The Stockholm School After Fifty Years: A Conversation with Lars Jonung

E.E.J.: During the 1930's the Stockholm School offered an approach to macroeconomics and stabilization policy that was close, in many respects, to that later termed Keynesian. Yet the Stockholm School appears almost to have passed into oblivion. As a contemporary Swedish trained economist, can you suggest why this has been the case?

Jonung: Although the Swedes had written a number of studies with a very strong Keynesian flavor, in retrospect, they made the fundamental mistake of not publishing their work in English prior to the publication of the General Theory in January 1936. Had they done this, most likely, the General Theory would then not have appeared so revolutionary because some of its basic ideas would already have been known to English speaking readers. This is the essence of Bent Hansen's account of Myrdal's and Ohlin's work prior to the General Theory, which it seems relevant to recall in this connection. Hansen wrote:

"No one who has studied Myrdal's and Ohlin's monographs, presented as Supplements 5 and 7 to the Main Report II of the Committee on Unemployment (1934) can possibly deny that these two authors, as early as 1933-34, had a fairly good understanding of the mechanisms of that kind of macroeconomic process which we know under the name of multiplier process and which conventionally is associated with Keynes' "General Theory." Definitional subtleties apart, it is also undeniable that both Myrdal and Ohlin discussed such a macro process in terms of national income aggregates with functional forms very similar, if not identical, to those employed by Keynes two years later in the "General Theory." To that extent, Myrdal and Ohlin did indeed anticipate Keynes' "General Theory." And their understanding of the importance of multiplier processes of reactions speeds and lag structures and of leakages related to import and tax payments was certainly more advanced than the "General Theory."

E.E.J.: Was there no interest on the part of the Swedes to communicate their ideas to scholars and policy makers elsewhere?

Jonung: Indeed there was some interest. Ohlin engaged Brinley Thomas in 1934 to translate his report for the Unemployment Committee into English. However, Thomas was prevented from carrying through the agreed upon translation. As matters turned out, the report was not translated until some forty years later by Hans Brems and published in "History of Political Economy" in 1978. Ohlin did however deliver the Finlay-lectures in Dublin in 1934. These were published three years later in the Economic Journal under the title "The Stockholm Theory of Saving and Investment." At that time Ohlin made some qualifications and "added a comparison with and a critique of Keynes' recently published General Theory." It seems worth recalling Ohlin's reflections about what

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might have happened if he had published his work earlier:

I thought that it was a bit distressing that I was not ready to publish the Dublin lectures before Keynes's book was out. Otherwise, economists around the world who found Keynes's presentation sensational could not have avoided noticing the very large similarities concerning the theoretical foundation and the policy conclusions. But such a reaction was perhaps at least partly a result of vanity: from the Stockholm School and my side, because it is uncertain how much faster these ideas would have made an international breakthrough by an earlier publication.

E.E.J.: What about the great Gunnar Myrdal, whose contributions are perhaps better known than those of any other of your countrymen?

Jonung: Even Myrdal was remiss in this regard. In fact, in his 1982 book "How is Sweden Governed?" he devotes a whole page to explaining his "inattendath," that is, his propensity for publishing only in Swedish in the 1930s.

"I wrote in Swedish for Swedes and to some extent for Scandinavians, but I had little interest to have my thoughts made known internationally. I remember that I got an invitation from the International Labor Bureau for translating my report on the budget of 1934 and have it published in their journal, which I turned down. I was busy with work at home and I did not want to bother about proofreading a translation. For similar reasons I had turned down Heckscher's nice proposal of arranging a translation of my doctoral thesis of 1927."

Myrdal got some of his work published in German in the 1930's, thanks to translations made by a German friend Gerhard Mackenroth, who was privadozent in Halle. Incidentally, it was Mackenroth, struggling with the translation of Myrdal's Swedish article on monetary equilibrium, who suggested the use of the term ex ante-ex post. Later, after the publication of General Theory, the Stockholm School economists made some of their work available in English. Erik Lundberg (1937) wrote his dissertation in English. Lindahl and Myrdal published major contributions in English immediately prior to World War II. Much of the content of the two books by Lindahl (1939) and Myrdal (1939) had been known in Swedish since the end of the 1920s or the early 1930s. However, at that time the Keynesian revolution had started to make an impact in the academic world and consequently Swedish work, being in the backwater of the General Theory did not attract interest. Still, several contributions of the Stockholm School have not yet been translated, notably Myrdal's (1927) and Svensson's (1936) dissertations.

E.E.J.: From a methodological standpoint how does the Swedish approach compare with the Keynesian?

Jonung: The most critical point is that the Swedes aimed at analyzing sequences of events by stressing differences between expectations and outcomes. They combined this dynamic approach with cautious reasoning. Their catastrophic approach was reflected in a disbelief in the existence of stable relationships between economic aggregates. Consequently, the Swedes were sceptical of such things as constant multipliers, stable propensities to consume or save, etc. This was particularly the case for Lundberg and Ohlin. Lundberg's (1937) dissertation is a prime example of this critical attitude towards equilibrium analysis based on formal models. Specically, Lundberg is sceptical of Keynes' use of equilibrium analysis or multiplier analysis. In his opinion, this approach is too static.

E.E.J.: Is their skepticism about equilibrium approaches related to their pioneering of expectations theory?

Jonung: While expectations and perceptions played an important role in their approach, the Swedes did not develop a theory of expectations formation. Instead they ended up with very "open" and indeterminate systems in which "anything could happen." Their analysis of economic processes tended to develop into "verbal story-telling" in which there was no end to the story, i.e. there was no equilibrium solution to the system, because expectations, and thus behavior and attitudes, were subject to continuous revisions and changes as new outcomes and development were registered.

It would seem that the strength of the Keynesian approach became apparent after Hicks' 1937-contribution. The Keynesian model was built as a static equilibrium model with no explicit role for time, whereas the Stockholm approach tried to capture revisions of plans. This offers the advantage of simplicity, in contrast with the Stockholm approach, which required mathematical tools outside the reach of most economists at that time.

E.E.J.: Does it seem reasonable to infer that the very openness of the Swedish model might have been among the reasons why they were less than successful in attracting followers to establish a genuine school.

Jonung: Certainty, that was a factor. Because the Swedes did not have a unifying model to rally around, they were less able to develop a closely knit school as did the Keynesian school. Instead, there was a tendency among them to develop taxonomies of their own. The simplicity and cleanness of the Keynesian model compared to the complex and "open" Swedish approach is no doubt a major reason behind the latter's disappearance.

E.E.J.: A related question concerns the apparent disinterest of the Swedes in empirical work. Why so little interest in empirical work for testing their theories?

Jonung: I can suggest at least three reasons for this disinterest. First, there were no data available to them that related to the central building-stones of their approach, plans and expectations nor did they make any serious attempt to obtain such data. Neither did they consider the empirical significance of their "period" concept, which was so central to their thinking, by undertaking to analyze the length of the proper period empirically. Second, their lack of a formal analysis handicapped them in the development and design of econometric tests. Again, their cassistic view proved adverse. Finally, their "parameter pessimism," that is, their disbelief that stable behavioral relationships could exist, seems to underlie their disinterest in empirical work.

E.E.J.: Does memory serve correctly that a national income study was undertaken in Sweden at about the same time as those of Colin Clark in England and the National Bureau studies in the United States?

Jonung: Yes, several of the most prominent members of the Stockholm School took part in an ambitious national income study in the early 1930s financed by the Rockefeller Foundation. Lindahl designed the theoretical framework for the income studies and Myrdal and Lundberg worked with him. It is, perhaps remarkable that this early empirical work did not inspire them to use the data collected. The National Institute of Economic Research was started in 1937 with Lundberg as its first director. The Institute, however, did not develop into a research center that adopted econometric techniques. Attempts were made by Lundberg at the close of the 1930s to start econometric studies. They were unsuccessful, however, for various reasons.

E.E.J.: Did this change later—say, after World War II?
Jonung: No, the Stockholm non-technical casuistic approach permeated the activities of the Institute into the post World War II period. Indeed, even into 1980s. The Institute has not yet developed any econometric model of the Swedish economy. By comparison the key concepts of the Keynesian model were easy to operationlize. As soon as data were forthcoming, Keynesian models became the subject of the rapidly growing branch of economists. The Swedes fell behind this development.

E.E.J.: Even with the strong predilection for econometric testing by contemporary economists, this research methodology is not essential to the development of a school. Did not Wicksell, Cassel, Lindahl, and Lindberg leave behind a legacy of economic theory on which their students could build?

Jonung: Unfortunately, Swedish economists stopped at an early stage, that is, already in the 1930s, the work of developing the theoretical tradition of the Swedish approach. There were several reasons for this. First of all, almost all of the young economists were attracted into other careers or fields of work. Gunnar Myrdal for example, left the study of macroeconomics around 1934 when he got involved in the population question. He only returned to monetary economics for a short period in connection with the translation and publication of Monetary Equilibrium in 1939. In the 1940s he published An American Dilemma, served as a minister in the Social-Democratic government, worked for the United Nations and turned later to development issues. Similarly, Bertil Ohlin became involved with Swedish politics, becoming leader of the Liberal Party and not returning to academic work until late in his life, when he got a new interest in the development of Swedish economic thought. However, he did hold his chair in Stockholm until 1963. Other members of the School also left for non-academic careers. Dag Hammarskjöld was working for the Riksbank already in the 1930s. There was no academic position available for him. Alf Johansson also became politically active. Erik Lundberg and Ingvor Svennilson had distinguished academic careers but did not further develop the theory of the School. Svennilson undertook studies of industrial economics and economic growth at the IUI, the research institute of the Federation of Swedish industries. Both he and Lundberg were engaged in various administrative duties. Erik Lundberg moved to the National Institute of Economic Research and continued with macroeconomic issues, publishing books and in the 1950s and 1960s. These articles, however, dealt with empirical not theoretical issues.

E.E.J.: Was not Lindahl associated with Uppsala?

Jonung: Yes, Lindahl was. He became professor at Uppsala and was thus a major exception by remaining at the University. His efforts were aimed at developing the methodology of the Stockholm School. However, he published no major work after 1939. Meanwhile, at the other universities, among them Lund and Gothenburg, the ideas of the Stockholm School did not catch on. At Lund, Johan Akerman was openly hostile to Stockholm School ideas. Indeed, he became a leading Swedish critic of the Stockholm School.

E.E.J.: Is the failure of the Swedish School also perhaps related to the Swedish University system?

Jonung: Yes, I believe so. The exodus of young talent is partially related to the incentive structure of the Swedish system of higher learning. Generally, up until now a department of economics had one or two professors and one or a few doctors. Young graduates, even if they were extremely bright, had only a small chance of staying at the university after finishing their doctoral thases. Also, economics as a discipline has had a weak academic base. In the 1930s, economics was a subject at only five institutions, Uppsala, Lund, Gothenburg, Stockholm and the Stockholm School of Business and Economics. It was difficult for such a small set of universities to maintain an independent line of thought in macroeconomics. Nor did Swedish economists, like many of their Austrian colleagues, emigrate to bring their ideas to the U.K. and the U.S.

E.E.J.: What of the textbooks that so often serve as a vehicle for the transmission of ideas? Is there not a counterpart in Sweden to the Samuelson or Ackley volumes of early macroeconomics fame?

Jonung: No; there's no textbook version of the Swedish approach, although for a long time, the reading list for undergraduate courses included Landberg's 1953 book. It is Keynesian textbooks imported from abroad that dominated the reading lists in Sweden at least from the 1960s and onwards. Indeed, the economics profession in Sweden quickly became Keynesian after World War II. Anglo-American ideas were not greatly at variance with Swedish views, which probably contributed to this transition.

E.E.J.: Given this affinity between Swedish scholars and Keynesian ideas, can it really be said that the Stockholm School "failed"?

Jonung: Some would, no doubt, conclude that the Swedish School was a failure. But, in my opinion, this is a faulty judgement. Elements of the Stockholm School have been incorporated into mainstream economics. The distinction ex ante-ex post is part of many textbooks. Hicks' "Value and Capital" is influenced by Lindahl's dynamic method. The sequential approach to macroeconomics of Lindberg, Lindahl and Myrdal and the stress on macrodynamics is still attractive as a research program. The work of the Swedes in the 1930s is thus now living as a part of present-day economics. This is a central theme that I expect will emerge when Swedish economists congregate in Stockholm in August, with other economists from around the world, to commemorate the anniversary of the "Swedish approach" which was presented by Ohlin to an international audience fifty years ago.