreknowledge "perhaps requires a certain reserve."[28] The mathematician Joseph Bertrand was less charitable. He found an essential contingency in the idealized economic marketplace, so that the price of a commodity would depend on the order of transactions and would not be determined by supply and demand curves. More generally, he concluded that the economic world was too slippery for mathematics and that practical knowledge in this domain is superior to mathematical abstractions (Bertrand 1883).[29]

Why were physicists so unreceptive to mathematical economics? It is, I think, wrong to suggest, as Mirowski has, that the marginalists were bumblers and that the physicists detected logical flaws to which the economists remained oblivious. The nub of the matter is that the physicists and engineers discussed here were unable to see the point of a purely theoretical economics. With very few exceptions, physicists and engineers took measurement to be more central than mathematical deductions to their discipline. They applied this standard even more stringently to economics than to physics because economics was not for most of them a pure research interest, but rather an aid to administrative decisions. Mathematical economics was detached from practice throughout the nineteenth century. So it was naturally more appealing to those who were indifferent to, or even opposed, centralized economic administration than to those who were looking to rationalize economic decisions. Whewell, who used mathematical reasoning mainly to undermine the policy prescriptions of Ricardian economics, appears exemplary from this standpoint. Toward the end of the century, Herbert S. Foxwell identified as one of the great merits of the new marginalist theory of Jevons and Marshall to have "made it henceforth practically impossible for the educated economist to mistake the limits of theory and practice or to repeat the confusions which brought the study into discredit and almost arrested its growth" (Foxwell 1886–7, 88). He even considered that mathematical and historical economics were allies in opposing the misapplication of theory. Mathematical economics, it seems, had the great virtue of demonstrable irrelevance, which was morally preferable to spurious relevance.

Few economic quantifiers, though, were content with demonstrated irrelevance. We should certainly not suppose that only engineers and physicists had the methodological or quantitative sophistication to apply economic numbers and calculations usefully to practice. By far the majority of practicing economists in the nineteenth century, and well into the twentieth century, were specialists in banking, commerce, or transport, not abstract theory.[30] And they too most often worked independently of abstract mathematical theory.[31]

This failure to make much use of theoretical economics in relation to practical and political questions applies even to the mathematical economists themselves. This is no surprise in relation to Walras, who found pure theory taxing enough and lost interest in the scientific study of practical economic issues. It is perhaps more surprising that we find almost nothing of the new marginalist economics in the policy writings of William Stanley Jevons and Alfred Marshall, each of whom nurtured a lifelong interest in economic affairs. The work of Jevons is especially revealing. He was an active and exceptionally sophisticated statistician. He was willing to make the effort of gathering up statistical information to learn about the causes of poverty or the conditions of trade. He even employed the mathematical theory of probability to infer fluctuations in prices, to demonstrate the exhaustion of coal reserves, and to detect an unwonted relationship between sunspot cycles and commercial crises (see Morgan 1989). Jevons was, in short, an avid quantifier. Yet one never encounters a word about marginal utility theory in his statistical writings. It may well be that in the long run he hoped to see statistics used in order to measure utility functions (Howey 1960).[32] But he never worked any of this out, never integrated his various economic interests. His own polymathy made him all the more conscious of a need for specialization. "The present chaotic state of Economics arises from the confusing together of several branches of knowledge. Subdivision is the remedy. We must distinguish the empirical element from the abstract theory, from the applied theory, and from the more detailed art of finance and administration. Thus will arise various sciences" (Jevons 1957).[33] From this standpoint, his much-advertised claim that economics should be mathematical because it is intrinsically quantitative rings hollow.

Marshall's economic thought is too complicated, too contradictory, to be divided into neat compartments. As is well known, he came to economics from Cambridge mathematics. By the time he published his *Principles of Economics* (1890), his mathematical enthusiasm was sufficiently diminished that he consigned all mathematics to a set of appendixes. Any mathematical result that cannot be expressed in natural language should be burned, he urged. And he preached that economics should follow biology rather than physics as its model.[34] This last point was honored mainly in the breach. And in place of mathematics he made extensive use of graphical representations. Those were idealized, never summaries of actual data.

The ambiguities of Marshall's economic style and pronouncements reflected a deep ambivalence of aims. He was a thoroughgoing professionalizer, earnestly committed to the creation of an effective economic discipline. At this he was remarkably successful. But he also wanted to educate potential businessmen in economics, to promote a chivalrous ethic that would reduce disparities of wealth without requiring heavy-handed bureaucratic intervention. He was not looking to train economic experts, but gentlemen, like those who led the Civil Service. He tried to make prominent political leaders feel welcome in the British Economic Association, provided they deferred to the academics on scientific issues. For their purposes, the cultivation of judgment was more important than the inculcation of quantitative skills. Probably his aims were incompatible (see Winch 1990). We need not worry much about the contradictions they generated. Neither professionalization nor the education of gentlemen called for much reliance on measurement or quantification, and as A. W. Coats remarks, Marshall feared the possible victory of empirical over "scientific and analytical" economics.[35] Occasional intrusions from the sphere of public discussion, such as debates about the gold standard, led Marshall to work for a time with statistics. Like Jevons and Francis Ysidro Edgeworth before him, he conducted this discussion without drawing on the mathematics of marginal economics (see Marshall 1926; Porter 1986).[36]

This remoteness from measurement and quantification was associated with a remoteness of neoclassical economic theory from practice, which, as I have argued, is one explanation for the indifference, even hostility, of many engineers and physicists to the new economics. To physicists in the era of Kelvin and Helmholtz, a theory was only meaningful if its terms were susceptible to measurement. Such views were especially common among those who were close to engineering and who wanted to see physics put to use. But it was also a moral ideal, an ideal of discipline, restraint, and humility. Just how severely it should be applied was contested in late-nineteenth-century physics. Kelvin, for example, criticized Maxwell for introducing terms into his theory that could not be measured. He argued, famously, that "when you can measure what you are speaking about and express it in numbers you know something about it; but when you cannot measure it in numbers, your knowledge is of a meagre and unsatisfactory kind" (quoted in Wise and Smith 1987, 327–8). Social scientists have often failed to realize that this was intended as an attack on the "nihilism" of theory. Indeed, Norton Wise and Crosbie Smith (1987) have urged that the willingness of Maxwell's school to relax this practical imperative, to allow a greater autonomy for mathematical theory, reflected the increasing professionalization of physics at the end of the nineteenth century in Britain. This suggests a parallel with the development of neoclassical economics. But the mathematical economists took their hypertrophy of theory much further than the Maxwellians. Maxwell and his followers tried always to come back to experimental predictions, matters of potential measurement, at the terminus of any theoretical excursion. Physicists were widely agreed that the proof of theory was in measurement.[37]

While neoclassical economists may have derived much of their mathematical theory using analogies with physics, they were very far from accepting the prevailing standards of physics as a practice. That practice was and is strongly associated with experimental quantification, and by no means first of all with mathematical theory. It would be invidious and seriously misleading to suggest that dissenters from the neoclassical approach have more nearly succeeded in following the pattern of physics. Clearly, though, it was the early econometricians who took most seriously the problem of measurement.

A fine example is Wesley Mitchell, head of the National Bureau of Economic Research and an active contributor to economic policy under

Herbert Hoover. Mitchell, somewhat audaciously, referred to neoclassical theory as "qualitative" and called for a major infusion of quantification into economics. By this he meant statistical measurement:

> Economists who practice quantitative analysis are likely to be chary of deserting the firm ground of measurable phenomena for excursions into the subjective. . . . If my forecast is valid, our whole apparatus of reasoning on the basis of utilities and disutilities, or motives, or choices, in the individual economy, will drop out of sight in the work of the quantitative analysts, going the way of the static state. (1925, 4)[38]

He complained that the "qualitative" theory of Jevons and Marshall "plays so small a role in our work as specialists in public finance and banking, in accountancy and transportation, in business cycles, marketing, and labor problems" (5). It poses the wrong issues and asks questions that cannot be addressed with quantitative methods. Hence, economic theory must "reformulate its problems" (6) and "change not merely its complexion, but also its content" (3).

Mitchell did not fail to allude to the physical sciences as a model for economic research. Scientific knowledge comes from the laboratory, he declared. Social statistics provide the laboratory of the economist. In physics, "we rely, and with success, upon quantitative analysis to point the way; and we advance because we are constantly improving and applying such analysis" (1919, quoted in Alchon 1985). It is obvious to us, as it was to Mitchell, that official statistical collections are not the same as laboratory results. They lack the crucial element of experimental control, which permits natural scientists to proceed not mainly by trying to describe a world that exists independently of their activity, but rather by creating a controlled microworld of artificial technologies in which their theories are valid.[39] Still, as Mary Morgan (1989) points out, econometrics succeeded in appealing to physicists, especially in the heady days of the early 1930s when the Econometric Society was founded. Nancy Cartwright (1989) has argued that inference from data by econometricians is in important ways strikingly similar to that by quantum physicists.

The ethos of neoclassical economic theory, in contrast, seems alien to that of physics, even if much of its mathematics did come from statics and thermodynamics. To be sure, physical theory too has in this century become increasingly autonomous from experiment and measurement. But to find a form of theory so detached from practice and data as is characteristic of neoclassical economics, we must look to mathematics rather than physics. Margaret Schabas (1989) points to the mathematical logic of Augustus DeMorgan and George Boole as the background to Jevons's mathematization of economics (see also Black 1973). Logic, though not yet integral to the mathematics discipline, was rapidly becoming so, as mathematics moved increasingly from realism to formalism. The incomprehension that Walras met so often reflected similar tendencies. He complained that too many readers expected mathematical economics to mean numbers and formulas, when he was using instead the theory of functions. As John Maynard Keynes remarked in 1921: "The old assumptions, that all quantity is numerical and that all quantitative characteristics are additive, can no longer be sustained. Mathematical reasoning now appears as an aid in its symbolic rather than its numerical character." And then he added, "I . . . have not the same lively hope as Condorcet, or even Edgeworth, éclairer les sciences morales et politiques par le flambeau de l'Algèbre."[40] This tendency to identify mathematics with formalism rather than formulas became all the more dominant in the 1930s and 1940s, when general equilibrium theory was established as the most prestigious research field in the economic discipline. The migration of mathematicians into economics was crucial for the establishment of this new research style (Ingrao and Israel 1990).

As Herbert Mehrtens argues, modernism in mathematics meant precisely a retreat from the world of space and time, flesh and blood. The paradise of mathematicians, identified already by Gauss in 1802, was a place in which *Geist* was no longer confined by space, nor chained to a ponderous, suffering body. David Hilbert, the Göttingen mathematician who gave modernism its authoritative formulation, was characteristically indifferent to the debate over Euclidean and non-Euclidean geometries. Geometry is not the mathematics of space; it is self-subsistent. It proceeds by positing axioms and deriving theorems, and if the results lead to no contradictions, the system is by definition mathematically true. Mathematics does not describe a world, but posits one. It is a language of symbols that refers to nothing outside itself. "The new language of mathematics does not need to be made certain in relation to an exterior reality, because it makes itself certain through its own work" (Mehrtens 1990, 68).[41]

Mehrtens explains the modernist turn in mathematics partly in terms of its professionalization, which permitted far more isolation from the problems of the sciences than had been possible previously. This disciplinary autonomy, he adds, is part of what makes mathematics exemplary for modernism generally. In economics, the mathematical turn served important defensive purposes as well. The mathematization of economics was key to its professionalization. It provided disciplinary identity and a standard of competence that discredited outsiders (Stigler 1982; see also Maloney 1985). It lifted economic discourse decisively out of the domain of public discussion, eliminating the threat that the pronouncements of economists would seem to be no more than a slightly obscure version of common sense. To the mere citizen, the obscurity of economic theory would henceforth be complete.

Wesley Mitchell was perplexed at this dedication of economists to marginal theory. Other economists, he noted, had defended economic mathematics as essential shared knowledge, which could hold the discipline together in the face of rampant specialization. But why, he asked, should economists tolerate a core of knowledge that is so useless in regard to every part of the periphery? Neoclassical theory can hardly succeed even at this when it plays so little role in any variety of economic practice (Mitchell 1925). Mitchell, though, failed to anticipate that neoclassical theory might *become* the dominant specialty, and thus, like Hilbert's mathematics, no longer depend for its perpetuation on any ability to describe the world. Further, its very dearth of content was for some purposes an advantage. One is reminded of the role of abstract art in fin-de-siècle Vienna, which the authorities approved for monumental buildings precisely because it lacked content and historical meanings. Any art with real content was unacceptably polarizing in a fractured, multinational state (Schorske 1980; Silverman 1989). Mathematical neoclassicism, while presupposing a broadly liberal individualist basis for economic order,[42] was almost neutral with respect to the narrower but more numerous issues of policy that must lead to endemic conflict in a genuinely political economy. The adoption of mathematical foundations served not only to translate emotion-charged issues into a technical language, but even more to create a basis for agreement that could be viewed as deeper than mere applications. A few splinter groups, most notably the Marxists, have refused to accept this narrowing and evasion. But from the standpoint of the dominant school, such dissenters are negligible. The abstract formalism of neoclassical mathematics has served admirably in preserving the unity of the economics discipline.

Conclusion

Mathematics is never neutral, never simply a technically superior way of accomplishing what practitioners of the social and natural disciplines are already doing. Its triumph in economics was associated with a vast change in the practices of that field. Alternative uses of mathematics and quantification have had sharply variant implications. Quantification and statistics were associated primarily with the management of economic affairs, often though not always in the public domain. Political economy was long a storehouse of arguments for not attempting to disrupt the spontaneous workings of the market. Some of the earliest mathematizers of economics, notably Whewell and Jenkin, aimed to neutralize this ideological message by showing that its arguments against public action rested on doubtful assumptions or even errors of reasoning. Mathematics has tended to render theory more nearly neutral, or at least to put more space between the economic discipline and the hubbub of political and commercial affairs.

Neoclassical theory has remained aloof not only from controverted issues, but also from the problems of practical management. For similar reasons, the pioneering mathematical economists established very little contact between neoclassical theory and statistics or measurement. Quantification and mathematization, in short, have been very much isolated from one another. Though the political conflicts between theory and practical quantification have been alleviated, differences involving aims and methods have persisted. The mathematization of theory has done nothing to harmonize it with statistical numbers. Whewell hoped that a demonstration of the irrelevance of theory would drive it from the field, leaving room for empirical and statistical study. Instead, the relative neutrality of mathematical theory has made it all the more satisfactory as a basis of professional economic discourse.

In this respect, as Donald McCloskey (1991) argues, modernist economics shares a good deal with modernist mathematics.[43] Its practitioners opened a wide rift between mathematical theory and measurement long before physicists or mathematicians could boast of anything comparable. Nineteenth-century physicists and engineers who had occasion to engage themselves with economic questions and to assess the merits of mathematical economic theory rarely saw eye to eye with the economists. Their economics tended strongly to the quantifying, managerial form. Many reacted to neoclassical theory with incomprehension. Sometimes, as in Laurent's exchange with Walras, they simply misunderstood it. When they misunderstood, though, it was in part because they had been brought up to think even less of theory without measurement than of measurement without theory.

The scientific ideal is often taken, not least by economists, as monolithic. It helps greatly to support this illusion when the broad domain of quantitative reasoning, extending from counting and measuring to mathematical deduction, is understood as a single, unified body of conceptions and techniques. The history of modern economics shows, as strikingly as any field, that this is a misconception. Imitating natural science is anything but an unproblematic endeavor.

Notes

The research for this chapter was generously supported by the Earhart Foundation, the Thomas Jefferson Memorial Foundation, and the John Simon Guggenheim Memorial Foundation. I thank Bruce Caldwell, Lorraine Daston, Neil de Marchi, and Philip Mirowski for helpful comments.

1. It is an exaggeration mainly because new historical and institutional approaches provided for several decades a strong alternative to the mathematics of the neoclassicals, in both Britain and the United States. Finally in the mid-twentieth century, these were relegated to the fringes of the discipline. See Ross (1991) and Coats (1988).
2. Gordon (1973) argues that even Marshall was unable to provide a persuasive model of a biological style of economics, though Camille Limoges and Claude Ménard, Chapter 13, this volume, show how his mechanical picture was framed by biological analogies. I have discussed in broad terms the diverse ways in which social thought has been patterned after the natural sciences in Porter (1990).
3. A phenomenon not limited to physics. A wide literature now touches on these issues from various standpoints; see Cannon (1978), Porter (1986), Smith and Wise (1989), Hacking (1990), Gooday (1990), Olesko (1991), and Wise (1994). For the eighteenth century, see Frängsmyr et al. (1990).
4. Perhaps the most extreme case of this is the reaction to Thomas Young's wave theory of light, admittedly by a liberal critic rather than a specialist in physical science: "It is difficult to argue with an author whose mind is filled with a medium of so fickle and vibratory a nature. . . . A mere theory . . . is the unmanly and unfruitful pleasure of a boyish and prurient imagination, or the gratification of a corrupted and depraved appetite" (Brougham 1803, 452).
5. See Say (1803); also Ménard (1980). Say provided an important model of systematic political economy for Ricardo and James Mill; see Halévy (1955). Henderson (1985, 407) mentions the use of a language of "disturbing causes" by classical political economists in England to fend off the statisticians.
6. The statisticians generally favored particular reforms, not systematic state intervention. See Coleman (1982) and Cullen (1975).
7. Whewell to Jones, July 23, 1831, in Todhunter (1876, 2:353). Whewell's negative intentions are also clear from two letters of 1829 to Jones, quoted in Henderson (1990, 16).
8. He investigated this conclusion mathematically, then assessed it against the empirical evidence supplied by Jones, particularly in Whewell (1862), lecture 5.
9. On Jenkin, see Wise (1994).

10. Comparative measurements of human and machine labor power go back to the beginning of the eighteenth century, especially in France; see Lindqvist (1990).
11. On Le Play's differences from the statisticians, see Hacking (1990, ch. 16).
12. There was a continuous though relatively inconspicuous tradition of energeticist economics dating from about the 1870s. For the most part it was deliberately subversive of mainstream economics. See Juan Martinez-Alier (1987).
13. This was originally published in 1887 in *Le génie civil.* For a modern discussion of this article, see Hébert (1974) and especially Desrosières (1986). By this time, graphic methods came naturally to engineers, at least in France; see Lalanne (1846).
14. See also Porter (1991).
15. For example, Coriolis (1835) and Reynaud (1842). Reynaud, however, concluded that the formulas connecting grades with costs of operation were too imperfect to be relied upon and that informal techniques of quantification were best.
16. Walras to Cournot, March 20, 1874, letter 253 in Jaffé (1965); hereafter WC.
17. Walras to Ferry, March 11, 1878, letter 403 in WC. See also letter 444 to Ferry. One must recall that Walras was looking to Ferry to find him an appointment in a French university.
18. Walras to Piccard, October 25, 1873, letter 239 in WC.
19. Walras to Jules du Mesnil-Marigny, December 23, 1862, letter 81 in WC.
20. Walras to Hirsch, January 18, 1881, letter 487 in WC.
21. Letters from Hippolyte Charlon, September 22, 1873, and to Charlon, October 15, 1873, numbers 234 and 236 in WC. On the Circle of Actuaries, see Zylberberg (1990).
22. Hippolyte Charlon to Walras, January 30, 1876, letter 347 in WC.
23. WC, vol. 3. Such dissatisfaction was not unique to Laurent. See, e.g., Geddes (1883–4, 950–63).
24. Laurent to Walras, November 29, 1898, and reply December 3, 1898, letters 1374 and 1377 in WC.
25. See three letters from Walras to Léon Marie from the end of 1899, numbers 1430, 1433, and 1434, and letter 1409 to Georges Renard, probably sent July 1899, in WC.
26. Renouvier to Walras, 18 May 1874, letter 274 in WC.
27. See, however, Schabas (1991), who argues against the identification of mathematical with professional economics.
28. Poincaré to Walras, 1901, letter 1496 in WC.
29. The best study of the reaction of mathematicians to Walras and to mathematical economics is Ingrao and Israel (1990). Central to their account is the skepticism of mathematicians and physicists because of doubts about economic mathematics unsupported by measurement.
30. See the important new work by Alborn (1991) and Klein (in press).

31. Hutchison (1969) remarks that, increasingly after 1870, economists' policy recommendations had at most a tenuous base in systematic theory. This is not to say that they were purely empirical. Certainly economists valued, for example, the keen awareness of unintended consequences taught by Adam Smith.
32. For a fuller discussion of the relations between utility and demand in Jevons's work, see Bostaph and Shieh (1987).
33. Maloney (1985) calls Jevons a "polymathic specialist."
34. Numerous remarks by Marshall on the dangers of excessive mathematization, the need for economic biology, and the like can be found in Pigou (1925). See also Marshall (1920).
35. Coats (1967, 713), quoting from a letter by Marshall to J. N. Keynes.
36. Hutchison (1953, 91) remarks that when dealing with policy questions, Marshall relied not on mathematical solutions to maximization problems, but on detailed factual study.
37. There is now a wide literature on measurement in nineteenth-century physics. Here I am relying mainly on Smith and Wise (1989). See also Hunt (1987).
38. I thank Mary Morgan for calling my attention to this article. Neoclassical theory may still be less important for economic applications than is, e.g., mathematical statistics. Tribe (1991).
39. See Hacking (1983); also Latour (1987), who points out that results of the laboratory do not remain confined to a microworld, but instead spread out along networks and remake the larger world. I have discussed the problem of networks and standardization in relation to statistics and the applied social sciences (Porter 1992a; see also 1992b).
40. (To illuminate the moral and political sciences with the lamp of algebra.) From the *Treatise on Probability,* quoted in Skidelsky (1983, 223).
41. My discussion draws on my review of Mehrtens (1990) in Porter (1992c).
42. The political consequences of these presuppositions of economic analysis are emphasized by Martin (1978).
43. The similarity of economics to mathematics has also been argued by Rosenberg (1992, ch. 8).

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PART III

Uneasy boundaries between man and machine