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‘In search of a better economics’

Interview with Heinz D. Kurz



Heinz D. Kurz is Professor (emeritus) of Economics at the University of Graz, Austria and founder and chair until 2015 of the Graz Schumpeter Centre. Previously he was a Professor of Economics at the University of Bremen, Germany (1979–1988) and the Theodor Heuss Professor at the New School for Social Research, New York, USA (1990–1991). He received a diploma in economics from the University of Munich, and a PhD from the University of Kiel, Germany. He was a Visiting Fellow of Wolfson College, Cambridge, UK (1976–1977) and a Visiting Professor at several European, American, South American and Asian universities. In 2002 he received the highest prize of the Austrian Academy of Sciences (historico-philosophical class) and in 2014 the University of Nanjing, China, made him a professor honoris causa. Heinz D. Kurz was the President of the European Society for the History of Economic Thought (2006–2008), a founding editor of the *European Journal of the History of Economic Thought*, and he has been a managing editor of *Metroeconomica* since 1998. He is the general editor of the unpublished papers and correspondence of Piero Sraffa, a project carried out on behalf of Cambridge University Press, UK.

As a first question, I would like to ask: how did you get interested in economics and how did you get in contact with Keynesian and Sraffian economics?

As regards economics, I thought this is a key subject in order to improve the lives of people and contribute to a more just society. This was my political motivation at the time of the students' revolt in the late 1960s. As regards my theoretical orientation, which crystallized quickly, the story is the following. The theme of the diploma thesis I was given was Marx's law of the falling rate of profit. I read Marx, found his argument impressive and challenging, more interesting than much of the ordinary fare we were offered at the university. Then there was Erich Preiser. He was a fascinating teacher who exposed us to Kaleckian, Keynesian and classical views. He and his assistants had a considerable impact on my intellectual formation. While writing my diploma thesis, one day I met a member of the Preiser group who inquired about my work. He told me that I ought to read a book by an author I did not know. I went to the library and was given a slim volume. I intended to read it in the afternoon of the same day. Alas, I didn't get beyond page 5 or so; I understood next to nothing. The book was either complete rubbish or very deep indeed. I decided it was the latter. The reference is, of course, to Piero Sraffa's *Production of Commodities by Means of Commodities* (1960). It has accompanied me ever since.

You finished in Munich with a work on Marx's theory of the falling rate of profit, with Erich Preiser? And then you moved to Kiel, as far as I know.

It was not with Preiser, but with Albert Jeck — Preiser died in 1967. After graduation I got several offers as a research assistant, but chose to join Albert, who had been appointed to a professorship at the University of Kiel. I also did my PhD thesis with him.

And this was then on Sraffa's work?

Initially it wasn't. It was the heyday of marginalist (or neoclassical) multi-sector growth models. So I started to work on them, but quickly got bored with their mechanistic approach — another variant of push-button economics. I returned to Sraffa's analysis, which is also about a multi-sector economy, but a great deal more fascinating. I understood quickly that his findings undermined several of the salient building blocks of marginalism. Peter Kalmbach, another Preiser offspring, had published his fine PhD thesis on post-Keynesian and neoclassical distribution and growth theories and had provided a succinct account of the Cambridge controversies in the theory of capital. He was invited to Kiel to give a talk and ever since we were in close contact with one another. It became ever more clear to me that Sraffa's contribution was the focal point of the criticism of the marginalist mainstream. While initially Joan Robinson was the main spokeswoman of the critics, there was no doubt that Sraffa was their *spiritus rector*. Unfortunately, Robinson, who benefited a great deal from him, did not always fully understand what he was saying and is responsible for some confusions that marred the debate. So how could I possibly not try to learn from Sraffa and trace the intellectual roots of his propositions?

Was this the period when you also got in contact with the Cambridge post-Keynesians or, if not, when did you establish contact with them?

After I had finished my thesis I was keen to go to Cambridge to meet Sraffa and other scholars around him — members of the so-called Anglo-Italian School. Supported by a British Academy Scholarship 1977–1978, I met, amongst others, Piero Garegnani, Luigi Pasinetti, Geoff Harcourt, Ian Steedman, Bertram Schefold, John Eatwell, Mario Nuti, Bob Rowthorn, and, of course, Piero Sraffa and Joan Robinson. I wonder whether they would all have called themselves 'post-Keynesians'. Actually, with Maffeo Pantaleoni I maintain that there are only two kinds of economics: good economics and bad economics. (This is why I dislike the notion of 'heterodox economics', since it involves some self-stigmatization.) And Sraffa's contribution clearly belongs to the former. I met with American post-Keynesians such as Paul Davidson, Sidney Weintraub and Hyman Minsky only subsequently, after having been appointed to a chair at the University of Bremen in 1979 and especially when I joined the staff of the International Summer School in Trieste in the early 1980s.

You were involved in the Trieste Summer School right from the start?

Yes, I was. We had a first meeting in Udine to coalesce and decide the format, etc. The idea was to bring together scholars with a critical orientation towards the mainstream and see whether they could join forces in the intellectual battle. The organizers were Sergio Parrinello, the proper flywheel of the project, Garegnani and Jan Kregel. Then there were Steedman, Schefold, Ed Nell, Weintraub, Davidson, Minsky and many others, including Josef Steindl, who repeatedly impressed me with his remarkable ability to interpret empirical trends. I was lucky enough to make friends with several of the aforementioned and collaborated with them over the years. In Trieste I also met Neri Salvadori, then a junior fellow, with whom I have been happily working ever since. The Trieste

experience found an echo in Bremen, where Peter Kalmbach, Otto Steiger, I and Harald Hagemann, who had joined us from Kiel, formed a research unit and a research seminar and Harald and I established an exchange programme with the Graduate Faculty of the New School for Social Research, New York. I taught there in spring 1985 and then again in 1990–1991 as the Theodor Heuss Professor, a chair endowed by the German Government in recognition of the New School's role as the University in Exile during the Nazi period.

You got your first professorship at the University of Bremen in a more or less heterodox or non-neoclassical department. What were the key developments there?

In 1979 my family and I moved to Bremen, where we stayed until 1988. I was at first very optimistic, because I thought that this was a place with an open and stimulating intellectual climate and where no single orientation in economics dominated and suffocated alternative ones. However, my optimism soon got frustrated. Instead of narrow neoclassicism I was now exposed to narrow 'Marxisms' of different sorts (poor Marx!), many affiliated with political splinter parties, such as the Stalinist DKP (German Communist Party), the Maoist KPD/LM, which at the time worshipped Enver Hoxha of Albania, etc. Boring, dull and in fact frightening! It was much more rewarding to discuss with mathematicians working on economic themes, especially Ulrich Krause and then Reiner Franke. We invited scholars, including Harcourt, Nell, Krishna Bharadwaj, Steedman, Garegnani, Parrinello and Salvadori to give talks in our seminar and provide young scholars and PhD students with the opportunity to mingle with leading representatives of non-marginalist economics.

This was also the period in which you, I think together with Peter Kalmbach, tried to work on a synthesis of Keynes's theory of effective demand and Sraffa's theory of relative prices. I think you called it the 'new classical synthesis'. What is your view on this attempt today?

Several scholars, starting from Sraffa's reformulation of the classical approach to the theory of value and distribution, felt that while the general rate of profits and long-period prices were well explained, given quantities, what was missing was a theory of quantities. Liberated from the straitjacket of Say's law and the full employment postulate its marginalist version entertains, the idea was close at hand to somehow bring together Sraffa's classical analysis and the Kalecki–Keynes principle of effective demand. This we did, indeed, in a number of papers. The macroeconomic upshot of this was a model of economic growth and income distribution, first published in German in 1988 (Kalmbach/Kurz 1988), and then, by me alone, in 1990 in a critical essay on Kaldor's contribution (see Kurz 1990). The model is similar to the Bhaduri–Marglin model, but exhibits a few features that go beyond the latter. First, it distinguishes between blue and white collar (or 'overhead') workers, the former being employed in proportion to actual output, the latter in proportion to the size of the capital stock. Second, it discusses the impact of several forms of technical progress on the steady-state performance of the system and focuses especially on regime shifts triggered by them between 'overaccumulation', 'underconsumption', 'Keynesian' and 'neoclassical' regimes. On a micro level I delved deeper into the problem of the choice of technique with respect to the extensive and intensive use of fixed capital items, dealing *inter alia* with shift work, moral obsolescence, time-dependent wages and prices and the like (Kurz 1986; see also Kurz/Salvadori 1995: ch. 7). Sraffa and Keynes were both critics of orthodox economics: the former showed that the marginal productivity theory of distribution could not be sustained and the latter that there was no reason to presume that the economic system, over a sequence of booms

and slumps, realized full employment of its productive resources. The two authors, while divided in destination, were united in dissent and sought to elaborate alternatives to the marginalist doctrine.

After some initial papers you do not seem to have followed this up, in the sense of producing a synthesis which provides the grounds for an alternative to neoclassical economics?

This is true. In the late 1980s I published a few papers in which I criticized the often cavalier way in which the problem of capacity utilization was dealt with in the literature. At any moment of time the capital stock of an economy is made up of capital goods of various vintages, and it is plausible to assume that a concern with cost minimization prompts firms to fully utilize the most efficient machinery even in conditions of a slump. Machines of an obsolete type that are still in use (Wicksell's 'rent goods') are worth employing for what they can get, as Sraffa stressed, thus clearly drawing the attention to effective demand. Such machines may be compared to certain qualities of land, which will be employed only if effective demand is sufficiently brisk. Unfortunately, as far as I know, up until today this hint has not been taken up in the relevant literature. My work on macroeconomics and effective demand moved somewhat into the background, because after we had met in Trieste, a very productive (for me) cooperation with Neri Salvadori began. After a paper we published in the *Journal of Political Economy* on Sraffa, we felt that the latter's approach and its roots in classical economics had to be expounded more thoroughly, open questions tackled rigorously and its fecundity demonstrated in a variety of fields that had not yet been tilled (for example, jointly utilized machines, capital utilization and exhaustible resources). We started to work on *Theory of Production: A Long-period Analysis*, which turned out to be a tremendously demanding project and absorbed all our energy. When it appeared in 1995 with Cambridge University Press (Kurz/Salvadori 1995), with a paperback edition following in 1997, we were quickly engaged in several debates and invited to many places far and wide to give talks on it.

What is your view today on post-Keynesianism as a research programme? Is it still worthwhile thinking about it in the sense of coherence or a minimum of coherence in order to establish an alternative to neoclassical economics? And if so, what should be in there?

Post-Keynesians have enriched our understanding of how capitalist systems function, and when they don't, what ought to be done to stabilize them. Among the few economists, who understood that a big bubble was building up at the beginning of the millennium that was bound to burst sooner or later, the majority were Keynesians and Marxists and other non-mainstream scholars. However, even in these circles there is room for non-incremental improvements of the analysis. I see especially the following intertwined problems that ought to be tackled: (i) the role of power in economics; (ii) the role of innovations; and (iii) the role of the monetary and financial sector in economics. Quite a bit has been accomplished in recent years as regards item (iii), including your own valuable work, but much is still to be done. As regards item (ii), the post-Keynesians would benefit greatly, I surmise, from absorbing some ideas of Marx, Schumpeter and evolutionary economics as regards the inherent dynamism and restlessness of the capitalist system (see Kurz/Sturm 2012). Finally, the problem of power, item (i), ought again to become a major tenet in economics, as it was at the time of the classical economists. Thomas Hobbes famously insisted 'Wealth is power', and Adam Smith agreed, adding that different social classes are typically characterized by different levels of access to information and capabilities to interpret it. Finally, one must admit that what is called 'the' mainstream is not chiselled in stone, but is changing continually. Its representatives frequently come up

with findings that are worth scrutinizing carefully. Solow and Samuelson, for example, were Keynesians of sorts.

Yes, in the short run.

Well, Solow argued that there is no presumption that in the long run effective demand does not matter. In his preface to the *Handbook of Economic Growth*, edited by Aghion and Durlauf (2005), he complained bitterly that in the entire book effective demand is mentioned not even once. I do not imply that post-Keynesians are well advised to become Solovians – clearly not – but Solow is very different from someone like Robert Lucas, for example. There has been a deliberate and coordinated move to roll back Keynesianism, from the Mont Pèlerin Society via Friedman to Lucas and his acolytes. As far as I know, Solow (and Samuelson) were not part of it.

But he is not completely innocent with respect to that development.

Of course not, but if you read his 1956 article, he made it very clear that he *assumed* full employment. The problem is people who interpreted him as saying that the system is self-regulating all the time, markets are always efficient, whether competitive or monopolistic, and that we do not have to care about economic policy. Incidentally, in the debate on capital theory, Solow's performance was not all that great. But he and even more so Samuelson would always listen to alternative points of view, and especially Samuelson was very supportive of the work Neri and I did, despite the fact that it ran counter to his own point of view. I was surprised to see Lucas denounce Keynes explicitly as an ideologue. I wonder who of the two deserves that characterization (see Kurz 2010a).

Another strand of your work with Neri Salvadori has been the discussion and critique of new growth theory. So what is your take on that? Is this again a dead end of neoclassical growth theory or do you see some progress in that area?

Some of the practitioners of new growth theory contended that it would completely revolutionize how we think about economic problems. There was an enormous hype, but Neri and I were rather sceptical. First, new growth models put forward ideas which have been with us for a long time; what was new was their formalization in an equilibrium setting. Think of Romer's treatment of R&D or Lucas's of human capital and the spillovers that are taken to accompany them. Second, looking at the models from a higher standpoint, perpetual growth resulted via getting rid of scarce original factors of production. This was accomplished by simply abstracting from such factors or by invoking mechanisms by means of which they were generated endogenously. If all resources are producible and accumulable, growth cannot be constrained any longer. In case there were still such original factors, the rest of the system was bound to generate positive externalities that overcome their negative effects on growth. By replacing simple labour by human capital, an accumulable resource, the new growth models have a point of contact with the classical economists. These took the amount of labour needed in the system as generated from within in terms of some population mechanism and labour productivity growth. Seen from this standpoint, what we were given were frequently old ideas in a new garb couched in the usual neoclassical optimizing framework – the lingua franca of much of today's mainstream. However, the problem of the heterogeneity and actually growing heterogeneity of goods was and still is skilfully circumnavigated with all kinds of tricks in order to avoid the capital theoretic problems Sraffa had pointed out. In the meantime a more sober view has spread, not least because of the poor empirical performance of the models.

When you went to Graz you extended your research, starting with Sraffa, incorporating Keynes, and now there is Schumpeter – and you became the Director of the Graz Schumpeter Centre.

The Graz Schumpeter Centre (GSC) was founded in 2006, but it had a precursor. Schumpeter was a professor there from 1911 for a little more than 10 years, but when I arrived in Graz this fact had not yet been ‘capitalized’, so to speak, in terms of some academic institution carrying his name. I must confess that at the time I tended to under-rate Schumpeter’s achievements. My assessment changed when I studied his works more closely. His *Theory of Economic Development* (1934), published first in German in 1912, is a masterpiece despite its many shortcomings. He understood that the characteristic feature of capitalism is its restlessness, its inherent dynamism, not its alleged tendency towards a state of rest at full employment – and in this regard he was a devout student of Marx’s. The precursor of the GSC mentioned in the above was, of course, the instalment of the annual Graz Schumpeter Lectures, which I founded together with Christian Gehrke and Richard Sturm in the early 1990s. The lecturers included, *inter alia*, Stan Metcalfe, Nathan Rosenberg, John Roemer, Duncan Foley, Philippe Aghion, Ian Steedman, the late Tony Atkinson, Joel Mokyr, Alan Kirman and Jacques Thisse – not a bad bunch of scholars, in my view. The lectures are published by Routledge (and some by Princeton or Cambridge University Press) and have greatly contributed to the international visibility of Graz University.

Are you in favour of a research programme aiming at a synthesis of Sraffa, Keynes and Schumpeter? Or would this give too much credit to Schumpeter?

I think one ought to learn from all good economists, and those you just mentioned certainly belong to them. There are others that deserve to be listed, including Adam Smith (see Kurz 2016a), my all-time favourite David Ricardo (Kurz 2015; Kurz/Salvadori 2015) and Marx, but also Knut Wicksell, for example. The fact that an author blundered does not necessarily relegate him to second place, provided his insights were substantial. One might even contend that the greatness of an author is mirrored in the magnitude of his blunders, but this would be going too far. Yet there is an element of truth in this, and Marx, Schumpeter and Keynes are cases in point. Can one synthesize what is correct and valuable in different authors into a coherent whole? In principle this should be possible, but then the question arises of why it has not been done already, long ago? General equilibrium theory was an attempt to kill all birds with a single stone, and as we know now, after Mantel, Sonnenschein and Debreu, it failed. It seems we cannot (easily?) abandon the old maxim: ‘horses for courses’. Forging a theory for the particular case under consideration is the right thing to do in order to ‘elucidate economy principles’ vis-à-vis ‘a labyrinth of difficulties’, as Ricardo put it (see Kurz 2015). Trying to forge a single theory for all possible cases is asking much too much. However, some new combinations of existing ideas appear to me to be apposite. As regards Keynes and Schumpeter, they both rejected the dichotomy of the monetary and the real sphere of the economy, the proposition that investors are the devoted servants of consumers and savers, and the doctrine that the rate of interest equilibrates saving and investment. Yet, whoever gives priority to investment over saving cannot possibly totally reject the principle of effective demand, and whoever wishes to talk about the medium and long run cannot do so without studying the process of creative destruction. Cross-breeding certain ideas of Keynes and Schumpeter thus strikes me as the natural thing to do. And since, I am sure, you would like to base your construction on a solid theory of value and distribution, you will (won’t you?) have recourse to Sraffa’s work.

Talking about Schumpeter in comparison to Marx, what is the value added which Schumpeter provides?

Schumpeter had the advantage of standing on Marx's shoulders and of having had access to a much larger empirical evidence than 'Old Moor'. Therefore it must not come as a surprise that he was able to formulate some elements of the process more clearly. I have especially in mind the triad invention–innovation–imitation. He studied the process with reference to population dynamics – the pioneer vs the incumbent firms – and emphasized the role of the financial sector in providing liquidity to the innovator to carry out his or her project. Schumpeter rightly emphasized the creation and destruction of variety. In at least one respect he was totally opposed to Marx, who saw all profits rooted in the 'exploitation' of workers. Schumpeter went to the other extreme and interpreted all profits as the fruit of innovations (Kurz 2012c; 2012d). His view cannot be sustained, as the criticism of his concept of the circular flow, in which there are neither profits nor interest nor capital, shows. But he was right in stressing the importance of 'new combinations' for economic development and asking how innovators get (or ought to get) compensated. After all, a major reason why Soviet-style socialism lost the systems race was its failure to generate enough innovations and productivity growth.

As regards the monetary side, that is also in Keynes: Schumpeter overlaps with Keynes in this respect.

The two views overlap, but they are not the same. It was Schumpeter who stressed as early as in *Theorie der wirtschaftlichen Entwicklung* (1912) that the financial sector is inherently unstable and may prolong and deepen crises. This was a clue from which Hyman Minsky, who was a student of Schumpeter's that turned Keynesian, elaborated his financial instability theory. Schumpeter was also clear that innovations are not limited to the 'real' part of the economy, but invade also the monetary part with potentially disastrous effects, if they increased asymmetric information. He was himself on the lookout for new ways of financing innovations, viz his pioneering theoretical and practical work on venture capital. His perspective on the economic system was evolutionist. Compared to Keynes, he rightly put much larger stress on technical, organization and institutional change and was concerned with analysing the co-evolution of the economic, political and cultural spheres. While he praised Walras's general equilibrium theory, he was convinced that the capitalist economy could not be understood in terms of it. Capitalism continuously generated change from within and was unstable, as he stressed time and again.

But not violently so, to use Keynes's terminology?

Yes, at least most of the time. But Schumpeter could not exclude the possibility of highly disruptive forms of technical change accompanied by mass unemployment, financial distress and an eroding trust in the political class. Interestingly he tried to explain the World Economic Crisis in the late 1920s and early 1930s as reflecting the concurrence of the troughs of three different types of cycles – Kondratieff, Juglar and Kitchin. But in general he believed in the self-regulating forces of the capitalist economic system. This flies in the face of his heretical view that the demand for labour need not always be inversely related to the real wage rate or his view that investment demand need not be inversely related to the rate of interest, as Keynes and marginalist authors contended (see Kurz 2012d). Such non-conventional demand schedules raise the problem of stability of the respective market and of the system as a whole. As regards investment demand, Schumpeter, in contrast to Keynes, took into account that a change in the interest rate will typically affect relative prices and therefore relative costs of investment projects. He inferred from this that it

cannot be excluded that a rise (fall) in the interest rate leads to an increase (decrease) in overall investment demand. I surmise that his Walrasian education was helpful in glimpsing this possibility, which Keynes, brought up with Marshallian partial equilibrium analysis, could not.

So Schumpeter already had a notion of the interdependence between distribution and relative prices?

He clearly did, but in a very rudimentary way only. He had read Smith, Ricardo and Marx, who had all stressed the connection between prices and income distribution, as had Walras within a different framework. But Schumpeter's very modest technical skills did not allow him to push the frontiers of knowledge in this regard. He repeatedly had good intuitions but lacked the tools to translate them into coherent pieces of theory. In fact, had he understood better the implications of the unconventional possibilities he invoked for marginalist theory, which despite the heretic elements in his analysis constituted the bedrock of his economic views, he may have spent more time on studying those 'anomalies'.

So Schumpeter is not inconsistent with your other heroes, Ricardo and Sraffa?

I would not go that far or put it that way. But as I have argued, for example, in Kurz (2008; 2012d), certain views of Schumpeter may easily be reinterpreted within a classical framework of the analysis (see Kurz 2008). One of the major tasks at the GSC is indeed to cross-breed classical and Schumpeterian ideas. We are very optimistic that this proves fruitful, and first results show that it does. Another and related part of our current work is connected with the work I had done with Peter Kalmbach, Harald Hagemann, Reiner Franke and others in Bremen on the impact of technical change on employment, growth, structural change and income distribution in terms of a dynamic input–output model (see Kalmbach/Kurz 1992). At the GSC we currently study the economic impact of the diffusion of cyber-physical systems (also known as 'industry 4.0').

What about your activity as an editor of Sraffa's papers and correspondence? Can we expect anything new, anything revolutionary for the years to come?

I wish the edition was already out with Cambridge University Press. This would have taken a huge load off my mind. Unfortunately, the project was delayed time and again for reasons of which I spare you the occasionally unpleasant details. I venture to say only this: had I been given full liberty in choosing collaborators, etc., the edition would have been published several years ago (in accordance with the deadlines negotiated with CUP). There is a lot of material that is of interest even nowadays – Sraffa's views on certain ideas of Keynes, especially liquidity preference theory (see Kurz 2010c), his criticism of Hayek's monetary overinvestment theory of the business cycle, his assessments of doctrines by Marx, von Böhm-Bawerk, Pareto, von Bortkiewicz, Hicks and others. The original meaning of 'revolution' was the re-establishment of an old, legitimate state of affairs, which had been superseded by a new, illegitimate one. In this sense one can say that Sraffa was indeed concerned with a 'revolution' in economics, proposing a return to the 'standpoint ... of the old classical economists from Adam Smith to Ricardo, [which] has been submerged and forgotten since the advent of the "marginal" method' (see also Gehrke/Kurz 2006; Kurz 2012b). He felt that much of contemporary economics served, often unconsciously, ideological purposes. (On the factors driving the development of economic analysis, see Kurz 2016b and the three volumes of Faccarello/Kurz 2016.) He in fact saw the emergence and rise of the marginalist school first and foremost

as a response to political tensions and upheavals that threatened the received social order. Böhm-Bawerk was a response to Marx as Friedman was one to Keynes. Sraffa considered Max Weber's maxim of 'freedom from value judgments' a pious hope in the social sciences. You see, Sraffa keeps me busy.

You have only sparsely intervened in public debates. There are others who are very active when it comes to economic policy debates; maybe too active to be taken seriously as an academic economist?

I am full of admiration for those who want to 'save the world' and care for people in poverty, ignorance and despair. I try to contribute my share to this. But I also know that having laudable intentions is not the same thing as doing good. And, as Adam Smith warned us, those who claim to look after our interests frequently are only concerned with theirs. The love of power, Bertrand Russell insisted, outweighs all other motives of human action and, like vanity, is insatiable. Seeing some of my colleagues desperately seeking to appear in the media and being listened to by politicians, makes me think that 'saving the world' is only a part, and perhaps a very small part, of their intentions. And there is the following aspect: the principle that, according to Einstein, ought to apply to scientific work – 'Everything must be made as simple as possible, but not simpler' – according to my experience typically is not applicable in policy advising: too simple is hardly ever simple enough. One can thus only wonder why 'economic policy advice' contains the noun 'vice'. This answers, perhaps, your question. I do not mean, of course, that we economists do better by staying in the proverbial ivory tower and not engaging in policy debates (some, however, should!) – not at all. In my lectures, seminars and talks I comment on policy issues and I hope my students, upon critical scrutiny of what I say, spread the gospel of good economics. Finally let me say that saving one's soul appears to me to be no less difficult than saving the world. The two also seem to me to be much more intimately intertwined than is typically assumed. In fact I am convinced, if more people succeeded in saving their souls, and I am not talking about religion, the state of the world and humankind would be less deplorable.

Of course, I would agree with that. Maybe a final question on the perspectives of economics as an academic discipline: what are the perspectives for more critical economists, what is your advice for the next generation?

My advice for the next generation is to study a serious subject. And I am afraid to say that I don't think that certain parts of economics today deserve this attribute. My experience is that a growing proportion of economists have become very cynical about their work; they don't take the subject seriously any more. The disenchantment with the state of the art is widespread, as is evidenced by articles in leading newspapers and periodicals, such as *The Economist* (see Kurz 2010a). However, the financial and economic crisis does not seem to have been big enough to shake up the political and academic system. The failure of politics to effectively regulate the financial markets is all too obvious. Among academics, the huge sunk costs of those who advocated the economics of Dr Pangloss, according to which we live in the best of all possible worlds, account for a remarkable intellectual inertia. The echo effect of the misallocation of resources, mental and other, in the discipline will accompany us for a long time. The marginalization of the subjects of economic history and the history of economic thought in the economics curricula amounts to erasing any traces of the failures of parts of economics and the policy recommendations derived from it. Who will in the future recall, for example, that in his presidential address to the American Economic Association in 2003 Robert Lucas boldly contended that the 'central problem of depression prevention has been solved, for all practical

purposes', and that 'the potential for welfare gains from better long-run, supply-side policies exceeds *by far* the potential for further improvements in short-run demand management' (Lucas 2003: 1). He concluded, triumphantly, that because of the progress made in macro-economics 'we are able to form a much sharper quantitative view of the potential of changes in policy to improve people's lives than was possible a generation ago' (ibid.: 12). This was a sweet dream that burst together with, and actually as a part and parcel of, the financial bubble.

And you do not see any correcting mechanisms?

There are, of course, such mechanisms at work, but they appear to be weak and rather slow. If they are too weak and slow, economists run the risk of becoming an endangered species. There are indications that this process is already under way. By worshipping for too long only one kind of orientation with doubtful credentials, the subject has manoeuvred itself into a precarious situation. An important finding of evolutionary economics deserves to be swiftly recognized: progress presupposes choice, and choice presupposes variety. Putting all of one's eggs in a single basket is a very risky strategy. And there are highly respectable alternatives to the mainstream that deserve greater prominence in the profession, both in teaching and in research. Some of these alternatives have been mentioned in the above. Alas, I am pessimistic that bubbles can reliably and definitely be avoided in a field as difficult as economics. But anyone acquainted with economic history and the history of economic analysis, both where it triumphed and where it went astray, will be aware of the danger and will be on guard.

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