

# Introduction: The State of Economics, the State of the World

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## 1776 and 1860

For the discipline of economics, and for the world at large, these are unusual times. The shock and awe of the financial crisis that began in the United States in 2008 and the series of economic fault lines it ripped open—from the sovereign debt crisis in the European Union to the massive slowdown in several emerging economies that we are currently witnessing—have led to much soul searching.

The past nearly two and a half centuries, from Adam Smith's *The Wealth of Nations* (1776) to the flourishing of empirical research and big data in current times, mark the astonishing rise of a discipline. From a broad, descriptive, and speculative subject, economics has come to acquire a common methodological foundation, mathematical structure, and a growing database. It has vastly enhanced our understanding of markets, exchange, money, finance, and the drivers of economic development.

How did this come to be? Where is economics headed? Will it be up to the diverse challenges of our times? Will global poverty be eradicated, or will it be exacerbated under the strain of a deteriorating environment? These are the questions we grappled with over the 2 days of the conference that is the basis of this book. The conference brought together some of the most prominent individuals who have, for better or for for worse (depending on your love or distaste for economics), played a role in making economics what it is today.

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There have been achievements in economics from well before 1776 to now. But for me, the transformational period of the discipline was the 100-odd years, starting from the second half of the nineteenth century. If you like birthdays, I have a date to propose to mark the birth of modern economics: February 19, 1860.

Stanley Jevons wrote a celebrated letter to his brother on June 1, 1860, saying that he had made a stunning discovery in the past few months that explained the “value” of different goods and gave him insights into “the true theory of Economy.” He told his brother that so thoroughgoing and consistent was his theory that “I cannot now read other books on the subject without indignation” (Collison Black 1973, 410).

When exactly did he hit upon the idea? Historians of economic thought have drawn our attention<sup>1</sup> to a special entry in Jevons’s diary, on February 19, 1860: “At home all day and working chiefly at Economy, arriving I suppose at a true comprehension of Value.” Birthdays for scientific breakthroughs are always questionable. But if we can have Mother’s Day, Valentine’s Day, Administrative Professional’s Day, I see no reason we cannot have Modern Economics Day, and February 19 would be my pick.

Of course, thinkers were already laying the foundations for Jevons’s breakthrough. Gossen had worked out quite a lot of this a good one or two decades before Jevons. Cournot laid some of the substructure in 1838. And the law of diminishing marginal utility and its significance were described by Daniel Bernoulli as early as 1738, to solve the St. Petersburg paradox, which had been discovered in 1713 by Nicolaus Bernoulli. (And, yes, it was all in the family, Nicolaus being Daniel’s brother.)

It is also important to note that although Stanley Jevons (1871) was clearly on to the main ideas of general equilibrium and value, he never quite got all the way there. We needed Léon Walras (1877) to put up the main structure. And for the full general equilibrium project to be completed, with the existence of equilibrium proved and its welfare properties spelled out, we needed to wait another 75 years for the seminal contributions of Kenneth Arrow.

By the time John Hicks, Paul Samuelson, Ken Arrow, Gerard Debreu, Lionel McKenzie, and others were doing their work,<sup>2</sup> modern game theory had been born. Over the next decades, the combination of a fully worked-out

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1. See La Nauze (1953).

2. See Hicks (1939), Samuelson (1947), Arrow and Debreu (1954), and McKenzie (1959).

general equilibrium system, game theory, and a little later, social choice, ideas of asymmetric information and adverse selection, endogenous price rigidities, theories of economic growth and development economics, and the first understandings of the rudiments of monetary policy would transform the landscape of economics.

Few activities in life are as innately joyous as the pursuit (and if one is lucky, the discovery) of new ideas, the unearthing of patterns in the abstract space of concepts and numbers or in the world of data and statistics. Frontline researchers must have the space, like artists and composers, to do what they do as an end in itself. The greatest benefits of research are usually a by-product of this freedom. But here at the World Bank, our preoccupation is much more down to earth and is driven by policy needs. Hence, what we wanted to take away from the conference was how we can draw on the best of economics to promote development and sustained, inclusive growth, and contribute to making the world a better place. The World Bank's research and data analyses have been enormously influential, reaching the desktops of finance ministers and policymakers all over the world; indeed, a special responsibility comes with this influence.

At the time of this writing, I have been chief economist of the World Bank for nearly 4 years. This conference and the book are an opportunity to share some of my concerns and questions with the distinguished gathering at the conference and also with a wider readership. The hope is that the conference and its proceedings (to wit, the present book) will strengthen the World Bank's mission of promoting development.

Because the World Bank's engagement is primarily with development economics, it may be worthwhile to point out that development economics, like economic theory, has had its moments of epiphany. Arthur Lewis had been troubled by two problems. First, there was the age-old question of why industrial products, such as steel, were so much more expensive than agricultural products. Second, why were some countries persistently poor, while others were so rich?

In an autobiographical essay, Lewis (1980, 4) writes about his eureka moment in 1952: "Walking down the road in Bangkok, it came to me suddenly that both problems have the same solution. Throw away the neoclassical assumption that the quantity of labor is fixed. An unlimited supply of labor will keep wages down, producing cheap coffee in the first case and high profits in the second. The result is a dual national or world economy." This epiphany was the genesis of his classic paper on dual economies in the

*Manchester School* (Lewis 1954), which would play a major role in his being awarded the Nobel Prize in 1979<sup>3</sup> and in triggering research on development economics.

### Intuition and Causality

I turn now, more specifically, to the subject of development policy. For the project of converting research to good policy, we need three ingredients: data (and evidence), theory (and deductive reasoning), and intuition (and common sense).

One of the great achievements of economics in recent decades has been in the area of empirical analysis. We have good reason to celebrate the rise of data and our ability to analyze data using different methods: from intelligent bar charts, through simple regression analysis and structural models, to randomized control trials. This recent success raises the hope of economics becoming a truly useful science (see Duflo and Kremer 2005; Banerjee and Duflo 2011).

There is, however, a propensity among some economists to dismiss all theory as esoteric.<sup>4</sup> Among other dangers, we run the risk of making our discipline inefficient. Suppose we insisted that Pythagoras could only use empirical methods. Would he ever have gotten to his famous theorem? The answer is: He might have. If he had collected a large number of right-angled triangles and measured the squares on their sides, he might have hit on the conjecture of the two smaller squares adding up to the one on the hypotenuse. But this approach would be extremely inefficient. Moreover, there would be a lot of debating and dissent. Some would charge him with using a biased sample of right-angled triangles, all from the Mediterranean region. "Would it work in the Arctic, in the Southern Hemisphere?" they would query.

We must acknowledge that many truths can be discovered more efficiently and more compellingly using pure reason. Further, there is a great

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3. This idea, combined with the rise of modern growth theory (see Arrow 1962; Lucas 1988; Romer 1994; Ray 1988; Aghion and Howitt 2009), has given us insights into the development process and development policy that were unthinkable even a few decades ago.

4. For one of the best discourses on the strengths and vulnerabilities of economic theory, see Rubinstein (2006).

deal of sloppiness in the way we reason about the use of evidence. For instance, hard-headed practitioners will often tell you the following: “If we do not have any evidence about whether some policy X works, we must not implement X.” (I was told exactly this fairly recently, in response to a suggestion I made.)

Let me call this rule in quotes an “axiom.” To see that it is an unreasonable axiom, observe that if we do not have any evidence about whether X works, then we also do not have any evidence about whether not-X works. But because we have to do either X or not-X, the original axiom has to be flawed.

For good policy, we need facts and evidence, but we also need deduction and reasoning. We can go a step further and make a case for using mathematics. Although the use of mathematics can be overdone (as has happened in economics), the immense achievements of Cournot (1838) and Walras (1877), and of modern economics, would not have happened without it. This is because mathematics is a disciplining device, even though it is demanding and clearly not something that is applicable in all situations. As Krugman (2016, 23), not being able to make up his mind whether a particular argument of Mervyn King (2016) was right, observes, “words alone can create an illusion of logical coherence that dissipates when you try to do the math.”

The power of doing a model right, even if it is abstract and uses assumptions that may not be real, can be seen from general equilibrium. Take Gerard Debreu’s (1959) classic *The Theory of Value*. This book is of great beauty, as spare as poetry. In some ways, it is comparable to the work of Euclid, for it brings together in a systematic way an amazing range of ideas. Euclid may not have been as original as Pythagoras or Archimedes, but in bringing intellectual order to a scattered discipline, he had few peers, and he served an enormous role in the progress of knowledge. Likewise for Debreu’s slim book.

The pathbreaking general equilibrium model of Walras, Arrow, and Debreu provided a template that sparked off some of the most original works in microeconomic theory—notably those by Akerlof and Stiglitz—which have to do with modeling the functioning of markets under imperfect information.<sup>5</sup> These works have greatly enhanced our understanding of micromarkets; why markets fail; and why prices are often endogenously rigid, resulting in credit markets with excess demand and labor markets

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5. See Arrow (1963), Akerlof (1970), Stiglitz (1975), and Stiglitz and Weiss (1981).

with excess supply. This research also has hopes of improving our macroeconomic analysis, because, as we know, Keynesian macroeconomic analysis, like Arthur Lewis's dual economy model, makes extensive use of price rigidities, and neither Keynes nor Lewis had an explanation for these rigidities. Thanks to the work of Stiglitz and a few others, we now have a formal understanding of open unemployment and credit markets that do not clear despite the absence of exogenous restrictions on interest rate movements.

Along with these positive theories, we have seen the rise of normative economics. Perched between analytical philosophy, mathematical logic, and the social sciences, this achievement was remarkable. Major contributions were also made by Samuelson (1947), Bergson (1938), and others, but the truly astonishing breakthrough was Ken Arrow's (1951) slim book: *Social Choice and Individual Values*. Arrow's impossibility theorem became the bedrock of an enormous research agenda. The leading figure here was Amartya Sen, whose work, straddling philosophy and economics, demonstrated that it is possible to bring the finest traditions of theory and mathematical logic to bear on age-old questions of ethics and normative principles (Sen 1970; see also Suzumura 1983). This work brought into the mainstream of rigorous analysis such concepts as rights, which were widely talked about but seldom subjected to careful scrutiny (Sen 1996). This body of work has been important for the World Bank, because its mission goals have foundations in such concepts (World Bank 2015b) and also in related country-specific research (Subramanian and Jayaraj 2016).

It is worth digressing for a moment to note that data and statistics belong to a larger domain of inquiry, which has to do with description. The term "descriptive social science" is often treated as a pejorative, which is unfortunate. As Amartya Sen (1980) points out in a powerful essay, developing a good description is not easy, and a huge amount of the progress of science depends on description. Description, be it in words or data, entails choice. Description is not regurgitating everything we see around us. We have to pick what is vital and make that available to others. How we describe and what we describe shape our understanding of the world. The "describer" is therefore a pivotal agent.

It is important to be aware that description can take many forms. What the anthropologist describes often does not take the form of numbers and data. But the description of what he or she has seen and, more importantly, experienced is vital for our understanding of the world. The concept

of “thick description”—which we owe to Gilbert Ryle (1968) and Clifford Geertz (1973) and used by umpteen anthropologists—has vastly enhanced our understanding of traditional and remote societies. It has enabled us to intervene more effectively. At times this intervention has been for the wrong reasons (for instance, to enable colonial domination), but it has also helped carry the development agenda further by extending the reach of modern medicine and education.

Historically, we have learned of the motivation and purpose of other lives, which are distant from ours, by the ardor and work of anthropologists. These topics are very difficult to learn and comprehend by data and statistics alone. Living with the subject and acquiring an intuitive understanding are often necessities. This knowledge has been put to good and bad uses, to help the poor living in distant lands and in traditional societies, and also to exploit people and spread imperialism and colonial control. For good or for bad, the knowledge has been useful.

The absence of such knowledge can create major handicaps. Consider terrorism. Because of the dangers associated with observers interacting with terrorist groups, we do not have studies of the kind anthropologists have provided for remote societies, resulting in an insurmountable knowledge gap.

The skeptics, from Pyrrho to David Hume and Bertrand Russell, were right: Neither fact nor deduction can take you all the way to the best policy to implement. The reason is that causality, regardless of whether it is present, can never be demonstrated. In the end, causality lies in the eyes of the beholder. For me, the most thought-provoking observation on this comes from a tribesman from Nepal. The famous National Geographic photographer, Eric Valli, seeing the tall trees these tribesmen climbed to gather honey, asked one of them whether they ever fell out of those trees. The answer he received was: “Yes, you fall when your life is over.”<sup>6</sup>

Given the impossibility of discovering causality, for good policy, it is not enough to have the facts; it is not enough to combine facts with theory. I am convinced we need one more ingredient: common sense and what I have elsewhere called “reasoned intuition” (Basu 2014).

Researchers refuse to admit it, but it is true that there is no escape from the use of intuition, and the bulk of what we call “knowledge” that we

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6. This quote, as well as the argument on causality, which is more intricate than may appear from these brief remarks, are taken from Basu (2014, 458).

acquire through life occurs casually, mainly by using common sense. It would be a mistake to insist that all knowledge has to be rooted in scientific method, such as controlled experiments. It is quite staggering to consider the number of things a child learns through nonscientific methods.

As to why such knowledge, acquired through intuition and common sense, may have value, we have to recognize that our intuitions are what they are because of evolution. These methods have survived natural selection, and so their power must not be dismissed out of hand. Evolution has shaped a lot of what we see in our economic life; this is widely acknowledged, but our understanding of the interface between evolution and economics, for which some foundations were laid by Maynard Smith and Price quite some time ago (see Maynard Smith and Price 1973; Weibull 1995) remains rudimentary. There is a foray into this topic in this book (see chapter 9) in the context of morality and its origins (see also Alger and Weibull 2013). But it is arguable that such innate knowledge acquisition applies to many other domains. The way people commonly acquire knowledge may not meet the test of scientific standards, but it cannot be dismissed out of hand. At the same time, casual empiricism can lead to superstitions, which we have to guard against. I have argued elsewhere (Basu 2014) that what we need is “reasoned intuition,” that is, the use of intuition vetted by reasoning. This is not a surefire method, but it is the best we can do.

Data, theory, and intuition are the three ingredients for human knowledge and progress. But even with all three in place, skepticism, as philosophers through the ages have reminded us and as Keynes (1936) did in chapter 12 of *General Theory*, must be a part of the thinking person’s mindset. One problem with scientists who lash out against superstition but do not question scientific knowledge is the double standard. They fail to recognize that, when it comes to certainty about the future, scientific wisdom is as much open to question as many other forms of knowledge.

### **Knowledge and Caveats**

We are heading into uncharted territory and struggling with the world’s economic problems. Recent problems include United Kingdom’s vote in favor of exiting the European Union (I suspect this important issue will persist for some time) and the decline in commodity prices (especially that of oil), which is creating a lot of stress in commodity exporting nations and

in corporations that have invested in this sector. Questions are being raised about the readiness of the discipline of economics to address such issues. The first thing to recognize, however, is not that economists misread or underestimated these crises, but how these problems show that there is still a lot about the economy that we do not know.

Experts in any discipline suffer from the disadvantage of not knowing exactly what it is they do not know. Take, for instance, medicine. Given how little we know about the human body and brain, when we consult a doctor with health problems, in most cases the right answer for the doctor to give is: "I have no idea." But we seldom hear this. Doctors almost invariably tell you what your problem is. What should warn you that when doctors say they know what your ailment is, they in fact often do not is that, even in the eighteenth century, well before the arrival of modern medicine, doctors seldom said they had no idea what ailed the patient. This is because doctors in the eighteenth century did not know—and doctors now do not know—what they did not and do not know. It is much the same with economists.

Among the areas of darkness that hamper development policy is our inability to link the micro and the macro. Suppose a government undertakes some intervention  $X$  in a thousand villages.  $X$  can be a conditional cash transfer, an employment creation program, or provision of a fertilizer subsidy. How do we evaluate the success of the program in reducing poverty? Typically, we do this by collecting data on the well-being of the people in these villages. If we are fussy, we may use all kinds of controls, including proper randomization. Suppose, through such a study, it is found that poverty has indeed gone down in the villages where  $X$  was implemented. Does this mean  $X$  is a good intervention? Not necessarily. Suppose the intervention  $X$  in a village has the following effect. It raises food prices a little and raises wages more. This will indeed lead to lower poverty in the village. But because a rise in food prices typically cascades across the whole economy, this intervention could mean that in other villages, which will only feel the full rise in food prices and a negligible effect on wages, poverty will rise. So it is entirely possible that the nationwide effect of the intervention will be no effect on poverty or even an increase in poverty, though poverty falls in the villages in which the interventions occurs.

These links between micro interventions and macro effects are poorly understood. We need to invest much more in this kind of research if we are

to succeed in battling nationwide and even global poverty and to combat inequality.

In other micro-theoretic areas, such as finance and the psychological foundations of human behavior, economics has made great strides, as discussed in this book.<sup>7</sup> But open questions still exist. In finance, it is increasingly recognized that there is no such thing as an ideal regulation. This is because financial products are amenable to endless innovation. Banks and financial organizations will keep developing new products, just as the pharmaceutical industry keeps discovering new drugs. And with each such financial innovation, we may need to modify and make our regulatory regime more sophisticated. Hence, this is one area where we have to reject the language of optimal regulation, which has a static connotation, and to create regulatory bodies that are flexible and ready themselves to innovate. This effort is complicated by the fact that when selecting financial products, people are often not rational and instead give into emotions, hyperbolic discounting, and framing delusions, as pointed out repeatedly in the recent behavioral economics literature.

One possibility is to label certain financial products as “prescription goods” and create the equivalent of doctors in finance, who have to sign off before a person is allowed to buy a financial product. We could, for instance, decide to allow balloon mortgages, but before a consumer can commit to one, he or she has to get a “finance doctor” to sign off on the financial viability of taking on such a contract. This cannot be done by mechanically following practices in medicine, but a case can be made for giving serious thought to such an architecture.

The interface between economics and psychology, and, more specifically, behavioral economics, has witnessed great strides; and we at the World Bank have tried recently to bring this progress to bear on the agenda of development policy with our *World Development Report on Mind, Society, and Behavior* (see World Bank 2015a). By drawing on evidence from laboratory experiments and field observations from around the world, behavioral

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7. For an elegant example of how economic theory can be brought to bear on a compelling idea in finance and financial crisis (namely, the phenomenon of infection, which has been widely noted, whereby one economy, seemingly unconnected to another, infects it with financial panic), see Morris and Shin (1998).

economics teaches us a lot about how and where we should intervene.<sup>8</sup> However, this discipline might risk becoming a catalog of findings. I call this a risk because of a propensity to think of the findings as set in stone, not realizing that they may be true in some societies at certain stages of development and might differ with place and time.

What is also needed is an effort to marry these findings more effectively with the concept of equilibrium (Akerlof and Shiller 2015). Then we would be able to leverage these findings to get much more out of them and also be able to predict better how the findings are likely to change from one society to another and to evolve over time. To my mind, one of the great contributions of traditional economics is the idea of equilibrium, which has many manifestations, from the general competitive equilibrium to Nash. We need to broaden the description of individuals from the narrow *Homo economicus* to that of more realistic individuals (with quirks, irrationalities, and social norms) and to use the idea of equilibrium in conjunction with this more realistic description.<sup>9</sup> What makes this effort intellectually challenging is that for most real phenomena, which seemingly rely on human irrationality or adherence to social norms, it is possible, with analytical ingenuity, to accurately model the same behavior using perfectly rational individuals.<sup>10</sup> In the end, better modeling calls for the use of judgement and intuition when deciding what assumptions we should rely on.

The World Bank has been increasingly engaged in this difficult area. Given the current drift of global concerns, we do not have a choice. These concerns naturally lead to another related field beyond the narrow confines of economics, that is, institutions and governance.<sup>11</sup> Our *World Development Report on Governance and the Law* (see World Bank 2017) takes on this

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8. See Kahneman (2000), Thaler and Sunstein (2008), and Hoff and Stiglitz (2016).

9. For a very interesting paper that attempts this, see Hoff and Stiglitz (2016). Earlier, Gintis (2009, chapter 10) provided an elegant model of bringing together the idea of human sociality and economic equilibrium in a unified game-theoretic discourse.

10. For an ingenuous exercise in this type of modeling, see Myerson (2004). Behavior, which at first sight seems so obviously driven by an irrational adherence to norms, can be explained as rational behavior in a more complex setting.

11. The importance of this field is stressed by Bourguignon (2015) in analyzing the African experience. As he stresses, this analysis is much more than an academic exercise. It is germane to the design of successful policy interventions.

challenging task.<sup>12</sup> One important area of policy making is the control of corruption, a big task faced by those at the helm of policy. Traditional economics treated an act of corruption (e.g., whether to pay a bribe to get an illegal electricity connection) on par with any other purchasing decision (e.g., whether to buy an apple)—that is, as an exercise in narrow cost-benefit analysis (see Bardhan 1997; Mishra 2006). It is not surprising that we have been so singularly unsuccessful in controlling corruption. To understand this phenomenon, it is important to bring in psychology and political institutions. Development policy cannot be built on economics alone.<sup>13</sup>

Finally, one area in which we have knowledge gaps but not as much as conservative commentators make out, is the connection between climate change and development. If we proceed the way we have done thus far, it is a journey headlong into disaster. This is unfortunate, because awareness of the connection between environmental resources and economic development came early, as evidenced in the works of Thomas Malthus, David Ricardo, Knut Wicksell, and others, even though we have been tardy in terms of action and policy. In recent times, the importance of this connection has been stressed by several authors, notably by Stern (2007, 2015).

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12. The challenge of this task is captured well in the short essay by Green (2016), which points to the necessity of delving into this arena if we want to do economic policy right, and to how hard it is to do, because it ruffles feathers and is intellectually such treacherous terrain. Academic research that addresses governance and political institutions with the sharp scalpel of analysis is still relatively rare, but see Dixit (2009) and Acemoglu and Robinson (2012).

13. Here I give the example of corruption to illustrate the need for multiple disciplines, but the need is quite ubiquitous in today's world of strife and conflict. An excellent example is the Middle East. It is difficult to explain what is happening there purely in terms of economic indices, from gross domestic product through poverty to various measures of inequality and polarization. As Devarajan and Mottaghi (2015) argue, what is happening, in essence, is a breakdown of a social contract, which, like plumbing, goes unnoticed when it functions well but is always important. One can go further and look at areas that seem squarely situated in the domain of economic problems, such as the subject of poverty and inequality mitigation, which is central to the World Bank's work. Is it enough to rely on market forces and natural economic growth? Careful econometric studies of countries that have been most successful in this, such as Brazil, show that we have to go beyond these phenomena. Ferreira, Ravallion, and Leite (2010), for instance, find hard evidence that changing social security practices and increasing social assistance expenditure by the federal government was critical, and in fact happened because of the 1988 Constitution.

Now with the Paris Agreement of 2015, there is a platform to relate what we know on the subject with action on the ground, which is not easy, because it entails some cross-country coordination. It is worth stressing here that this engagement should be viewed very much as part of shared prosperity, because it entails intergenerational sharing of resources and well-being.

### **Money and the Person of Influence**

The previous section discussed some gaps in our knowledge. One big gap is in the area of monetary policy. Although economics has made some dramatic breakthroughs in some practical areas (such as how to design auctions and how to micromanage demand and supply in sectors), its grasp of the impact of macroeconomic and especially monetary policy interventions is rudimentary. It is true that we have learned to manage hyperinflation, and we can hope never to see again, at least in advanced economies with sophisticated central banks, the kind of runaway inflation seen in, for instance, Hungary in 1946 and Germany in 1923. But as the global financial and growth crisis that began in 2008 continues unabated, and governments and central banks flail at this with different policies, it is evident that large gaps exist in our understanding of the impact of macroeconomic policies, and the linkage between the financial and real worlds (Stiglitz 2011). This is something I learned by fire, during my nearly 3 years as a policy maker in India (from 2009 to 2012). Although monetary policy was not my charge, it became clear during this time that much of our interventions were based on imitating policies followed by central banks in advanced economies, unmindful of the fact that their contexts differed.<sup>14</sup>

One reason for this deficiency is that we do not understand the functioning and role of money in a market economy the way we understand, for instance, the Walrasian general equilibrium system for real goods and services. Money in general equilibrium was part of a big research agenda in the 1980s, but that agenda has remained incomplete. One reason is that it

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14. I discuss this in my recent book (Basu 2015), where I also argue for the need to make more experimental policy interventions in emerging economies, which would allow them to collect their own data and use these to develop their own, more context-specific policies.

is mathematically a very hard problem. But it must not be abandoned for that reason. In the rush to solve the next morning's problem, often these deep questions take a back seat. But as the world struggles to cope with the slowdown, and the widespread use of negative interest rates does not seem to work (and in fact has a negative backlash from which no country is able to individually break out of), it is important for economists to keep working on some of this fundamental research.<sup>15</sup> If the full general equilibrium model took some 75 years—from Jevons and Walras to Arrow and Debreu—and the study of money in equilibrium started in earnest in the 1970s and 1980s, we have little reason to abandon the problem as unsolvable.

To see the mystifying nature of money, one can look at a very different problem—the power of peddlers of influence. With the US presidential election in the offing, there was a lot of writing about lobbying, influence peddling, and corruption. In my youth in India, I remember talk about “persons of influence,” referred to those days as “men of influence.” I recall being baffled by one particular person and wondered why he was so well off. He had no special skill, no resources. He was just the man of influence (let me call him “M”). In those days, it took a wait of 6 years to get a phone connection. If you needed it sooner, you could try calling M and requesting his help. He would call up the relevant person in government; and more often than not, the favor would be done. If someone needed to get a child into a good school, she could ask M, and if M agreed, he would request the school principal to make an exception and take in this kid out of turn. It struck me much later what he was doing and I wrote it up as a model of the man of influence (Basu 1986). M was a person with a mental ledger of favors done. If  $i$  needed something from  $j$ , whom she did not know, she could ask M to ask  $j$ . Then  $j$  would do the favor, not because  $j$  cared for  $i$  or ever expected to need a special favor from  $i$ , but because  $j$  knew that someday he would need a favor from  $k$  and would need M to make a request of  $k$ . It is M that no one wanted to offend, because M was a clearinghouse with a memory. This is what made M a man of influence. In some sense, a person of influence is like money or a blockchain. It is a record of information and works only because everybody thinks it will work.

This description and even the model is straightforward enough. But its integration into a full general equilibrium model is extremely hard and

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15. Some of the fundamental questions in this area are raised in Calvo (1996).

remains an open agenda, thereby handicapping policy makers greatly and forcing them to rely more on intuition and guesswork than hopefully will be necessary in the future.

### Politics and Economics

When discussing development policy, I have been stressing the role of economic theory and empirical economics—in brief, input from professional, scientific analysis. The lack of this input dooms many a developing economy. But it is not always easy to marry scientific analysis with the ground realities of politics. Maybe because I moved so abruptly from academe to policy making, I cannot be unmindful of the importance of the role of how one engages with politics and politicians. When I moved from Cornell University to the Indian government at the end of 2009, I quickly became aware of the potential conflict between the prescription coming from theoretical economics and political compulsions. One quickly learned that when a politician tells an economist, “You are so good at theory,” it is meant to be a devastating criticism.

I have recounted in Basu (2015) how, at one of my first meetings in my new job with the prime minister and some of his advisers, I was discussing how to control food inflation, which was then at double digits. I spoke at some length on changing the manner in which food reserves are released in India to get the maximum dampening effect on prices. I basically drew some policy lessons from the logic of Cournot equilibrium. I was delighted that my suggestion was accepted, which, I now believe, owes as much to my not uttering the words “Cournot” or “equilibrium” as to Cournot’s excellent theorizing.

One gets a fascinating glimpse of the interface between the world of economic ideas and political compulsions in developing countries from Arthur Lewis’s experience as chief economic adviser to the Ghanaian government. He was invited to take this position by Kwame Nkrumah, the country’s first prime minister and president. The United Nations and the United States tried to block this appointment on the grounds that Lewis was “not very sympathetic to the Bank [the International Bank for Reconstruction and Development, commonly referred to as the World Bank]” (Tignor 2006, 147). There were also concerns, such as the one expressed by A. W. Snelling, an official in the British government, that “Lewis is a socialist, but a moderate one” (Tignor 2006, 148).

Lewis's tenure began extremely well, with Nkrumah personally excited at the prospect of Lewis steering the Ghanaian economy to a takeoff. On taking office, Lewis plunged into work, especially related to the second Five-Year Plan, with widespread support from others in government. But soon Lewis's idea of what constitutes good economics and Nkrumah's insistence on political compulsions came into conflict. Seemingly small differences of opinion—for instance, whether to spray cocoa trees that had been attacked by capsid beetles (pardon me for having forgotten who took which side)—became the cover for deeper conflict: the professional economist's insistence on good economics and the politician's stubbornness about what is politically good.

Lewis left office at the end of 1958, with Nkrumah's letter, gracious but recognizing that they could not work together, in his pocket: "The advice you have given me, sound though it may be, is essentially from the economic point of view, and I have told you on many occasions, that I cannot always follow this advice as I am a politician and must gamble on the future."<sup>16</sup>

### Interests and Ideas

Some months after I moved from academe to the Indian government, a reporter asked me: What was the one thing that I had learned in this transition? Unusually for a question of this kind, I had an answer. The reader may recall Keynes's beautiful observation on the power of ideas, which ended with the following: "I am sure that the power of vested interests is vastly exaggerated compared with the gradual encroachment of ideas" (Keynes 1936, 283–284).

As an academic, I loved the observation but did not believe in it, viewing it as the self-serving remark of a professor. It was only after I joined the Indian government and sat in interminable meetings with ministers and bureaucrats that I came to believe in Keynes's observation.

Ideas play an unbelievably important role, and so those in the business of ideas have a special responsibility. As a consequence, I view this conference and this book not just as an intellectual contribution but as a critical ingredient for the work that is meant to be done in an organization such as the World Bank.

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16. Nkrumah to Lewis, December 18, 1958, quoted in Tignor (2006, 173).

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