Induction, Knowledge and Efficiency

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Induction, Knowledge and Efficiency

Kaushik Basu

IN his recent article (*EPW*, May 5-12) C 1 Kurien makes several points concerning my earlier pieces (*EPW*, Special Number, 1988 and March 17, 1990) and also commits many fallacies. In response I confine myself to the issues raised by Kurien, excepting for one brief digression on induction.

I

Let me begin by putting aside something which, though philosophically a deep matter, is not central to our debate. This is regarding the acquisition of knowledge.

I believe that empiricism is critical in acquiring knowledge but I would not push it to the extreme, where all else is excluded, as Kurien seems to do. He goes to the extent of suggesting that arithmetical identities are learnt through experience. Thus he writes: "I have learned through experience that 2+2 can be shown to be equal to 4". This presumably means that Kurien learnt this by experiments like first placing two apples on a table, then another two and then counting them all together. But then, what about the fact that 1 million plus 1 million equals 2 millions? Either we would have to say that Kurien does not know this or that he has seen more apples than anyone else would care to. The absurdity of this shows that arithmetical identities are *not* purely a matter of experience.

II

I want to use the lead given by the above discussion and make a digression here and share with the readers a viewpoint on epistemology.

How do we know what we do? The usual answer is: "By induction". We know that the sun will rise tomorrow because we have seen it rise so many times before. The problem with this is that the same experiment seldom gets repeated. The sun has never risen on the kind of day that tomorrow will be. A person who has never seen a kiwi fruit and predicts that it will fall downwards on being dropped in mid-air may think that he 'knows' this from past experience. But that is a delusion for he has never had that experience before.

It may be argued that induction requires us to base our knowledge on *similar* experiments and not the *same* one. But what is 'similar'? Can we predict that kiwis will fall downwards from the fact that oranges do? It seems to me that what is a similar experiment is impossible to define excepting in a circular fashion.

But then, you may argue, am I not being too pessimistic? After all, we do know things and do make lots of predictions successfully. We know that the book kept on the table will not fly out of the window; that the pencil will not sing songs.

My own view on this is that we are lucky. Our minds happen to predict things in a way which matches reasonably well with the way nature works. Our reasoning (e g, that the book will not fly out *because* books have not been known to fly) is fallacious but fortunately the conclusion which this fallacious reasoning leads to is usually correct.

Why are our minds programmed so as to match with nature tolerably well? I am unable to answer this fully but a minimal reason may have something to do with *natural selection*. A human race whose mind is not thus programmed would not survive for too long. The *fact* of our survival suggests that the programming happens to be reasonably good.

III

To return to the debate at hand, one major grouse Kurien has about my paper and response, referred to above, is that I define some terms formally but not all. He seems to suggest that it should be either one or the other. But clearly it is not possible to give formal definitions of all words, if for nothing else, simply because we need words to define words. Hence, we may deduce that Kurien is against any term being formally defined. Given that he would, presumably, not want to defend such a position, he has to give up his one-or-the-other axiom which is the source of the problem.

It may be claimed that what Kurien was arguing was simply that I should have used the term 'exploitation' in a particular sense. Note first that in stating my formal claim about efficiency in agrarian economies, I was using the term 'exploitation' quite precisely, though I was not using it in the sense in which it is used in the Marxian literature. Kurien is insistent that I ought to use it in that sense. But that is quite absurd because I do not *have* a theorem to offer about Marxian exploitation and I cannot commandeer one because Kurien wants it.

It is true—and for this I am grateful to Kurien—that I should have clarified even more the sense in which I was using the word exploitation. But it is unfortunate that Kurien has refused to go beyond semantics to understand the theorem in question. There is nothing ambiguous in it though its popular restatement can be open to more than one interpretation. This is precisely the reason why formalism is so important in economics.

One source of Kurien's confusion lies beyond my paper. It stems from his misunderstanding of Pareto.

Kurien writes: "Even if ninety-nine persons will favour a change in terms of their personal preferences one person can say: 'Oh, no, but that change will make *me* worse off in terms of my preferences and going by what Vilfredo Pareto has taught us, I win'."

The above quotation makes it clear why Kurien believes that a commitment to Pareto implies a commitment to the *status quo*. But that is utterly wrong. To take Kurien's other example, suppose a change which would hurt a rich moneylender a little and help 99 poor borrowers is being considered. Does the Pareto criterion recommend against such a change? The answer is an emphatic No. It is true that the Pareto criterion does not recommend the change either. The failing of Paretianism is not that it is *status quoist* but that it is too reticent—it makes no recommendation wherever there is a conflict between agents.

But this is easily rectified by using what is called a *Pareto-inclusive* rule. Most applied welfare economists, in fact, use such rules. A *Pareto-inclusive* criterion is one which accepts a Paretian recommendation where such a recommendation is forthcoming and supplements it with other value judgments wherever the Pareto rule has nothing to say.

I write all this by way of clarification, because, to repeat what I have said before, the validity of the theorem (discussed in my 1988 *EPW* paper) which relates a moneylender-landlord's power of extortion to the optimality properties of the equilibrium does not depend on our view of the moral appeal of Paretianism.