

## **‘I believe that the construction of an alternative to the mainstream is possible’**

Interview with Alessandro Roncaglia



Alessandro Roncaglia is one of the most distinguished figures in contemporary Italian economics and a leading exponent of the Sraffian school of thought. Currently, Professor Emeritus of Political Economy at *Sapienza University of Rome*, Roncaglia has spent over four decades bridging the gap between rigorous economic theory and the historical evolution of ideas. A former student of Paolo Sylos Labini and a close associate of Piero Sraffa, Roncaglia’s work is characterized by a profound critique of neoclassical theory. He is perhaps best known internationally for his seminal work *The Wealth of Ideas* (2005), a comprehensive history of economic thought that argues against the ‘whig history’ view of economics. Instead of seeing the field as a linear progression toward a single truth, Roncaglia demonstrates how different competing paradigms have emerged to address specific historical problems. Roncaglia is a member of the Accademia Nazionale dei Lincei and served as the President of the Italian Economic Association. His work is a cornerstone for scholars seeking a pluralistic, historically grounded alternative to mainstream economic models.

*When did you become an economist, and specifically, a non-orthodox economist? Who influenced you?*

The events of life! I was initially undecided between economics and Ancient Greek: two very different paths. When I was 17, I was severely beaten by fascists. This experience pushed me toward more concrete matters, and I had heard a wonderful lecture by Paolo Sylos Labini on the economic policy of Fascism. Those two interconnected factors steered my course.

Consequently, I sought advice on how to study economics. At that time, economics was considered a social science, a field that involved demography, sociology, and philosophy. The first advice came from a great-uncle of mine, a professor of commercial law, who told me: ‘Find where Sylos teaches and study there, because the professor is what truly matters, and he is the best’. Then I went to Sylos, and he advised me to choose the degree course in Statistics. He explained, ‘To be an economist, one needs mathematics, statistics, history, and philosophy. You can acquire history and philosophy on your own, but if

you're not compelled, you won't study enough mathematics and statistics'. Incidentally, Statistics was where he had moved to teach.

This worked out exceptionally well; there weren't many required economics exams, so I took additional ones beyond the mandatory list, but crucially, we took many exams in mathematics and statistics, two in sociology, and two in demography. We studied social sciences as an integrated whole. Therefore, my training was, if you like, inherently that of a non-orthodox economist: economics as a social science, not as preparation for an administrative or a financial job.

Once, Sraffa asked me what Sylos taught. I replied, 'Well, he begins with neoclassical theory, the U-curves, consumer demand, those topics. Then he moves on to the Sraffa critique of Marshallian theory, to his own theory of oligopoly, and then concludes with his own econometric model'. Sraffa commented with a laugh, 'First he corrupts students, then he redeems them!' But he did agree, and I too agreed with this 'corrupt and redeem' strategy: we need to teach – especially in the first and second years – the theory that is 'not ours', but also find room for our point of view. Students who have reached the majority age have a right to know that there are different opinions. It would be fair if mainstream lecturers did the same and taught some of 'our theory', but we know how that ended up.

In summary, this was the nature of my early academic formation. In the initial phases, I was also heavily involved in political activities.

*Are we discussing the 1950s, the 1960s?*

No, the late 1960s. Specifically, I participated in the student occupation of 1966 following the death of Paolo Rossi.<sup>1</sup> It was there I met Roberto Villetti, who became a close friend and a professional politician in the Socialist Party. We were followers of Riccardo Lombardi,<sup>2</sup> advocating a structural reforms strategy. I dedicated a significant amount of time to political work.

While I never formally joined a political party, I was involved in a few organizations, such as the Salvemini Movement, where I served on the board when Ferruccio Parri was president. I participated in political negotiations – for instance, those for the formation of the 'Rose in the Fist' alliance between Radicals and Socialists – and in drafting several policy documents and electoral platforms. I was offered the opportunity to run for Parliament twice, and once to serve as a Minister, but it was clear to me that my main interest was research and teaching: political activity is a moral duty, but a different profession from the one I had chosen.

*You declined.*

Yes. Following the Fascist beating I sustained, I find it difficult to travel and being in tumultuous environments, so to speak. Therefore, I prefer working at home and maintain a quieter existence. And I was convinced not to be fit for the ministry I was offered, agriculture.

1. Paolo Rossi was a student in Rome, the first death caused by a fascist beating after the Second World War.
2. A prominent leader of the Italian Socialist Party.

*I see. So, fundamentally, was it Sylos who initiated your path?*

Yes, certainly. Sylos, but also his assistants – you didn't know Mauro Ridolfi<sup>3</sup> – each of us had a sort of tutor. I also had discussions with Michele Salvati,<sup>4</sup> and Luca Meldolesi<sup>5</sup> gave me excellent advice on my book about Sraffa: to add as a first chapter material that I was presupposing as obvious and was dispersed in the other chapters, namely the underlying foundations of Sraffa's analysis. I was explaining concepts like the standard commodity, basic goods, non-basic goods, etc., without providing the groundwork, essentially. And then, Sraffa himself became important.

*Tell us about your period in Cambridge.*

Well, Sylos sent my degree thesis to Sraffa. Regarding that thesis: I had planned to write it quickly because I wanted to get married. So, I looked at the available topics and thought, 'Well, nobody has addressed the second part of Sraffa's recently published book, so I'll focus on that'. I went to Sylos to propose it. He had initially told me, 'Choose whatever topic you like'. He looked at me and said, 'Excellent. Now, first of all you will read Adam Smith's *Wealth of Nations*, then Ricardo's *Principles*, and finally Marx's *Capital* – all three books, mind you!' This task was much more time-consuming than I had hoped when choosing my topic, but decisive for understanding Sraffa, because if you don't have a solid grasp of the Classics, especially their conceptual foundations, you can't truly engage with him. I had anticipated a simpler endeavor. Nevertheless, I worked hard and managed to graduate quickly.

So, Sylos sent my thesis to Sraffa, and I went to King's College for two months during the summer on an exchange program. There, I met Mario Tonveronachi<sup>6</sup> and various friends who remain close, for instance, Gianni Vaggi<sup>7</sup> and Piero Boitani (who is not an economist, but a now famous scholar of comparative literature). I had gone to Cambridge specifically to speak with Sraffa. He welcomed me very warmly. There was an unfortunate incident: I broke my leg playing soccer on the lawn, so I used crutches, and my wife was seven months pregnant – we were quite the sight! It was there that I began working with him, discussing his book, my thesis, and so on.

He had a specific method: he never told you what he thought, but he would criticize whatever you said, and you had to defend your point. Then, we would see how the discussion progressed. The same approach continued when I returned. First, my daughter was born, which forced me to postpone for a year. I returned to Cambridge in the autumn of '71 and stayed continuously until the autumn of '73. After that, I traveled back and forth to visit him.

During that time, I did not formally enroll in the university because Sraffa could not serve as a supervisor; he was the Librarian, not a faculty member. So, he supervised me informally. He made it possible for me to use the library and attend lectures, and I went to him as my de facto supervisor.

3. Ridolfi acted as an assistant to Sylos Labini, to later become Full professor of economics at Perugia University.
4. Now professor emeritus of economics at the University of Milano. Obtained a second degree in economics in Cambridge.
5. Also a graduate in economics at Cambridge in the same year.
6. Later professor of economics in Siena.
7. Later professor of economics in Pavia.

*But in the meantime, did you hold a position in Italy?*

Yes, I had become an assistant in Perugia. Immediately after graduating, I applied for a CNR grant and won it; Luigi Spaventa<sup>8</sup> was on the committee. A month later, Sylos called me and said, 'Spaventa would like to see you'. He wanted to offer me this position – well, 'offer' is not the right word: he always said, 'there will be an open competition for the job, and you should apply', and he had spread the word throughout Italy, but people were terrified. Instead of having 100 competitors, I only had one, a friend of mine, Carlo Del Monte, whom I had told of the competition, also a graduate from Sylos; the others had all fled.

Anyway, I won this competition, so I was first a 'temporary' assistant ('incaricato'), then a tenured assistant, meaning I was on leave of absence when in Cambridge.

*And who held the chair in Perugia?*

Spaventa. Yes, that's why he invited me to participate in the competition. He had read my thesis and decided that I was suitable, so to speak.

*Spaventa's positions are less...*

He changed a lot. He was Cambridge-educated, but after a period in Oxford, he was also very close to Hicks. In his lectures, for example, he used a small text of his on Sraffa's equations, including those for fixed capital, which was entirely inappropriate for a Law curriculum and a first-year course. So, when Spaventa left, and I succeeded him after an interval (first Mario Tiberi, then Nicola Acocella),<sup>9</sup> I dropped that booklet. I was a convinced Sraffian, but you simply cannot start teaching Sraffa to first-year Law students; they hate equations!

By the way, Sraffa had started his university career in Perugia, teaching that same first-year course, and utilizing Marshall's *Principles* as his textbook (he told me, however, that he had very few students; and – we checked – I had in my study the same armchair he used in his times; since we were four in a room with two desks, I used that armchair with wooden library scaffolding on it as my desk).

*Let us go back to your period in Cambridge.*

So, with Sraffa, I initially worked on Torrens, writing the introduction to an Italian edition of Torrens's main book, but always continuing to discuss the interpretation of Sraffa's book. Gradually, I began writing my own book, and he supervised me in this work. I can assert that the ideas are all mine, though he compelled me to specify carefully the form they took, and above all compelled me to change my mind on one specific point. In my thesis, I posited a connection between Sraffa and the early Wittgenstein. And you know that Sraffa and Wittgenstein were friends; Sraffa was the one who made Wittgenstein change his mind from the first to the second book, from the *Tractatus* to the *Philosophical investigations*.

*No, I don't know that story.*

The first Wittgenstein is very structured, very axiomatic. There are elementary propositions and complex propositions. Elementary propositions are true or false, and a complex

8. Professor of economics in Perugia, and later at Roma 'La Sapienza', and member of the Parliament.

9. Both professors at Roma 'La Sapienza'.

proposition is true if it is composed of true elementary propositions, thus providing a clear logical criterion of what is logically true and what is wrong. In all matters in which you cannot have such a clear-cut criterion (religion, esthetic judgements, and so on), well, ‘Whereof one cannot speak, thereof one must be silent’. For a graduate in Statistics with extensive mathematical training, Boolean algebra and so forth, such an axiomatic approach was admirable. I had studied Wittgenstein with Michele Ranchetti, a renowned Wittgenstein specialist, at a school organized by Claudio Napoleoni and Franco Rodano and intended to form young political activists for left-wing parties, which I attended between 1968 and 1969. Ranchetti made us study the first Wittgenstein, discussing it sentence by sentence, so I knew it well, I liked it, and I connected it to Sraffa.

But Sraffa had criticized the first Wittgenstein. In the preface to the second book, Wittgenstein states (more or less): ‘These ideas derive from a very strong change of opinion compared to my previous book. And I thank Russell, etc., but above all I thank Sraffa, because he was the one who made me change my mind’. He used a very charming comparison. He said that after a conversation with Sraffa, one felt like a tree from which all the branches had been torn away, a story we all used to tell in Cambridge. And the general opinion was that ‘Wittgenstein was great, because the trunk had resisted’.

He was lethal in discussions. Once, after we had discussed economics, he said to me, ‘Well, now let’s talk about something else. What do you think of Daylight Saving Time?’ I said, ‘I think it’s useful’. ‘Why?’ He always did that. I tell him something, he criticizes me, I try to respond, he criticizes me again. If you didn’t try to respond, you were finished; you had to hold your ground until the end. Eventually, I said, ‘Okay, I understand, Daylight Saving Time is useless’. ‘Why do you say that?’ After more discussion, I said, ‘Okay, I understand it’s difficult to establish if it’s useful. It’s probably more useful than useless, but further enquiry is necessary’.

That’s how he was. However, the thing he genuinely made me change my mind about, in what I wrote about him, was this link with Wittgenstein. The second Wittgenstein focused on language games. That is, there is no single, unique structure of language, as he argued in the first book, but rather many different language games. Therefore, there are many possible language games. If you shout ‘Brick!’ you are warning the person next to you that a brick is falling. If you point to a brick for your daughter, you are teaching her that this object is called a brick, and so on. ‘Brick’, while you’re on the scaffolding building a house, means pass me a brick. So, the same term has different meanings depending on the context.

Now, for me – and I believe for Sraffa, though he never stated so openly – the same thing happens in economics as well. You have a background worldview that you cannot structure into a single theoretical model – such as the general economic equilibrium theory – and you cannot interpret any specific theory as the foundational structure for the entire economic field. Therefore, Sraffa should not be interpreted in that way, nor is the correct line of development that of Pasinetti (i.e., to build upon Sraffa’s model, though his analysis is very useful at a normative level, for our understanding of the conditions for full employment), nor that of Garegnani (who maintains, more or less, ‘this is the theoretical core, and the rest must rely on it’). These are the basic differences among the three Sraffian schools in my opinion. And so, what I did in my book was to interpret Sraffa in this way: as the solution to a fundamental analytical problem, fundamental for the critique, fundamental for the reconstruction, but a specific theory aimed at a specific issue. So much so that Sraffa’s analysis of prices of production can be interpreted in two different ways: by inserting it into a marginalist conceptual context – so as to criticize the marginalist theory of value and distribution – or by inserting it into a reconstructive context based on the classical-Keynesian worldview. Naturally, there are analytical

differences, meaning you must add the hypothesis of constant returns to move from reconstruction to critique.

*I believe one of the things that interests us, particularly Marc [Lavoie], is always the degree of potential cohesion between post-Keynesians and Sraffians. I imagine that diverse views coexisted even in Cambridge during your time there.*

Cambridge was quite a challenging place. Personal friendships coexisted alongside fundamental theoretical disagreements. For instance, Sraffa had a strong friendship with Kaldor, but he disliked Kaldor's theory of distribution. He was friends with Joan Robinson, but there was a basic disagreement on the notion of equilibrium applied to prices of production. And so on.

The person I connected best with (apart from Mario Tonveronachi, we shared a flat and used to study together) was no longer in Cambridge; it was Jan Kregel, whom I met at a conference in Nice, and we immediately found we had much in common. He edited the English edition of my book on Sraffa. We started discussing, and he spent some time in Italy, then in 1978, he invited me for a semester at Rutgers – and I was his guest there. We began collaborating. I believe his attempt to reinterpret Keynes based on Chapter 17 – on own rates of return – by moving beyond the short-period framework is crucial.

Kregel's interpretation is this: the short period serves to establish assumptions that delimit the boundaries. It's similar to what Sraffa does, 'I take the quantities produced as given, I take the technology as given, and thus I achieve analytical results'. Keynes similarly eliminates problems of technological change in that way, and a series of other minor issues, by postulating the short period. Thus, he uses this hypothesis for closure. Within this closure, he constructs something that holds meaning even outside this closure, just as Sraffa did. This is the schema of entrepreneurial decisions based on the comparison of rates of return, by which he operates in a context where relative prices change over time. This allows for an openness to changes in technology, etc. It points to the factors affecting entrepreneurial choices in a given context.

Now, all of this is compatible with an analysis of Sraffa interpreted as a snapshot of the system at a given moment. This means that with Kregel's interpretation of Keynes and my interpretation of Sraffa, the two things are perfectly...

*Consistent.*

Consistent.

A digression: In Trieste, I became friends with Hyman Minsky, and we discussed a Keynesian–Sraffian manifesto that we had begun writing, in which each of us would set out the fundamental principles of the two viewpoints. And it worked. Minsky is quite an extreme Keynesian; he's not one of the Cambridge post-Keynesians. It worked much better with him than with the post-Keynesians in Cambridge.

*Is there any record of this manifesto?*

I've moved lodging four times since then... I don't know where the paper disappeared! I recall a long walk with Hyman and Esther. We were staying at the Hotel Europa, outside Trieste, where the school was held. We took a short bus ride, then walked all the way to San Giusto, chatting endlessly. Then we would see each other by the sea, and so on. After that interminable chat about this specific topic, I sat down to write, passed him the page, and he made corrections, and we started a sort of bargaining on each sentence; we tried

to involve Jan, and there was Geoff Harcourt. The school ended, and we never finished anything more on it.

*What a pity.*

Yes. But I believe such an alliance is possible. You must first focus on the method. That is, the method must be that of Wittgenstein's language games, or what I call analytical bricks: each brick is a separate entity dealing with a separate problem. Take Harrod. Harrod's three equations form a delightful little model. And you can interpret that in the Kregel way or the Asimakopulos way. In some interpretations, it's not compatible with Sraffa, in others, it is. However, it's an independent brick. Or Sylos's theory of oligopoly, which is another extremely important brick that provides you with certain insights.

So, I believe that the construction of an alternative to the mainstream is possible if you accept this method of separate bricks, with the idea that to merge them, you must look at the underlying conception, which imposes constraints: if you interpret Sraffa as a foundation upon which to build *à la* Pasinetti, you lose compatibility with Keynes. If you interpret Keynes as limited to the short period, you lose compatibility with Sraffa. You must understand which interpretation, which conceptual foundations, allow them to be unified. In the meantime, there is nothing wrong – in my view – with teaching a student the core of Sylos's oligopoly, the core of Keynes, Sraffa, etc., though this work of reconstruction is naturally complex, far from a simple adding up of the different bricks; for each one of them, we must connect it to an underlying common conceptual framework. That's what I'm attempting, but heaven knows whether I'll succeed.

Therefore, an open alliance is possible. The open alliance requires abandoning the idea of 'I am the one who constructs everything'. There must be an open debate, not a fight to see who survives. I believe I have influenced Jan, and I know Jan has influenced me. So, we are moving slowly. We will never fully arrive because life eventually ends, but in my opinion, we are moving in that direction.

*Are you still in contact with Jan Kregel?*

Less than I would like. Mario Tonveronachi, who is continuously in touch with him, acts systematically as the intermediary. We have done some work together recently, including a paper on Keynesian–Sraffian economics, contributed to the festschrift for Annalisa Rosselli. That was the last thing we completed together, but we continue to collaborate. He is also a foreign member of the Accademia Nazionale dei Lincei and he occasionally comes to Rome for participating to some of our conferences. The drawback is that I do not travel.

*The other topic of interest to us was the Trieste School.*

The main organizer was Sergio Parrinello, who aimed at organizing a meeting place for economists outside the mainstream. Thus, he asked Garegnani and Kregel to join him in the organizing committee. They had continuous tension on whom to invite. I was invited systematically either by Parrinello as an applied economist, for the work I had done on the economics of oil, or by Jan as a post-Keynesian, so that I never had the opportunity to give a lecture on Sraffa. Parrinello tried to overcome the tensions by expanding the organizational committee to include Graziani, Becattini, Sylos, and myself, but we couldn't resolve the impasse. It's a shame because the School was a very useful meeting point for many people, both for young scholars who got a taste of these different positions, and for us as a platform for debate.

*It wasn't easy: as a student of Graziani, I recall aspects where, even in the 1980s, after the Trieste School, these conflicts... in many cases the clashes seemed more personal than academic.*

Yes. Of course, it is always possible to get along well personally with people holding diverse theoretical positions. I formed a great friendship with Josef Steindl, and we were the only ones who attended all the schools from the first day to the last. This is because Parrinello always left for two days on his birthday, and Jan occasionally arrived a little late. Steindl had an extraordinary musical culture, and I liked music, so we chatted a lot. Steindl held Kaleckian positions, meaning he was very skeptical of Sraffa. I believe that through discussions, you smooth out these differences. I had reservations about Kalecki, and it is to his credit that I eventually abandoned them.

*Let's turn to future perspectives. Marc [Lavoie] wrote that modern Sraffians and modern Post-Keynesians have much more in common, perhaps, than Sheila Dow suggested.*

I haven't read Sheila Dow, but I believe that's true. The challenge, as we discussed, lies in how to structure this unity. We Sraffians must recognize that there's a heavy role for Keynesian uncertainty – not risk, but uncertainty in the true sense. American post-Keynesians should recognize the need for what we call a theory of value. I think there are important elements, both for a critique of the marginal tradition and for the reconstruction, in both Keynes and Sraffa. I don't see how one can deny either. It all depends on how you recombine them.

Similarly, there are important elements elsewhere. For instance, Sylos's theory of oligopoly, based on the notion of barriers to entry, has been greatly undervalued by both Sraffians and Keynesians. This is a mistake because it offers a different theory of market structures. In Sylos's theory, competition and monopoly are two extremes. The actual situation is always more or less distant from these two extremes and is what you call oligopoly – imperfect competition isn't the right term. But it's a theory that yields results highly divergent from the standard theory of market structures.

Let me give you two examples (I saw you were interested in my political activity). In the theory of oligopoly, the degree of utilization of productive capacity plays a crucial role. So, when the automotive market collapsed with the oil crises in the seventies, the neoclassical prediction was that the price of cars must fall. My prediction was that the price of cars would rise. This is because as the degree of utilization falls, the fixed cost per unit of product increases, and it increases for everyone. Consequently, the behavior of implicit collusion inherent in concentrated or mixed oligopolies leads to price increases. And that is precisely what happened. And, for policy choices, it was quite different whether you predicted inflation or not.

Another case where I managed to make a correct prediction – and one that had political influence – was during the invasion of Kuwait, August 1990. I had written a book on oil: the oil market as a case of trilateral oligopoly. My prediction was that crude oil stocks were more than sufficient to continue operations, so that we could avoid dramatic measures to cut consumption, which would have had a negative effect on activity.

In the same vein, and also based on the theory of oligopoly, at one point the idea circulated that ENI (Italy's energy company) should sell its upstream activities to focus on refining and distribution. In discussions with ministers, socialist members of parliament, etc., together with a few friends, I succeeded in persuading them that it was not appropriate to do so. Fortunately! Because with vertical integration in the oil industry, if you abandon the upstream segment, you incentivize entry into the downstream, into refining, by crude

oil exporting countries. Consequently, the refineries you intend to focus on become those most vulnerable to competitive attack.

*Of course.*

So, our alternative to the mainstream approach proves valuable from a thousand perspectives. Sylos's idea is that in every field, not only in the economy, there are barriers to entry that are neither zero nor totally insurmountable. Sylos applied it even to university careers. He would say that when you, a Sraffian, a non-orthodox economist, apply for a chair, you have to jump a little higher than the others. 'Don't complain', he'd say, 'because in my time barriers were much higher'. And we shouldn't complain either, because today the barriers have again increased! You can apply this idea of barriers to entry to virtually all contexts.

*Yes. So, you feel that Sylos's oligopoly theory should be revisited more by younger scholars.*

Yes. It should be integrated with the Keynesian theory of employment and the Sraffian theory of value, so to speak. A reference to a classical-Sraffian theory of value is necessary, and Keynesian theory is equally fundamental, but oligopoly theory is fundamental too. These three pieces, together with other elements, must be the core components of a reconstruction.

*I've never been a historian of thought, but when I begin the first lecture of my macroeconomics course, I state that Keynesian theory is based on imperfect markets and the degree of capital utilization. Otherwise, if you have perfect competition, you cannot hypothesize everything Keynes did. So, I thought a view integrating oligopoly theory with the Keynesian view was already quite established, but evidently, I am mistaken.*

Because you have to see in what ways oligopoly theory yields results that differ from the normal ones, so to speak, in all economic fields and not only in the field covered by Keynes; and you have to clarify the connection of the various theoretical bricks to a common conceptual framework. If you interpret Sylos's theory in the way Modigliani did, that would be impossible.

*There is a more technical issue we are interested in: prices of production, the uniform rate of profit, and mechanisms of convergence to the uniform rate of profit.*

Well, I deny the mechanisms of convergence. Steedman demonstrated this perfectly, in my opinion, in his book on Marx. You don't need convergence mechanisms to understand that in a snapshot, you isolate certain factors, and the result of those factors is those prices, namely an explanation of the interrelations between relative prices and income distribution. The system then moves.

One of the points of disagreement I had with Garegnani was that he would say, 'the core is stable, technology moves slowly'. While it's true that technology moves relatively slowly in the aggregate, what matters is that we are dealing with relative prices, and sectoral movements in technology – think of sectors with a technological explosion – may be, both in principle and in practice, much more rapid than adjustment processes working their way through all the interrelations of the economy.

Garegnani tended to say the same thing for wages, he tended to assume wages as given, whereas Sraffa suggests stressing the influence of the rate of profit, namely of monetary

and financial policies. This is a very subtle suggestion, because Sraffa doesn't actually suggest a theory of distribution for a given moment in time. You cannot determine an equilibrium level of wages and the rate of profit. There is a dynamic, which is Sylos's conception: that the problem of distribution is not a problem of equilibrium theory; what you can try to build is a theory of changes in distribution.

In any case, if you try to take the real wage as given, as Garegnani originally did, you must consider that when inflation is volatile, the real wage can change enormously from one year to the next. It is not that stable. I remember that when prices exploded between 1975 and 1979, given my salary as a non-tenured professor in Perugia, my real income decreased by at least 20 percent.

*Even with the 'scala mobile' (wage indexation)?*

Because the single-point scala mobile mechanism we had at the time only covered the base (average) wage. Therefore, in the case of a salary higher than the base wage, as university salaries were at that time, it covered only part of the price increases. So, an inflation of 40 percent in a few years meant a 20 percent drop in real income for me.

*Because it was inflation that did not stem from wage movements.*

No, it was derived from the increase in oil prices. Yet, Monetarists built the failure of the Phillips curve on top of that. That was the moment when Monetarism won, based on an erroneous explanation of the inflation that followed the oil crises.

*I'd like your impression on where the young scholars in our field are headed. I feel that, from a certain perspective, the synthesis between Sraffa and Keynes is becoming somewhat trivialized – for instance, with the super-multiplier approach being promoted as Sraffian, which I don't find very convincing. Also, what I've noted is that while Garegnani's students were once a very closed and aggressive environment towards the rest of the world, they now seem much more open to dialogue. What is your view on this?*

I don't know, I must say that I have somewhat delegated contact with the younger generation, partly because I don't move around much. I am focusing much more on the Lincei, while Carlo D'Ippoliti, Marcella Corsi, Giulia Zacchia, and Giulio Guarini are the ones who keep a connection with younger scholars... and I don't follow these discussions very closely either; I'm trying to complete what I can of my own researches. My latest book on *Power and inequality*, published a year ago, is in fact the result of a work I started with my friend Roberto Villetti in the late sixties; it has an interdisciplinary approach and an explicit political content as well.

My impression is that there is a lot of empirical work being done, which can be useful across different perspectives, provided some basic framework and foundational concepts are also agreed upon. There are some things that leave me a bit perplexed, such as a concentration on the individual distribution of income. Certainly, discussing social classes is more complicated when the three traditional social classes no longer make sense, but that doesn't mean that social classes have become a completely useless analytical tool. Thus, the near abandonment of research on the functional distribution of income is a bit of a shame.

The spreading of applied research suggests there is a degree of fragmentation of contemporary theory. After the foundations of marginalist theory collapsed, scholars scattered: some focused on auction theory, others on energy, or international trade or the environment, and so on.

*But you're talking about the mainstream, though.*

But the same thing has happened with us as well. And there is, even on our side, a reduced interest in the history of economic analysis, which is where you can reconstruct the conceptual foundations of the different approaches, and that is the prerequisite for putting together the different analytical bricks for a full-fledged alternative to the mainstream.

*Thank you very much.*

*The interview was conducted by Gennaro Zezza on 3 June 2025 in Rome.*

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