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# The Fallacy of Misplaced Concreteness in Economics and Other Disciplines

In the modern university, knowledge is organized into academic disciplines. There are clear norms establishing what such disciplines must be. These provide criteria that divide subject matter among the disciplines and establish goals for the internal structure of each one. This organization of knowledge has been brilliantly productive, but it also has built-in limitations and dangers, especially the danger of committing what Alfred North Whitehead called "the fallacy of misplaced concreteness." This fallacy flourishes because the disciplinary organization of knowledge requires a high level of abstraction; and the more successfully a discipline fulfills the criteria established for it, the higher is the level of abstraction involved. Inevitably, many practitioners of successful disciplines, socialized to think in these abstractions, apply their conclusions to the real world without recognizing the degree of abstraction involved.

Outside the physical sciences no field of study has more fully achieved the ideal form of academic discipline than economics. Precisely because of its success, it has been particularly liable to commission of the fallacy of misplaced concreteness. This chapter emphasizes both the success of economics in realizing the ideal form of the academic discipline and the inevitable limitations that accompany this achievement. It gives some egregious examples of the fallacy of misplaced concreteness in prestigious economic writing. The following chapters illustrate the pervasive effects of the fallacy in more fundamental ways.

Much of the thought of the modern world has been shaped by admiration of the brilliant success of physics in the seventeenth and eighteenth centuries. Physicists developed a conceptual model of nature from which a vast range of predictions followed. These were tested and some were found to be correct. Other empirical results required that

concepts and theories be altered. Large bodies of mathematics that had been developed for purely theoretical purposes in earlier centuries turned out to be applicable to the working out of the explanatory and predictive power of the world model.

Physics was empirical in two very important senses. First, the hypotheses that contributed to the world model were suggested by observation and experiment. Second, the validity of the model was tested by drawing forth its implications and examining their correspondence to what could be observed. What distinguished this science from other investigations of nature, however, was not its empirical element but its formal and deductive aspect. Aristotle had encouraged the assemblage of empirical data and its classification. But he had not anticipated the possibility of elaborate deductive systems. The study of living organisms long continued to follow Aristotelian lines more than Newtonian ones, but the ideal of science was established as the discovery of laws from which facts could be deduced.

Of course, in a strictly empirical sense, the observed facts do not directly correspond with the laws. For example, Galileo's famous proof that the speed with which bodies fall to the earth is not affected by their weight does not correspond with experience. Everyone knows that a rock falls faster than a leaf. What is shown is that in a vacuum, the speed would be the same. Even here further qualifications are needed. The moon does not fall to the earth at all in an empirical sense. The law applies empirically only to objects that are stationary with respect to the earth, or in the same relative motion. Further, the law applies only to objects that are within the gravitational field of the earth and that are unaffected by other gravitational fields.

All of this was well understood by the early physicists. To explain empirical phenomena, it was necessary to develop models that simplified reality in order to bring out fundamental features. The right abstractions embodied in simplified models made possible far more powerful analyses and predictions.

The difference between the predictions of the simplified model and the actual behavior of objects allows for the study of other forces. For example, consideration of the fact that the moon does not fall to the earth despite the gravitational force of the earth upon it calls attention to the tendency of any moving object to continue in a straight line. The actual movement of the moon results from the joint operation of two principles—the gravitational pull of the earth and the moon's own momentum. Any deviation of the movement of the moon from what is predicted by these two principles, however tiny, will require search for whatever additional forces are operative.



Admiration for the success of physics has led to two somewhat divergent ideals for the organization of knowledge. One ideal is to attain a unified science in which it would be shown that every aspect of nature can ultimately be explained from the laws of physics. This would mean that chemistry would become a subdivision of physics, and biology a subdivision of chemistry. Some would aim to display human social phenomena and psychology as a branch of biology, thus ultimately as a part of the world machine.

Although this vision continues to play an important role in the Western psyche, it has thus far proved impossible to make much progress in the study of living things by deducing their behavior from the laws of physics. Even chemistry exhibits far too much novelty by combination to be reduced to physics. For practical purposes it must be studied in its own terms. This applies a fortiori to biological and social phenomena. Accordingly, the way in which the norm of science informed by physics has actually functioned is by establishing the several sciences in relative autonomy, with each aiming in its own field to achieve a form resembling that of physics, one in which laws or models are found from which facts can be predicted. Yet this aim remains unrealized even in the other natural sciences. Even in chemistry, there are numerous brute facts that are underivable from any small set of premises. Nevertheless the deductive ideal guides theoretical work.

Despite the prestige of physics there has been resistance to this model in some areas, especially in the study of human beings. For the most part, at least until recently, history was held to be fundamentally different from nature. The question for historians was what in fact took place. No attempt should be made to deduce what occurred from laws of history or unchanging models. Other students of history have stressed that the essential task is to understand rather than explain or predict. They have concentrated on hermeneutics as their special method.

In the nineteenth century, the organization of knowledge that took place was shaped by the second type of influence of physics—that is, the division into autonomous sciences—combined with the power and prestige of the different methods used to study human phenomena. German universities provided the leadership in organizing knowledge into *Wissenschaften*. *Wissenschaft* is often translated as "science," but because "science" in English strongly favors the model of physics against that of history, it is better to translate it as "discipline." Thus knowledge in Ger-

many was organized into two types of disciplines, the disciplines of nature, modeled on physics, and the disciplines of the human mind or spirit.

The study of human social phenomena never fit comfortably into either type of discipline, so that social studies have displayed the tension between them. They have humanistic elements and also elements that relate them to the natural sciences. In the United States, however, there has been a strong tendency for them to think of themselves as social "sciences."

One way of stating the underlying difference of the scientific and humanistic disciplines is that the former focus on what is universal and necessary, the latter on what is particular and contingent. Of course, the universality of the sciences cannot be absolute in most cases. Classical physics could view the structures of nature as absolute, but biology could study only what was universal to living things, and the social sciences could attend at most to what was universal to human beings. More often, the social sciences studied what is universal to particular types of society. Nevertheless, the quest for models or laws of general applicability rather than the effort to identify and understand the contingent features of reality shaped the methods of those social studies that most emphasized their status as social sciences.

## The Place of Economics

The work of Adam Smith and the other early British economists had a strong historical and humanistic component, but the movement of economics initiated by them and especially accented by David Ricardo has been toward being a science. In part it has sought to find models and laws applicable to all human beings, but primarily it has focused on the laws governing modern industrial economy. It is sometimes not as careful as one would wish in clarifying the limits of the type of society to which these laws are applicable.

The choice of economists to focus on the scientific rather than the historical study of the economy was fateful. On the one side, it has made possible the development of powerful analytical tools and predictive devices. On the other side, it has led to serious distortions. These were inevitable once the choice was made.

When physics arose, it assumed that what it studied had not changed from its creation. The specific configurations of matter had of course changed, but the laws governing them were assumed to be immutable.



This assumption was quite appropriate to the data and paved the way for enormous progress. Today, physicists know that this assumption is not entirely true. It is now generally held that the laws of physics came into being with the structures of nature that evolved in the Big Bang. The crucial determinations may have taken place in a fraction of a second. But though this means that the laws of nature are not eternal, that at some point they may come to an end, there is every indication that they are very stable throughout the whole intermediate course of events. For physicists, in most of their work, to abstract from evolutionary change in their subject matter does very little harm.

Nevertheless, the fact that physical reality and the laws that describe it are not immutable, does call attention to the subtle error that has too often entered the notion of "law." It shows that laws are correlative to the things whose behavior they describe. There could be no laws of electricity until there were electromagnetic fields. In this sense all laws are contingent. The necessity that lies in the law is grounded in the fact that certain patterns necessarily characterize entities of a particular sort. Entities that did not "obey" the laws of electromagnetism would not be electromagnetic fields.

This recognition that law and subject matter are correlative is not practically important when what is studied is for practical purposes unchanging. A biologist interested only in how members of a species now behave can ignore it. But when biologists became interested in how species arise, and how they change, then the limits of the laws became clear. Those who want to find more fundamental laws seek the laws of evolution—that is, the universal characteristics of evolutionary change. But even the laws of evolutionary change themselves change with the types of organisms that are evolving.

In the United States, those who established the several branches of the study of human society as sciences modeled their understanding of science more on physics than on evolutionary biology. That is to say, they concentrated on the laws exemplified by the societies they studied rather than on the way the behavior expressed in these laws came into being or changed through time into other forms. This means that the laws that are discovered are laws "governing" specific types of society and become unapplicable as those types of society give way to others. But there is danger that the habit of attending to laws will lead the practitioners of the disciplines to attempt to apply them beyond their limited sphere of relevance.

Economists know that the structures they study are not eternal and

that most of them are not coterminous with human existence in general. Adam Smith begins by contrasting the system he studies, that in which the division of labor is far advanced, with earlier forms of human society in which there was little division of labor. He knew that the industrial developments that interested him in England were virtually absent in Poland. Obviously what he studied were historically contingent phenomena. Furthermore, he was no mean historian.

The early economists theorized both as to how the industrial system arose and as to where it was heading. The classical economists saw a temporary phase of growth that must culminate in a new steady state economy. Hence, even when they discerned models and laws that were operative in the economic events of their time, they recognized that at some point in the future different models and laws would function. In short, they knew that the laws "governing" the economic system change as the system changes.

The evolutionary or historical character of the economy has never been denied or wholly ignored. Hegel and Marx gave it rich attention in the nineteenth century. Alfred Marshall, the founder of neoclassical economics, was highly sensitive to the historical character of the actual economy. Nevertheless, economists on the whole wanted economics to become increasingly scientific, and their idea of science was based on physics rather than on evolutionary biology. That meant that economics had to focus on formulating models and finding laws "governing" present economic behavior rather than seeking laws "governing" the changes of economic systems or asking about contingent historical matters. As a result, when useful models have been found and when hypotheses have proved successful, they are treated as analogous to the models and hypotheses of the physicist. Their limitation to particular historical conditions is neglected. Leon Walras, in his *Elements of Pure Economics,* undertook "to do for economics what Newton had done two centuries earlier for celestial mechanics" (1954; Maital 1982, p. 15). In the twentieth century, economics has followed Walras. Milton Friedman notes of economists that "we curtsy to Marshall, but we walk with Walras" (1949, p. 489).

The choice to follow physics in this way has proved partially successful. It has made economics by far the most theoretical and rigorous of the social sciences. It has allowed economics to guide and predict as no other social science has been able to do, at least during certain historical periods. But it has exacted its price by accentuating the problems that follow from having elected to be a science that abstracts from



the deep changes in that which it studies. If it had followed Marshall (1925, p. 14), who claimed that "the Mecca of the economist lies in economic biology rather than in economic dynamics," it would have observed these changes and adapted to them. Having followed Walras, the observation of facts has been subordinated to the concerns of theories. Those facts not correlated to the theories have been largely ignored.

The decision to follow physics was the decision to mathematize. Mathematics can work only with what can be formalized. In economics, this has meant, in practice, what can be measured. Hence the aim of mathematization biases economics toward aspects of its subject matter that can be measured. In *The Economics of Education,* John Vaizey recognizes this with unusual candor: "I must confess to an instinctive conviction that what cannot be measured may not exist" (1962, p. 14). The "instinctive conviction" is more likely to be the result of socialization in the discipline, but in any case Vaizey's awareness of his bias led him to deal with nonquantifiable aspects of education. There have been others who have declined to do so. Prestige increasingly is associated with mathematical sophistication and less with what light may be thrown on what is actually going on.

Not all mathematicians have welcomed the mathematization of economics. Witness Norbert Weiner's biting comment: "Just as primitive peoples adopt the Western mode of denationalized clothing and of parliamentarism out of a vague feeling that these magic rites and vestments will at once put them abreast of modern culture and technique, so the economists have developed the habit of dressing up their rather imprecise ideas in the language of the infinitesimal calculus. . . . Any pretense of applying precise formulae is a sham and a waste of time" (Weiner 1964, p. 89).

Nor did earlier economists unanimously accept the mathematization of their discipline. Witness the challenge of J. E. Cairnes to the new mathematical methods championed by his friend Jevons: "So far as I can see, economic truths are not discoverable through the instrumentality of mathematics. If this view be unsound, there is at hand an easy means of refutation—the production of an economic truth, not before known, which has been thus arrived at; but I am not aware that up to the present any such evidence has been furnished of the efficacy of the mathematical method" (Cairnes 1875, p. vi).

A century later there are certainly some economic insights arrived at with the help of mathematics. For the most part, however, mathematics has simply been used to restate more rigorously economic truths ar-

rived at by other more intuitive modes of thinking. Rigor is not to be despised, but neither should it become a fetish, as it in fact has become in academic economics. There are probably no important theoretical or policy debates that have been resolved by econometrics, which was supposed to provide the empirical test for resolving all disagreements. What happened, however, was that each side of any debate developed its own econometricians (or "econometricians" as some critics have called them). Historically the attempt to use mathematics to further economic discovery was certainly justified. But disappointing results must be admitted. Even mathematical economists such as Nicholas Georgescu-Roegen and Wassily Leontieff believe that further effort toward mathematization is counterproductive.

Leontieff, a Nobel Laureate in economics, has been sufficiently distressed by this tendency to write an open letter to *Science*. In this letter he declares that the king is naked, but that few in academic economics recognize this, and that those who do recognize it do not dare to speak up: "Page after page of professional economic journals are filled with mathematics formulas leading the reader from sets of more or less plausible but entirely arbitrary assumptions to precisely stated but irrelevant theoretical conclusions . . . econometricians fit algebraic functions of all possible shapes to essentially the same sets of data without being able to advance, in any perceptible way, a systematic understanding of the structure and operations of a real economic system" (Leontieff 1982, pp. 104–105).

## The Limitations of Academic Disciplines

Some of the limitations and failures of economics result from its modeling itself on the discipline of physics rather than on that of biology or of history. But for economics to have defined itself as a subdivision of biology or history would have had other limitations. The problem lies with the disciplinary organization of knowledge that so dominates the modern university and through it the thinking of the contemporary world. It is this organization of knowledge that forces economists to choose between scientific and historical understanding of what they are doing.

Adam Smith lived and thought before the effort had been made to organize all knowledge into disciplines. He saw the economy as part of the whole of human activity, and he investigated it historically and empirically. Out of these investigations he formulated generalizations that have proved extraordinarily illuminating, and he drew conclusions from them.



As a *discipline* economics must differ from the work of Adam Smith in two ways. First it must identify its subject matter in more separation from the rest of reality. Second, it must articulate the method that it finds most appropriate to the subject matter, the method that will then define it as a discipline. These needs are not dictated by advantages in understanding the actual economy. They are dictated by the disciplinary organization of knowledge.

This organization requires that each discipline have a subject matter clearly distinguished from the others. That necessitates a drawing of boundaries not present for the early economists. The definition of a discipline also requires methodological self-consciousness, and the method must be one that not only illumines the separated subject matter, but further selects the features of the subject matter that will be noticed and treated. In addition, it limits the number of people who may call themselves economists and who receive a salary as an economist.

The early economists studied the economy as an aspect of the whole of social life. Its interconnections with other aspects of that life were as important as its own inner principles. For example, many of the debates among economists were shaped by concern for the relation of economic developments to population. But from economics as a discipline this concern and others like it must be expunged. The study of population belongs to demography. The debates of the early economists can occur now only in interdisciplinary contexts, and the disciplinary organization of knowledge makes these peripheral. Within economics as an academic discipline the complexities of the impact of economic growth on population, and of population growth on the economy, are largely ignored. Again, this is not because it has been shown that these relationships are not important. It is because the disciplinary organization of knowledge requires a separate subject matter for economics, for demography, for sociology, and so on.

This whole process of replacing the concrete with the abstract is encouraged in one further way. This organization leads to the social organization of the university into departments. In addition, the most important relations of the members of a department with persons outside it are not with members of other departments within the university, but with other practitioners of the same discipline in other universities. The primary loyalty of university professors is likely to be to the guilds and to the promotion of their discipline rather than to their particular university or to their students. Indeed, there are many for whom the advance of their discipline is the major source of meaning, the organizing center of their lives, their deepest commitment. The discipline becomes

their god. We call this "disciplinolatry." That disciplinolatry is far advanced in economics was implicitly acknowledged by Paul Samuelson in his presidential address to the American Economic Association: "In the long run, the economic scholar works for the only coin worth having—our own applause" (1962, p. 18).

Commitment to the discipline and its future leads to keen interest in recruiting students who will major in it. The tendency is that courses taught for the general student body function more to attract majors and start them on their way than to facilitate the understanding of the subject matter by outsiders. In any case, concentrated attention is paid to socializing students into the discipline and to preparing leaders for the future through graduate programs.

Once socialized into the guild, relations with other members of the guild are far more comfortable and satisfying than those with outsiders. There is a wide range of common assumptions that express themselves also in shared values. In this way the external threat to these assumptions and values is minimized. The result is, of course, that what has come to be assumed within the discipline appears self-evident and in no need of critical analysis. New generations build on the work of earlier ones without asking whether these earlier achievements are truly relevant to the new situation. Indeed, the study of the *newness* of the new situation is not encouraged.

A recent study of graduate education in economics concludes that "graduate economics education is succeeding in narrowing students' interests." According to the study's survey of the perceived relevance of other fields to economics, physics scored the lowest, and ecology or any other biological science was not even listed among the fields to be ranked (Colander and Klamer 1987). Small wonder that economic models sometimes conflict with biophysical realities.

Those students of the discipline who do raise radical questions about it are rarely appreciated. Indeed, they find jobs scarce and encounter difficulties in getting their work published. They are likely to be denied a place on the program of guild meetings and to be made to feel unwelcome there. In short, they are ostracized. The discipline can proceed in a cumulative fashion increasingly canalized by what has been accepted in the past, currently referred to as "the mainstream." The abstractions that are universally accepted are taken as the reality.

This procedure is far advanced in economics. Leontieff's letter to *Science* protests about this, too. He expects the sterile scholasticism to which he objects to persist as long as the tenured members of leading



economics departments continue to exercise, largely through journal editorships, tight control over training, promotion, tenure, and research grants. The methods employed to maintain intellectual "discipline" within the academic descipline of economics can, he says, "occasionally remind one of the those employed by the Marines to maintain discipline on Parris Island" (Leontieff 1982). What Leontieff does not seem to recognize is that the problem is not so much an abuse of power by biased senior faculty as an outgrowth of the disciplinary organization of knowledge itself. The likelihood that the successors of the present generation of leaders will have a broader view of economics and of its responsibility to society is slight, unless there is a conscious criticism of the forces that have pressed the discipline of economics to focus on these abstractions.[1]

## The Fallacy of Misplaced Concreteness

The problem with economics is that it has succeeded all too well by the standards of the academic world. It is a successful discipline, and it has succeeded much better than any other social study in becoming a deductive science. These successes have involved a high level of abstraction, yet the whole ethos of the university in general, and of the department of economics in particular, discourages the full realization of the extent of the abstracting that has gone on. The result is that conclusions are drawn about the real world by deduction from abstractions with little awareness of the danger involved.

Alfred North Whitehead noted that this tendency began early with economics.

> It is very arguable that the science of political economy, as studied in its first period after the death of Adam Smith (1790), did more harm than good. It destroyed many economic fallacies, and taught how to think about the economic

1. Since one of the authors of this book is a theologian, it may be well to make explicit that the problem with theology as an academic discipline is similar to that with economics. Cornel West contrasts a theological effort of which he approves with academic theology, as follows. "Shunning the narrow confines of the intellectual division of labor in academic institutions, DEI [Departmento Ecumenico de Investigaciones, in San Jose, Costa Rica] rejects the compartmentalized disciplines of our bureaucratized seminaries and divinity schools. Instead DEI promotes and encourages theological reflection that traverses the field of political economy, biblical studies, social theory, church history, and social ethics. In this way, DEI reveals the intellectual impoverishment of academic theologies that enact ostrichlike exercises in highly specialized sand—with little view to the pressing problems confronting ordinary people in our present period of crisis" (Hinkelammert 1986, p. v).

revolution then in progress. But it riveted on men a certain set of abstractions which were disastrous in their effect on modern mentality. It dehumanized industry. This is only one example of a general danger inherent in modern science. Its methodological procedure is exclusive and intolerant, and rightly so. It fixes attention on a definite group of abstractions, neglects everything else, and elicits every scrap of information and theory which is relevant to what it has retained. The method is triumphant provided the abstractions are judicious. But, however triumphant, the triumph is within limits. The neglect of these limits leads to disastrous oversights . . . [Whitehead 1925, p. 200]

These tendencies in economics were recognized at the time. The great Swiss economist, Sismondi, observed the error in the early nineteenth century.

> The new English economists are quite obscure and can be understood only with great effort because our mind is opposed to making the abstractions demanded on us. This repugnance is in itself a warning that we are turning away from the truth when, in moral science where everything is connected, we endeavor to isolate a principle and to see nothing but that principle. . . . Humanity should be on guard against all generalization of ideas that causes us to lose sight of the facts, and above all against the error of identifying the public good with wealth, abstracted from the sufferings of the human beings who create it. [Sismondi 1827]

Walter Bagehot, in his *Economic Studies,* wrote of Ricardo: "He thought he was considering actual human nature in its actual circumstances, when he was really considering a fictitious nature in fictitious circumstances" (1953, p. 157). Whitehead called this "the fallacy of misplaced concreteness." He defined it as "neglecting the degree of abstraction involved when an actual entity is considered merely so far as it exemplifies certain categories of thought" (1929b, p. 11). More generally it is the fallacy involved whenever thinkers forget the degree of abstraction involved in thought and draw unwarranted conclusions about concrete actuality. Nicholas Georgescu-Roegen wrote: "It is beyond dispute that the sin of standard economics is the fallacy of misplaced concreteness" (1971, p. 320).

Sismondi, Bagehot, and Whitehead did not oppose all use of abstractions. The problem lies in neglecting the extent to which our concepts are abstract, and therefore also neglecting the rest of the reality from which they have been abstracted. In Whitehead's words, "The methodology of reasoning requires the limitations involved in the abstract. Accordingly, the true rationalism must always transcend itself by recurrence to the concrete in search of inspiration. A self-satisfied rationalism



is in effect a form of anti-rationalism. It means an arbitrary halt at a particular set of abstractions" (Whitehead 1925, p. 200).

What is the set of abstractions that political economy has riveted on economic thought and at which it has come to a self-satisfied halt? One of the most important is the abstraction of a circular flow of national product and income regulated by a perfectly competitive market. This is conceived as a mechanical analog, with motive force provided by individualistic maximization of utility and profit, in abstraction from social community and biophysical interdependence. What is emphasized is the optimal allocation of resources that can be shown to result from the mechanical interplay of individual self-interests. What is neglected is the effect of one person's welfare on that of others through bonds of sympathy and human community, and the physical effects of one person's production and consumption activities on others through bonds of biophysical community. Whenever the abstracted-from elements of reality become too insistently evident in our experience, their existence is admitted by the category "externality." Externalities are ad hoc corrections introduced as needed to save appearances, like the epicycles of Ptolemaic astronomy. Externalities do represent a recognition of neglected aspects of concrete experience, but in such a way as to minimize restructuring of the basic theory. As long as externalities involve minor details, this is perhaps a reasonable procedure. But when vital issues (e.g., the capacity of the earth to support life) have to be classed as externalities, it is time to restructure basic concepts and start with a different set of abstractions that can embrace what was previously external. (The distinction in Chapter 3 between localized and pervasive externalities is a step in this direction.) The frequency of appeal to externalities is a good index of the overall problem of misplaced concreteness in economic theory. But there are more particular examples as well.

Perhaps the classic instance of the fallacy of misplaced concreteness in economics is "money fetishism." It consists in taking the characteristics of the abstract symbol and measure of exchange value, money, and applying them to the concrete use value, the commodity itself. Thus, if money flows in an isolated circle, then so do commodities; if money balances can grow forever at compound interest, then so can real GNP, and so can pigs and cars and haircuts.

No less an intellect than John Locke committed this fallacy in his theory of private property. He at first argued that one's legitimate accumulation of property was limited to what one could use before it spoiled. Thus the physical tendency to spoil, rust, rot, and decay set a kind of

natural limit to accumulation of real wealth. But, Locke argued, with the advent of a money economy, that natural limit disappears because money does not spoil, and wealth can be accumulated in the form of money. Note that the characteristic of the abstract symbol (nonspoilage) comes to dominate the characteristic (spoilage) of the concrete reality being symbolized. Locke's limitation on wealth disappears even though wealth still spoils. One might as well argue that butter accumulation is not limited by spoilage because the quantity of butter is measured in pounds, and pounds can be summed indefinitely in a ledger without spoiling.

Clearly, the existence of millionaires does not necessarily imply rotting stockpiles of goods. Indeed, money balances do not imply the existence of any real goods at all. The willingness of the community to hold money derives from the inconvenience of barter, and the fact that money is an indent or lien against *future* production, which cannot spoil because it does not yet exist. But the real wealth of a community, even in a money economy, consists of goods to which the principle of spoilage still applies. So accumulating money balances cannot indefinitely be matched by accumulating real wealth. Therefore at some point accumulated money becomes a lien against future production rather than a claim check to simultaneously existing goods. The willingness of future producers to honor those past claims on their current production will at some point become an issue. In practice, such an excess of money claims over real wealth will likely result in debt repudiation by inflation. Current producers would charge more and pay themselves more money for their product and thus bid that product away from those whose claims do not result from current production but derive from past claims stated in fixed money amounts. The concentration on money and the market rather than on physical goods, with the concomitant decision to model itself on the methods (but not the content!) of physics, has been characteristic of the whole of modern economics. This paved the way for the primacy of deduction and the focus on mathematical models and computer simulations that are the hallmark of current practice in the discipline. Such elaborate and beautiful logical structures heighten the tendency to prize theory over fact and to reinterpret fact to fit theory.

An extreme example of this tendency is provided by Gary Becker and Nigel Tomes (1979) in their model of intergenerational distribution of income. They attempt in rigorous fashion to extend the model of individualistic utility maximization over intergenerational time periods and



use it to explain long-run changes in the distribution of wealth and income. The model requires a self-identical, well-defined decision-making unit over intergenerational time. Individuals die off, so they won't do. Families won't do either because although they endure they are neither self-identical nor independent. Families endure only by merging and mixing their identities through sexual reproduction, and thus are not independent or well defined over intergenerational time.

Your great-great grandchild will also be the great-great grandchild of fifteen other people in the current generation, many of their identities now unknown. Presumably your great-great grandchild's well being will be as much an inheritance from each of these fifteen others as from yourself. Therefore it does not make sense for you to worry too much about your particular descendant, or to take any particular action on his or her behalf. The farther in the future is the hypothetical descendant, the greater the number of co-progenitors in the present generation, and consequently the more in the nature of a public good is any provision made for the distant future. To the extent that you are concerned about the welfare of your descendant, you should also be concerned about the welfare of all those in the present generation from whom, for good or ill, your descendant will inherit. Thus a concern for future generations should reinforce rather than weaken the concern for present justice—contrary to what is often supposed. Although we are not all brothers and sisters in the literal sense, we are quite literally co-progenitors of each others' distant descendants.

The thrust of these evident consequences of sexual reproduction is toward community and away from individualism—a thrust generally resisted by standard economics, especially the Chicago school, of which Becker is a prominent member. To avoid this thrust and keep the world safe for individualistic maximization, Becker and Tomes adopt the obvious if extreme expedient of assuming asexual reproduction! It is one thing to abstract from the incidental in order to highlight the fundamental. It is something else to abstract from the fundamental to save a model. When the concrete fact of sexual reproduction conflicts with the abstractions of individualistic maximization, the authors hang on to their abstractions as somehow more real. Becker and Tomes try to convince the reader, quite unsuccessfully in our opinion, that this absurd assumption is for expository convenience only and that nothing important hangs on it (Daly 1982).

The focus on mathematics in place of empirical attention to physical reality takes its toll also in a crucial argument of Julian Simon in *The*

*Ultimate Resource.* He wishes to show that we need not be concerned about absolute shortages of natural resources. He writes: "The length of a one-inch line is finite in the sense that it is bounded at both ends. But the line within the endpoints contains an infinite number of points; these points cannot be counted, because they have no defined size. Therefore the number of points in that one-inch segment is not finite. Similarly, the quantity of copper that will ever be available to us is not finite, because there is no method (even in principle) of making an appropriate count of it" (Simon 1981, p. 47).

Note that Simon switches from the concept of infinite *divisibility* to infinite *amount,* from the infinity of points on a line to the infinity of copper in the ground, with nothing but the word "similarly" to bridge the gap. No doubt the abstract properties of numbers can be used to describe many facts about copper, but not every property of abstract numbers is obliged to convey a concrete truth about copper.

A final example also has to do with resource availability. Lester Thurow argues:

> In the context of zero economic growth and other countries, a fallacious "impossibility argument" is often made to demonstrate the need for zero economic growth. The argument starts with a question. How many tons of this or that non-renewable resource would the world need if everyone in the world now had the consumption standards enjoyed by those in the US? The answer is designed to be a mind-boggling number in comparison with current supplies of such resources. The problem with both the question and the answer is that it assumes that the rest of the world is going to achieve the consumption standards of the average American without at the same time achieving the productivity standards of the average American. This is, of course, algebraically impossible. The world can consume only what it can produce. When the rest of the world has consumption standards equal to those of the US, it will be producing at the same rate and providing as much of an increment to the world-wide supplies of goods and services as it does to the demand for goods and services. [Thurow 1976, p. 40]

Professor Thurow thought well enough of this argument that he reproduced it verbatim five years later in Chapter 5 of his otherwise admirable book, *The Zero-Sum Society* (1981, p. 118). Thurow appeals to the abstract accounting conventions of the circular flow of exchange value in order to "prove" that the physical flow of resources can never be a constraint on economic growth. He tells us that it is not only possible for the U.S. standard of resource consumption to be generalized to the entire world, it is "algebraically impossible" that it should be otherwise! Never mind about tons of nonrenewable resources and all those numbers that are "designed" to be mind-boggling. Aggregate production equals aggregate income, and that is all there is to it! Unfortunately for Thurow's argument, the algebra of circular-flow accounting identities tell us absolutely nothing about the adequacy of biophysical resources to sustain worldwide a per capita resource use rate equal to that of the United States (Daly 1985).



Enough examples have been presented to lend credence to Georgescu-Roegen's claim, cited above, that misplaced concreteness is the cardinal sin of standard economics. Nor can these examples be dismissed as straw men. We have quoted only from deservedly respected economists of diverse ideological bent, professors from such prestigious universities as Chicago, MIT, Maryland, and Yale. Our purpose is not to impugn their professional status, but merely to argue that when the best economists fall so easily into the trap, we should have greater respect for the trap and guard more against it.

## Avoiding the Fallacy

How can we guard against misplaced concreteness in economics? For one thing, we could warn students about it in the early chapters of economic principles texts, as we already do for the fallacy of composition, *post hoc ergo propter hoc, petitio principii,* and other Latin crimes against reason. As far as we have been able to ascertain, no text mentions the fallacy of misplaced concreteness. They do talk about abstraction, but mainly in order to emphasize its powers, not its dangers.

One must admit that avoiding misplaced concreteness is not easy. We simply cannot think without abstraction. "To abstract" means literally "to draw away from." We can draw away from concrete experience in different directions and by different distances. To expect perfect judgment in choosing the direction and distance of abstraction proper to each argument, and never to mix up levels in the middle of an argument, is to expect too much. It seems we must always commit this fallacy to some degree, and we must think of minimizing it rather than eliminating it entirely. For this reason it is a very subtle fallacy—more a general limitation of conceptual thought than an error in logic.

There are nevertheless two rules of thumb that will help us to minimize misplaced concreteness. One is, in Whitehead's words, "recurrence to the concrete in search of inspiration." One technique for getting back to the concrete is to look at all four of Aristotle's notions of

cause. These four causes (material, efficient, formal, and final) can be explained with reference to a house. The material cause is the lumber, bricks, and so forth from which the house is made. The efficient cause is the carpenter and his tools, which effect a change of form in the material. The formal cause is the blueprint that the carpenter is following. The final cause is the purpose for building the house—for example, shelter and privacy. In economics our attention is overwhelmingly focused on efficient and formal causes. If we remember material and final causes as well, we will be less likely to commit the fallacy of misplaced concreteness. Whitehead said, "A satisfactory cosmology must explain the interweaving of efficient and final causation" (1929a, p. 28). Likewise for a satisfactory political economy.

One could hardly accuse Whitehead, the coauthor of *Principia Mathematica,* of harboring a vulgar prejudice against abstract thought. He just insists, like a good economist, that we constantly weigh the costs of our particular abstractions against the benefits, and that we be willing to recur to the concrete now and again.

Whitehead describes the costs and benefits of abstraction as follows.

> The advantage of confining attention to a definite group of abstractions, is that you confine your thoughts to clear-cut, definite relations. . . . We all know those clear-cut, trenchant intellects, immovably encased in a hard shell of abstractions. They hold you to their abstractions by the sheer grip of personality.
>
> The disadvantage of exclusive attention to a group of abstractions, however well-founded, is that, by the nature of the case, you have abstracted from the remainder of things. Insofar as the excluded things are important in your experience, your modes of thought are not fitted to deal with them. [Whitehead 1925, p. 200]

The second and related rule of thumb is to avoid excessive professional specialization.

> The dangers arising from this aspect of professionalism are great, particularly in our democratic societies. The directive force of reason is weakened. The leading intellects lack balance. They see this set of circumstances, or that set; but not both sets together. The task of coordination is left to those who lack either the force or the character to succeed in some definite career. In short, the specialized functions of the community are performed better and more progressively, but the generalized direction lacks vision. The progressiveness in detail only adds to the danger produced by the feebleness of coordination. [1925, p. 200]

That this danger is an aspect of the fallacy of misplaced concreteness is indicated in the following paragraph where Whitehead adds, "There is



development of particular abstractions, and a contraction of concrete appreciation. The whole is lost in one of its aspects" (1925, p. 200).

Those fields of economics that deal more with the whole and the concrete, such as economic history, comparative systems, history of economic thought, and economic development ought to be more emphasized, not only for their own sakes, but also as an antidote to the near toxic levels of rarefied abstraction encountered in the "core courses."

Recognizing the fallacy of misplaced concreteness is particularly important to establishing economics for community, because community is precisely the feature of reality that has been most consistently abstracted from in modern economics. The need is not for one more theorem squeezed out of the premises of methodological individualism by a more powerful mathematical press, but for a new premise that reinstates the critical aspect of reality that has been abstracted from—namely, community.