The Cambridge Controversies in Capital Theory

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This is a summary of my book *The Cambridge Controversies in Capital Theory*: *A study in the logic of theory development*, Routledge, 2002.

The main purpose in writing this book was to puzzle out how a debate in which the best economists of the 1960s and '70s participated left little or no trace. This is all the more amazing since that debate is one of the few examples in economics of a common effort in which a negative result was proved beyond any possibility of doubt. One of the ways of stating that result is to say that the quantity of capital cannot be measured independently of the rate of interest. The opposite had been considered to be one of the main tenets of neoclassical economics, and is still the dominant view. According to its critics at the time, this "falsification" (I use quotation marks because the result was not an empirical falsification but a mathematical counterexample, which is stronger) of one of the most important predictions (or rather, logical consequences) of this tradition ought to have been the downfall of the house that Léon Walras, Carl Menger, William Stanley Jevons and Alfred Marshall built in the last quarter of the 19th century. At the invitation of the editor of BARK, Richard L. Epstein, I will try here to present that material in such a way that it is accessible both to philosophers who are not versed in economics and to economists not versed in philosophy.

The reason for delving into the intricacies of the Cambridge controversy was that no one seemed to have given (or even tried to give) an explanation of why a controversy that raised so much dust in the leading economics reviews in the 1960s and '70s petered out and fell into oblivion. Amongst the results of the debate were "hard," mathematically sound proofs produced by Nobel-prize winning economists showing that it is impossible to use a single aggregate measure of capital. Nevertheless, all growth models and many others continue to use this aggregate K still, as if no such results had been found. But the matter was not so clear-cut. Some of those who took part in the debate reminisced that nobody seemed to know what it was about. That gave me the suspicion that methodological differences were at the root of the confusion. More specifically, my idea was that the high level of abstraction of the debate had something to do with it. Those hypotheses proved to be correct, but in a way that was very different from what I thought initially.

But first: what was the debate about? Despite the confessions of confusion by participants, it is not so difficult to tell afterwards. As so often in economics, what happened in the controversy was that an old argument by an old economist

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from the past was dusted off. The economist in question is Eugen von Böhm-Bawerk[2] and the problem is how capital can be measured, where by "capital" is meant all means of production except labour and land. Böhm-Bawerk thought the answer was time, that is, the longer a production process lasted, the more productive it would be. However, even at the end of the 19th century, when he proposed this solution, economists knew that one aggregate time index (the average period of production) for all capital equipment did not do the job. One production process may use one very costly machine during the first 6 months of a production process (for the extraction of rough diamonds, for example) after which the unfinished product is turned into a half-finished product entirely by hand for 3 months, after which a machine takes over for the final 3 months to produce a perfect jewel. A different production process extracts diamonds by hand in a process that lasts 6 months and then elaborates them into jewelry in a fully mechanized process that takes 6 months. Which of the two is more capital-intensive? Time is not the solution to this problem; it is part of the problem itself. The reason is as follows.

Investing in equipment carries with it what economists call alternative costs. If I buy or construct a machine to produce A, my costs are all the gains that I could have realized with the funds or equipment if I had used them differently (for instance in buying a lottery ticket). The longer the time period involved, the greater the *opportunities* for gaining (or losing) more than I stand to gain (or lose) with the investment actually realized. In a (purely hypothetical) complete system of spot and futures markets with complete information, this opportunity cost of capital is fully expressed in the rate of interest. In a complete market system without full information, the rate of interest also expresses risk. In a market system such as we know, where many products have no futures market, nor even a spot market, the rate of interest is only a very imperfect index of opportunity cost (and hence of value). But let's leave incomplete markets, risk and uncertainty aside for the sake of argument. That is, let's assume that there are spot and futures markets for everything, and that economic agents have full information and act in riskless conditions. Even in such an idealized system, the quantity of capital engaged in my two hypothetical processes depends on the rate of interest. At a high (compound) rate of interest, using machines during the first and last phases of the first process entails a higher cost than the second process, where capital is engaged during the first half only. If, however, the interest rate is low, it may very well be the case that the second process is the costlier one. Conclusion: The time profile of investments is one of the factors that determines the value of the stock of capital, and a higher rate of interest may accompany a more capital-intensive production method. In the jargon of the debate, this is called *capital reversing*; returning to a negatively correlated capital-intensity is known as the *reswitching* of production techniques.

I give an example: If we assume for the sake of argument that we have a model in which we can describe changes in time, capital-reversing consists of a

[2] Rather than burden this paper with excessive references, I'll direct you to my book for those.

rise in the interest rate that goes together with a rise in the amount and the cost of capital; reswitching is the possibility that, if the rate of interest rises further, the amount and the cost of capital diminish. These are two possibilities that neoclassical economics had never taken into consideration. In order to give an impression of the shock the discovery of these theoretical possibilities caused, think of the rate of interest as the price of means of production, such as machinery. Would you buy more machinery if it cost more? If your answer is "It depends," you are on the right track. The conditions on which this depends is what constitutes the bulk of the controversy, the neoclassical economists trying to find conditions in order to save their theoretical building, their critics trying to give arguments why conditions that went against the letter and the spirit of neoclassical economics were very likely to obtain. Roughly speaking, according to neoclassical economics, individuals (such as investors and entrepreneurs) try to maximize their utility using all the relevant information about possible gains, costs and opportunities.

One of the curious things of the debate is that no one ever produced any real empirical argument. The whole controversy remained at the level of *a priori* probabilities and the plausibility with which conditions favourable or unfavourable to the neoclassical case might be found (economists consider something to be plausible if they can give purely hypothetical arguments for it that they themselves find convincing; the reader ought to get used to the idea that most of economics is not an empirical discipline). This is very briefly the theoretical background of the debate. The doctrinal background, so to speak, was that the then (and still now) dominant tradition, neoclassical economics, had completely neglected the problem of the measurement of capital. Yet, in the theory of production, an aggregate index K was used, among other things to determine the rate of interest and the distribution of income. In the 1950s followers of John Maynard Keynes, who, moreover, had left-leaning political ideas, finally began to analyze an aspect of the economy that their master had deliberately left unanalyzed: the structure of production. Most of them taught in Cambridge, England. In economics the distinction between the short and the long run is not a matter of clock or calendar time but coincides with the point at which the structure of production can be changed, i.e., when investments come into play.

For instance, the Keynesian recipe of stimulating consumers' demand is a typically short-run measure; it serves to get rid of existing stocks of unsold products and to activate existing but under-utilized machinery and idle workers. As soon as entrepreneurs start to invest in new machinery, economists speak of the long term. The relevance of the distinction may be gleaned from the possibility, in this example, that as soon the government-stimulated demand ceases, consumers may return to their old habits. The new production lines for whose creation financial and physical resources may have been taken away from other means of production may turn out to be unprofitable because of lack of demand. What seemed an effective short-run measure for getting out of a crisis in the long run has only aggravated it.

“In the long run we’re all dead,” said Keynes. “Yes, but we ought to complete Keynes’ short-run analysis with a theory of the long run, i.e., a theory of production or capital,” thought Joan Robinson, Keynes’ most critical and by far most brilliant follower. So she set out, in the early 1950s, to construct this theory of the long run, which should have encompassed Keynes’ “general” theory as the special (short-run) case it really is. During the 1930s Keynes had done battle with what we would now call neoclassical economists, and Robinson and other followers of his inherited their hero’s critical attitude. Neoclassical models of capital-using production all used a single aggregate index K that was implicitly supposed to vary inversely monotonically with its “price,” the rate of interest. In the course of the development of her capital theory, Robinson came across cases where the quantity of capital did not vary inversely monotonically with the rate of interest. She hit upon theoretical examples of reswitching and capital reversing. Yet, whereas she could have used these results as ammunition against neoclassical economics, she brushed them aside as anomalies; she failed to realize the impact her results could have had as an instrument of criticism of the dominant mainstream. David Champernowne, another Cambridge, UK economist, came up with similar results at about the same time. Even though he was more immediately aware of their critical potential, instead of mounting an attack on classical economics, he tried to find the conditions under which a positive correlation between rate of interest and capital-intensity could occur.

In my book I present a detailed discussion of the two articles by Robinson and Campernowne and the other most important publications of the capital controversy. Those case studies also supply explanations of the mostly mathematical and often very intricate proofs. Each batch of case studies is followed by a chapter in which a methodological analysis is given. In this way, the complete methodological apparatus is gradually developed along with the discussion of the history of the debate.

Champernowne’s strategy turned out to be exemplary of the behaviour of all economists who subsequently became involved in the debate. Let’s, just for a moment, adopt a Popperian perspective of economics (many economists openly declare that what they are doing is empirical science and that falsifications should be taken seriously). If one of the basic theorems of neoclassical economics is convincingly falsified (even in a purely hypothetical, non-empirical, but plausible model), then, by the falsification viewpoint, neoclassical theory ought to be abandoned, at least as a general theory. The articles following the results published by Robinson and Champernowne, both Cambridge, UK, economists and their neoclassical colleagues, who were mainly located in Cambridge, Massachusetts, follow this approach.

The most famous of the latter tribe are Paul Samuelson and Robert Solow, both of whom were awarded the Nobel prize, though not for their involvement in this debate. They and others produced counterexamples similar to those given by Robinson and Champernowne. The conclusion of the UK tribe was that neoclassical economics did not provide a sound basis for a theory of production. They failed, however, to come up with a more satisfactory alternative theory. Their transatlantic counterparts, on the other hand, never drew the conclusion that they should give up their theory, or at least the part of it that dealt with production. What they did instead was to construct ever more complicated models that showed the conditions under which the so-called anomalous cases could occur. So far, there is nothing wrong with this. What they added to their analysis, however, were completely *a priori* judgments on the probability with which these cases could occur. Now, for an empirical science, as they claim economics to be, probability judgments ought to be based on empirical (econometric) research. But it is exactly that type of research that shone by its absence.

The UK critics of neoclassical economics also used this *a priori* type of reasoning. Of course, they differed from their mostly American colleagues by arguing that the anomalous cases were very likely to occur. But they, too, failed to do give empirical arguments. "No wonder the debate was inconclusive," is what you might conclude. But that is not what was at stake. The fundamental result was that the neoclassical production model could not deliver what it was thought to deliver: a monotonically inverse relationship between rate of interest and the quantity of capital. This important theoretical conclusion, which was reached with the help of rigorous, mathematical counterexamples, was completely ignored in all subsequent work, both theoretical and empirical.

All this led me to believe that this *a priori* method of reasoning is deeply ingrained in economics. Critics and defenders of the neoclassical paradigm alike use the same method of purely theoretical reasoning in which the consequences of the "laws," or predictions ("theorems" they often call them) are maintained while the antecedent of the "law-like statement" ("conditions") are adapted. Economists say they derive conditions for their theorems. This reminded me of the method of decreasing abstraction. The difference is, however, that the most "concrete" or factual models, those describing empirical reality, are never reached. In the philosophical literature I found the works of the Polish philosophers of science Wladislaw Krajewski and Leszek Nowak, who seemed to give the best general description of this reasoning method.

Very briefly, according to Krajewski, in a process of decreasing abstraction the idealizing conditions in the *if*-part of a (pseudo-) law-like statement are replaced, step by step, by more realistic conditions, and the prediction or the *then*-part of the (pseudo-) law-like or theorem is adapted accordingly. The major difference between Krajewski's analysis and the facts of the capital debate is, however, that in the debate the conclusions ("theorems") were not adapted in the light of changing conditions. The economists, neoclassics and their critics, maintained the *then*-part of the conditional law-like statement unvaried and looked for alternative conditions under which it could be maintained.

Alan Musgrave, apparently without knowing Krajewski's or Nowak's work, gave an almost satisfactory analysis of this procedure in an article that discusses Milton Friedman's 1953 paper "The methodology of positive economics".[3] Musgrave discusses the various strategies that economists may use when faced with a counterexample in terms of domain-decreasing but precision-increasing terms. Translated to the debate: An attempt to save a "theorem" from a counterexample may lead to a smaller domain of application but a higher precision in what is said about that domain. The only defect of Musgrave's article is that is based on the (rather endearing) idea that economists change their conclusions following empirical falsifications. Musgrave was (and still is) so imbibed with the Popperian idea of the importance of empirical refutations that he fails to see that practising economists could not care less. I replaced Musgrave's emphasis on empirical counterexamples by counterexamples in general (all counterexamples in the debate are theoretical) but took over his description of the different moves (I prefer to call them strategies) that are possible in reaction to a counterexample ("anomaly").

I completed my methodological apparatus by borrowing the model of theory development in mathematics of another of Karl Popper's students, Imre Lakatos. His book *Proofs and Refutations* applies perfectly to the debate; many of the strategies by these economists are just what Lakatos called "monster-barring."

Now, what is the explanation for the apparent aimlessness and sense of confusion on the part of those who debated? All of them, both critics and advocates of the neoclassical model, got lost, quite literally, in the maze of the often very intricate technical and mathematical strategies that they used to argue for or against the neoclassical production model. And when they occasionally wrote down their methodological reflections, they were equally confused. They said that they had been guided by facts, but what really fuelled the debate were purely hypothetical, often very intricate mathematical conditions. In brief, these economists behaved like the sleepwalkers of Arthur Koestler's book with that title. What really took place was that by decreasing (or increasing) the level of abstraction of their models, these economists lost sight of the general or global problem, which was the question of how we can model capital-using production processes in a satisfactory, non-circular way. The non-circularity is important, since any measure of the amount of capital needs as one of its ingredients the rate of interest, and the rate of interest depends on the amount of capital. Almost all of these economists also suffered from the illusion that they could use mathematics as a neutral instrument. Some very nice cases in the book show the contrary.

The extent to which this "blindness" was operative (and there is no reason to think that economists have changed their habits since) is illustrated by the fact that the only economists who found a counterexample and fully realized its importance for criticizing neoclassical economics was Piero Sraffa. In 1960 he published a little book, *Production of Commodities by Means of Commodities* in which he clearly explained the far-reaching consequences of a positive correlation

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[3] See "On models and theories, with applications to economics" by Richard L. Epstein in this issue of BARK for references and a discussion of that paper.

between the rate of interest and the quantity of capital. But even though he was a central figure in the circle of followers of Keynes in Cambridge, UK, his results were not used in the battle against Cambridge, MA. Only later was the importance of his result recognized, which led to a falsification of the history of the debate: By some he was wrongly portrayed as one of the people who gave the kick-off for the debate.

Some participants and some of those who report on the controversy explain the confusion with an appeal to the different ideologies of neoclassical and left-wing Keynesians. That misses the point entirely. Everybody uses the same models and types of argumentation and the confusion arises out of technical factors that begin to lead an almost autonomous life. In many cases this is reinforced by the fact that the mathematics takes over from the economics.

My analysis can explain a number of historical ironies. One is that Joan Robinson hit upon reswitching and capital reversing—later so powerful in getting the neoclassics to accept the criticism of their aggregate approach—but dismissed them as curiosities. The non-influence of Sraffa was already mentioned. Champernowne's discovery, in the early 1950s, that reswitching could only be ruled out by an additional assumption was neglected by others, who rediscovered the same results independently. What was perhaps Champernowne's most important observation, namely, that reswitching is only problematic in a dynamic context, was only admitted by a few former participants long after the debate had finished and without referring to Champernowne. The importance of this for my methodological analysis of the debate is that Champernowne's comment is tantamount to saying that everybody was using a model that abstracted from dynamics in order to describe a dynamic phenomenon. In other words, economists used the wrong level of abstraction; in the models they used to describe the dynamic phenomena of reswitching and capital reversing they had "idealized away" or abstracted from the passage of time.

Another irony was that the neoclassical economist Samuelson woke up sleeping dogs by calling attention to reswitching, thus achieving what Champernowne and Sraffa had failed to achieve: to center the debate on one of the weak points of the neoclassical model. These and other strange-looking historical facts are explained by the participants' obsession with the local, technical puzzle-solving that was part of the reasoning back and forth between more general models and special cases, and which can be so well described by the Krajewski-Nowak-Musgrave-Lakatos analysis. The economists involved were sleepwalking in a maze of "local" puzzles rather than being guided by instant rationality (as Lakatos calls the caricature that scientists always act rationally and immediately see all the relevant aspects of a problem) at the "global" level.

There is a lot of talk in the debate about "deriving the conditions" under which certain "theorems" hold. This suggests that the theorems are deductively derived from the conditions, as should be the case in the philosophical model of reasoning using valid deductions from laws (the *deductive-nomological model*). What really went on, however—or so I argue—is that economists confused two relationships: explanation and reduction. They cannot be blamed, since they follow the rather diffused habit of considering the two symmetrical: The logical structure of explanations and reductions are identical and every explanation is a reduction in reverse.[4] A higher-level theory (for instance, a more general theory) explains a lower-level (for instance, a limited-domain) theory and the special-case theory can be reduced to, or be logically "re-connected" to the general theory with the help of the correct conditions. Krajewski is aware that there is a problem with this relationship in an idealizational context. According to the Polish model of idealization, idealizing models or theories are logically entailed by, or follow deductively from, more factualized or less idealized models (I use "factualized" for referring to models whose conditions are closer to the facts). A general intuition is that idealizing theories (the standard example of such a theory is Newtonian mechanics, which stipulates regularities of point-masses, which are entities that do not exist) explain factual (including empirical) models. This goes against the so-called symmetry thesis, which says that every explanation is a reduction in reverse (and vice versa). Krajewski signals the problem without giving a solution.

My solution is to accept the intuition that idealizing theories are somehow prior to factual models but reject the idea that we can model this priority in terms of the (deductive-nomological) logic of explanation. What I propose instead is to model the relationship as one belonging to the logic of discovery: Idealizing theories serve as a heuristic for the construction of factual models. But that seems to substitute one problem for another. Using the work of Matti Sintonen (who applies a logic of questions to the analysis of scientific explanation) and Mario Bunge, I arrive at a reconstruction of the relationship between idealizing theories and factual models in terms of presuppositions: idealizing models presuppose idealizing theories. The logical analysis of presuppositions is that if A presupposes B, B is a necessary condition for A, that is, A implies B. This has the advantage that we can keep the deductive relationship that Krajewski proposes: Idealizing theories follow deductively from factual models.

This led me to propose a behavioural hypothesis for what drove economists in the debate: Despite their talk of deriving conditions, what they were really trying to do was to construct their models in such a way that their idealizing theory could be deductively derived from the conditions. More and less general cases coincide with what Krajewski (following Niels Bohr and Popper) calls corresponding and corresponded models. In brief, economists in the capital debate follow a correspondence strategy. The unity and heuristic power for which neoclassical economists are envied by their critics lies in the fact that their basic, idealizing theory can serve relatively easily as the presupposition of many models. The difficulties they nevertheless ran into in the capital debate are caused by the fact that they stick to a wrong level of idealization: the exclusion of time and dynamics. Time is a fundamental factor without which capital-using processes cannot be modelled without running into reswitching and capital reversing. In a

[4] See, for example, Richard L. Epstein, "Arguments and explanations", BARK 1, 2001.

dynamic context these possibilities are no longer anomalies but perfectly normal "phenomena" (the quotation marks serve as a reminder that in the debate we are never dealing with empirical cases).

There are two other consequences of the hypothesis that idealizing theories should be seen as presuppositions. First, it gives a firm logical basis for what Lakatos describes as a convention. Namely, a particular hard core is not accepted as a matter of convention; its heuristic power lies in the opportunities it offers to find models from which it can be logically derived. Second, the logic of presuppositions is more satisfactory than the unexplained "logic of tendencies or capacities" that Nancy Cartwright invokes for modelling the relationship between fundamental and phenomenological laws.

I use only the part of Sintonen's analysis that concerns syntactic presuppositions. That seems all right for the Cambridge controversy since all the proofs, counterexamples, etc. are formulated in mathematical models. An application of my methodological apparatus to other, less formalized, discussions in economics might benefit from Sintonen's discussion of pragmatic presuppositions. Such an extension of my analysis would also provide a welcome opportunity to test its robustness vis-à-vis different domains of application.