

A CONVERSATION WITH GEOFF HODGSON

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Chapter 21

A CONVERSATION WITH GEOFF HODGSON

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We would like to go over some of the key issues that have mattered and still matter to you. Please feel free to begin this in any way that you like.

Thank you for giving me this opportunity to put forward some ideas. Perhaps I will start with my conception of the individual in mainstream economics and draw up some of its implications. Before I do that, I want to emphasise that I do recognise that mainstream, or what some people call neoclassical, economics has proved itself to be remarkably flexible in the last 25-30 years. My experience as an economist in economic departments 30 years ago was that mainstream economists would be less tolerant of anyone who questioned the view that agents maximise something, be it utility or profits or anything. It was just taken as a foundational assumption and if you challenged it, you were not an economist. That has changed, particularly with the rise of behavioural economics, experimental economics and some aspects of game theory. While recognising this flexibility, like John Davis and others, I also see a set of core ideas that does not seem to change very much. “Max U,” as Deirdre McCloskey calls it, is still at

the centre of economics. The survival of rational expectations models in macro, despite the financial crash, is evidence that there is still a gravitational pull towards that rational actor.

One of the reasons why I chose to move to business schools some time ago is my profound disagreement with the debasing model of a utility-maximising agent. I am deeply unhappy with it, as a scientist. Utility maximisation is not meaningless or lacks use. As an organising heuristic, it has some value. But it is unfalsifiable, in the sense that you can fit it to any data. While one may argue that the standard utility maximisation approach has been falsified in cases of inconsistent preferences, it is impossible to decisively prove that preferences were inconsistent. Any case of supposedly inconsistent preferences would be a set of examples through time, with particular decisions made in different times and different places under different circumstances. One will thus always be able to give variables a time tag, and what was treated as intransitive can become transitive. There is no data which decisively demonstrates inconsistency of behaviour. I prefer meat to fish today, fish to meat tomorrow, but there could be all sorts of explanations for this: different contexts, news about mad cow disease, and so on.

I do not suggest that because utility maximisation is unfalsifiable it is unscientific. I am not a crude Popperian. But I think that its unfalsifiability relegates the idea of utility maximisation to a heuristic or organising device rather than a detailed explanation of human behaviour. Another way of making this point is to refer to experiments on non-human organisms, like pigeons, rats in cages, and so on. Some have alleged that these organisms are utility maximisers: the behaviour they exhibit is consistent with some utility maximisation scenario with some particular preference

function. While these experiments with non-human organisms may be meaningful in some sense, the conclusion I would draw is that utility maximisation is so flexible that it does not even tell us what is specific about human behaviour. The problem with utility maximisation is its excessive universality.

Although Herbert Simon got the Nobel Prize for his behavioural critique of utility maximisation, behavioural economics was not particularly popular, until another version became established in the 1990s. In this second phase, some of Simon's more radical challenges were watered down, with many behaviourists arguing that they were studying errors and learning. They noted that in some circumstances people made errors, giving undue weight to particular elements from a sample, for example, but viewed behaviour as a process of adjusting towards some underlying optimum. Behavioural economics thus became the study of deviations from some version of utility maximisation, which illustrates to me that mainstream economics is still very much wedded to this core. Whether we accept or reject utility maximisation, the point is that we need to understand humans much more. We need to appreciate what is specifically human about our nature, our behaviour and our motivations, so that we have a better understanding of the economy.

We need to introduce morality. Normative issues are present in mainstream economics, partly in welfare economics, partly in work on justice and on social order, and have been addressed by various kinds of economists, including game theorists. Kenneth Binmore's work, for example, is very rich and interesting. Binmore uses the framework of game theory, which relies on maximising agents, to establish certain normative claims about orders in societies. But his work is really about conventions, not moral imperatives. I make a crucial distinction between the two. A convention is

something, as the name implies, that we do for convenience. The rules of language are conventions. Different languages have different rules: some languages tell us to put the verb at the end of a sentence, other languages say you can put it in other places. The fact that there are different rules suggests that there is limited universality involved. When you conform to a convention, you follow its rules for reasons of efficacy or communication. Consider traffic conventions: even if there were no police, in Britain one would drive on the left because other people drive on the left.

There is a fundamental difference between “when in the UK drive on the left” and “thou shalt not kill.” Moral claims are much more than conventions; they are much more than rules followed for convenience. Convenient rules have no moral force behind them. When we say to people, “thou shalt not kill,” perhaps with the qualification “except in self-defence,” we are saying that this is a norm, irrespective of circumstances or convenience. Likewise, with many other rules, particularly those concerning violence and treating other people’s property. These are moral laws precisely because they are purportedly universal. They may have qualifications that make them circumstance-specific, but with that caveat, they are independent of circumstances, and they supersede mere conventions. This is a standard argument in moral philosophy. Another argument many moral philosophers make is that this view of morality is incompatible with versions of utilitarianism, of which the utility-maximising individual is an offshoot. Welfare economics uses criteria like Pareto efficiency, which makes judgements on the basis of people’s utilities. Moral philosophers argue that a view of individuals maximising their utility is insufficient to substantiate moral judgement, because moral rules transcend that.

I further think that consequentialism is flawed for the reasons provided by John Dewey, which have to do with the interaction of means and ends. Strikingly, both Marxism and neoclassical economics are versions of consequentialism. Marxism, as some people crudely put it, sees ends as justifying the means. There is no moral code in Marxism other than to do with the future utopia; means are chosen for that end. In welfare economics, the Pareto efficiency criterion overlooks the detailed nuts and bolts of the processes by which you get there. By contrast, Dewey points out that in real life, given the complexity of the world and our uncertainty about it, our ends will change as we employ a means. There is an interaction of ends and means: as we understand situations better, our ends may change. We have seen this dynamically in Britain with Brexit. The end is vaguely there but as people talk about the mechanics of getting there, new arguments are created about the end as well as the means. Not only do the process of using means potentially alter the ends, but also the ends are not independent of the means, practically or morally. Engaging means to achieve an end may lead to discussions about moral judgements that qualify the end. It follows that the consequentialist separation of outcomes from processes is flawed. Like its utilitarian cousin, consequentialism is insufficient to substantiate moral judgements.

This was recognised by Charles Darwin. In *The Descent of Man*, Darwin talked about humans specifically and explicitly (in *The Origin of Species* he ducked that question), that is, about how humans evolved and how they generated certain rules to survive. He noted that humans are group-based social animals, and that sociality went back to our primate ancestors. And by simple argument concerning group selection and the survival of the group, Darwin argued that groups with rules which give moral imperatives to people about protecting members of the group, respecting the rights of

individuals within the group, have a selection advantage. Darwin thought that language was a necessary condition, because a moral code is in a sense coded, which allows one to instruct people with the rules and determine whether they are breaking rules or not. We now have research by primatologists like Franz de Waal, which finds forerunners of these kind of rules among primates. De Waal is reluctant to call this morality because it is not an explicit moral code, but there are rules which have an emotional charge that are enforced in groups, even among primates. Pre-human or non-human primates are capable of quite sophisticated behaviours, including altruism, sympathy and empathy, even for other species. As an example, De Waal documented a chimpanzee trying to help an injured bird and make it fly. This capacity, this basic empathetic feeling, seems to have a long evolutionary history.

This is extremely useful insight. We now understand much more robustly how group selection arguments work, so there is an evolutionary basis for the claim that individuals, as well as being selfish, have, to different degrees, propensities for moral motivation. Interestingly, this was also in Adam Smith's *The Theory of Moral Sentiments*. Smith did not have an evolutionary theory, but he did have a view about human nature being a combination of self-interest and social engagement which typically gives us some disposition towards justice and morality. Of course, to repeat, people vary in the degree to which they are like that, but generally speaking, for Smith, it was an important feature of human society. I think this is both exciting and important. It is exciting because it leads to new research agendas. And it is important if we want to understand a number of things, like the nature of institutions. I am trying to go further than other people who work in this area, particularly Samuel Bowles and Herbert Gintis, who are still stuck in a utility-maximisation approach. They use very rich evolutionary

models, and discuss altruism and social preferences, which are preferences that are not purely self-regarding, but that story to me is not going far enough in accommodating notions of morality. There still is dispute within this stream of literature on the importance of morality, and I think primatologists give us some reason to take it much more seriously.

While Amartya Sen is extremely important in this area – his work on economics and ethics was pioneering, and he has been one of the major critics of the standard approach – I find in Sen a failure to engage with the evolutionary argument. Without the evolutionary argument, one ends up with a rather arbitrary listing, a kind of intuitive list of what human attributes are. Notions like identity, which are important, are simply suggested out of the blue, rather than grounded on some evolutionary story which you can then model, test and appraise in the light of evidence. So I find myself in agreement with Sen to a great extent but the underlying theory I think is inadequate. It has an arbitrary quality. By contrast, Thorstein Veblen used an evolutionary argument to scrutinise what he called the neoclassical economics of his day. He asked whether it was consistent with our understanding of human evolution. Our present task is to build up a richer understanding of human agency and motivation, of individuals in social settings. We need more than utility functions, even if we use utility functions. We need an understanding of certain human attributes, like identity, morality and so on, but if we make conjectures about these attributes, they have to be consistent with some evolutionary story.

How relevant are concepts such as Masahiko Aoki's punctuated equilibria to understanding morality?

There are examples of punctuated equilibria. One example would be attitudes to homosexuality. Fifty years ago, many people in Britain and Western Europe would say it was unnatural, and the idea of same-sex marriage would be laughed at. But we changed to a new equilibrium with an extensive, albeit imperfect, toleration of homosexuality, and even the installation of same-sex marriage. Similar changes occurred with attitudes to gender and gender roles. While these changes in our morality are never complete – there is always some kind of battle – these are dramatic shifts of equilibrium in society.

So moral codes do change with circumstances and institutions. We have dilemmas in all modern societies concerning different cultures inside society, some of which have quite different moral codes, particularly concerning religion, gender roles and so on, and we are in a process of continuous discussion and debate about how to deal with those things. While all these discussions have a moral character, we are parking on one side the difficult issue of the right morality or the true morality. I do actually believe there is such a thing as a true morality, but elucidating and justifying it is very difficult.

So you are a moral realist.

I am a moral realist. I do not believe morals are just arbitrary codes, and think there are certain moral values that trump others. One could point to something like the United Nations Universal Declaration of Human Rights as a moral code which approximates something that we might think of as superseding other moral codes that we may confront from day to day. But what we are talking about here is how societies evolve

their morality. There is a lot of discussion in anthropology, and now even in economics with Peter Leeson's work, of how different moral codes evolve and fulfil certain functions, irrespective of what we think about them. That is, irrespective of how we may judge them morally ourselves, these codes have a moral character, even when they involve things that we may object to, like subjugation of women for example. We may wish to criticise such codes, but it is also important for us to understand how those moral rules work and how they came to be.

To what extent are moral rules, and rule-following more generally, underpinned by habitual behaviour?

It is individual habit which we lay down. We learn certain moral dispositions from our parents and we accommodate them, often without thinking. As we progress to adulthood, we think about moral codes. We think, "is this rule appropriate?" or "is it right to do that?" We face all these dilemmas as we become adolescents. While we have got habitually laid down rules from our schooling and parenting, we can place these under review, and we can change our moral view, change our mind. For example, some adolescents would not believe in sex before marriage but may change their view. Our parents may have told us you must get married before you have sex, but on review we may change our minds. These dilemmas are commonplace, not just in adolescent behaviour but also in adult behaviour. So the fact that they are habitual and dispositional and emotionally charged is important, but that does not mean they are beyond review. On reflection, perhaps in conversation with someone else, we can review our behaviour, say "I think I behaved wrongly," and change by laying down new habits. Once that new

rule becomes part of us, becomes part of our moral personality, we register a new disposition, become alert to new things, and change our minds about and attitudes to different things.

More generally, why do people follow rules and, conversely, why on some occasions do they break rules? Before discussing this, I think we must first recognise that there are different kinds of rules. Some rule-following can be understood using what game theorists call coordination games, where everyone has an incentive to coordinate with others, irrespective of preferences. I may prefer to drive on the left but when I go to France I drive on the right because it is a coordination game. Moral issues come into the story because in addition to observing traffic flows, we may believe that we should be decent law-observing people. When we are taught in school not to split infinitives we realise it is a matter of convention to do so, but we may also do so because we do not want to upset our teacher.

There are other rules where that is not the case. Consider stealing. There are many occasions where we could steal and get away with it. If we generally do not steal, it is not a question of convenience or convention, but because we have some notion of right or wrong, we have some notion of our own meaning and identity. I do not want to be, to feel like, a thief, and I do not want to be seen as a thief. Even if we are sure that we will not be caught, we have an internal process of dealing with a transgression such as stealing somebody else's property. For certain reasons to do with our capacity for moral feeling and our notion of ourselves, our reflection on our own persons and our own personality, we refrain from doing that. This moral force is not simply an individual thing; it depends on context. We know this from studies of corruption in societies. Where corruption is standard, where brown envelopes are handed over, it is very

difficult to avoid it, because it is the only way to be recognised as someone worth transacting with. In addition, the social costs one incurs for being seen as corrupt are minimal because everyone else is doing it. Building up social norms and habits against corruption is a long and difficult process for societies, and we should not underestimate what is involved.

Likewise, building basic economic institutions like property, contract, employment, and so on, is a very, very difficult process that requires moral motivation as well as convenience. If we focus simply on costs and benefits, that is, on material or monetary carrots and sticks to get people to follow the rules involved, we leave something out. We miss key components of any well-functioning society, because we fail to see just how much societies depend on moral rules. This has been missed in much work on economic development, but I think we have ample evidence of the importance of these things. This is about institution-building. I think this is where the study of how institutions work in their varied ways, and how people are motivated to follow rules, is extremely valuable and helpful in practical terms.

What is your take on the top-down vs bottom-up debate?

There are limitations to both standard top-down and bottom-up approaches. In the development context, we know that what coheres often much more than anything else, and what provides a moral compass for people to operate together, is some notion of community. We have studies of families, clans or religious groups, like Avner Greif's work on Maghrebi traders, where some notion of group identity with rules and moral norms inside that group enables it to function within its own terms. The Maghrebi

traders traded around the Mediterranean and the policing mechanism was the reputation of the group. If people cheated on deals or did not deliver in particular ways, or went beyond what was regarded as proper and appropriate conduct for that group, they would be internally reprimanded or punished in some way. The identity and reputation of that group was an important policing mechanism. Similar things occur in the developing world with families and clans as well: our family does not do this or that, as we have demonstrated in the past, we are trustworthy people, etc. This is how much of the world still works, particularly in rural, underdeveloped areas. This primes us on a kind of building block of social order and a moral system within it.

The problem arises beyond the boundaries of any particular group to multiple groups, families and clans accepting common rules and having enforcement mechanisms that transcend those rules. When this cannot occur or has limited efficacy, things remain stuck at a particularly low level of development. From history, the solution is not obvious, but a reasonably suitable one would take into account things like nation-building and state-building. The creation of nation-states in Western Europe from the 1500s onwards, and the forging together of a national identity whereby people identify with a state or a religion, is important, with religious conflict playing an enormous role. Within that was the long and bloody path which led to the creation of units which superseded families, clans, groups or regional entities. The creation of nations is an extraordinarily difficult process and often has involved, historically, severe conflict and bloody civil wars.

National institutions and administrations, and the means by which they create sets of rules which have moral efficacy, are neglected in both the top-down and bottom-up approaches. The top-down approach oversimplifies things by saying that you just set up

things like land rights, and give people incentives to follow it. This is defective: who polices the policemen? There is an infinite regress. Unless at some stage in that regress moral norms come in, and some sense of serving the nation becomes the norm, it will likely not work. You can never construct something which is purely based on self-interest and which polices itself in this kind of context. There must be some kind of hierarchy and some kind of supreme principle which ensures that most of the time the system operates. Building national administrations is a crucial step, and this may take a long, long time in many countries. Some countries have gone down that road.

Japan, for example, developed enormously after the Meiji restoration in 1868, by welding together a modern administration, importing Western institutions as well as changing its own institutions, and creating a nation-state with a clear identity and national solidarity within the space of 100 years or so. We have other cases that we can learn from. So while it is difficult, it is not an impossible task. After all, Western Europe did it as well. China was very early in building a national state. What China has failed to do is to build a system of law and an associated morality which buttresses that law. There is a sense of morality in China with Confucianism, but as yet China has not built a state which relies on that system of morality to enforce law. This is a good example of what needs to be achieved to build nations.

What can we learn from Elinor Ostrom's work?

I am a great fan of Elinor Ostrom's work, particularly her 1990 book on collective action, which I think is a modern classic. She talks about common pool resources in a very interesting empirically-driven way; it shows how important empirical grounding is

for economics. Commons arrangements are problematic. They require rules and enforcement mechanisms, but she went beyond the simple story of the self-interested individual, acting in a way to pursue their own interests without sufficient incentives to safeguard that common pool resource itself. A lot of the rules involved cannot be understood as coordination games. They are situations where free-riding behaviour would be expected in circumstances without specific social arrangements. What is interesting in Ostrom is the empirical identification of social mechanisms like approval or punishment that specific communities have used to overcome that problem. And she goes further than that by looking more specifically at the kinds of punishments, their severity, and talks about the gradation of punishment, from mild at first, to rebuke, and then escalating in many cases to higher punishments if people consistently break the rules. Her work showed that the standard economist's toolkit, or at least analyses of those situations based on a simple economist's toolkit, was inadequate, and that we have to enrich our understanding by taking into account social rules.

In her later work, she talked about the ways in which those social rules evolved through time, so it linked up specifically with evolutionary analyses of institutional change. Sadly, she died before that line of enquiry reached maturity. I think this is extremely important and very illustrative for the kind of debate we need to have when we seek institutional designs that are neither purely market-based nor top-down planning. There is an important caveat, though: the key area we need to look into further is the degree to which this analysis of the commons, which very much depends on face-to-face contact in relatively small communities, can be extended upwards, that is, scaled up to large-scale settings where there are other and very different enforcement mechanisms. In some of her papers, Ostrom discussed common pool-like approaches to

major global problems like climate change. Unfortunately, that work did not go very far, or at least not far enough to reach the sophistication of analysis that she achieved in her earlier case studies. We should worry about the difficulty of scaling up an Ostrom-type arrangement in the political area, and also need to look more closely at the idea of using a common pool metaphor for things like corporations. I am not sure it works.

While Ostrom's work is very illuminating as regards micro design principles, it cannot be regarded as a universal paradigm of institution-formation and has limited general applicability. It remains rather specific in its scope. This does not diminish the fact that it gives us very weighty lessons about the nature of institutions.

Beyond the issue of scale is the problem of the level of abstraction.

I think that a good way of starting is to say, roughly, what kind of world you are talking about. In our book on *Darwin's Conjecture*, Thorbjørn Knudsen and I started from ontology. This overcomes a number of problems, particularly cases where people talk past each other. Some people are talking about the development of one thing and other people are talking about the evolution of populations with new appearances and exits from the population. Those stories are quite different, although they may overlap because individuals develop as well as populations, but a lot of the fruitless conversations among evolutionary thinkers and social scientists have been conflating or confusing different ontologies. By specifying the fact that we were talking about populations, within which there are developmental stories, we emphasised the importance of understanding populations as a starting point. What we did was sketch out that ontology, which is highly abstract.

Part of the ontology that we outlined used the replicator/interactor distinction, and the more we worked on this, the more we came to emphasise that the replicator was not a thing. The replicator is a set of rules; it is a rule system, like a computer program. Richard Dawkins invented the concept of replicator but sometimes talked about the replicator as if it were a thing. This is clear from the very title of the book, *The Selfish Gene*: the gene is a replicator and is metaphorically given a personality, which is selfish. That is misleading, not simply because of the alleged selfishness of the gene, but also because it involves treating the replicator as a thing. It is not like an organism or robot. It is a set of instructions, as Dawkins also acknowledges. But there is a tension in his writing between these two conceptions, and some of the debates and the philosophy of biology have got tied up with this. We insisted that the kind of thing going on with replicators is informational and ruled-based, with information understood in a simple Shannon-Weaver sense of a conjunction: a system which receives inputs and limits outputs, with rules that relate to the two. It is not information in a richer notion of knowledge or understanding. At that basic level, it is possible to interpret the whole of evolution as an information system.

Some philosophically-inclined biologists argue along these lines that the ontology of the kind of system we are looking at in evolutionary terms, is one massive information processing system. I think it is possible to conjoin an evolutionary approach with a view of the system as an information process. This ties in with Friedrich Hayek, who viewed markets are information processing systems. All this implies a sort of fundamental switch of metaphor. We are not dealing with stuff, in the sense of material inputs and material outputs, but with informational inputs and outputs. This suggests that we need different mathematical and other tools to deal with it, like some entropic

measures of order or chaos, which can somehow cope with this complexity in a particular way. Just as Hayek linked together evolutionary ideas with rule-following and groped towards an understanding of the evolution of human societies as the evolution of different rule-following systems, I think we could link together evolutionary thinking with specific stories about how rules and rule systems evolve.

But we have to go further than Hayek to understand those different levels of rules and the different types of rules. Recall the difference between conventions and moral rules. Hayek does not really make that kind of distinction. The origin of law, which is the system beyond custom, which has institutionalised authority and general adjudication, typically through a state machinery, is also important. The evolution of the state and evolution within the state systems of law are relevant because, alongside custom, law is important. Sometimes they come into conflict. We know from the study of firms and economic entities that law is part of the story, but that custom and culture are part of the story as well. There is a very rich agenda linking these aspects together. The big question to me and others working in this area is: how much time should we devote to the general or abstract theories and how much time should we devote to the detail. That is a difficult one to crack because we need to do both, but to do both we need many more people working in this area.

A relevant example here is how modern biology developed, particularly after Darwin. Darwin combined his grand framing theories, abstract notions like natural selection, with detailed illustrations. It took a long time for people to take both parts of that story together. It was only by the accumulation of examples, experiments, shared understandings and shared questions that the two sides of the story – the abstract theory and the detailed evidence – congealed and became a dynamic, scientific agenda with,

since the Second World War, enormous achievements. It took thousands of people.

There are many good people working in this area in economics but not nearly enough to anticipate a comparable take-off. I think we have a lot to do over a long future.

Was this problem of the abstract and the detailed, of the very general and the very specific, already in Veblen?

Before Veblen, this issue of the role of abstract principle and the role of detailed data and empirical investigation was central to the German historical school's rather crude reaction against British classical economics. Both Adam Smith and David Ricardo were criticised for being overly abstract and not understanding peculiar circumstances.

Friedrich List, for example, argued that it was important to look at the detail of how a country develops, to look specifically at education systems and so on, and build up trade from there, instead of justifying it based on grand principles. The early German historicists rejected, or tended to reject, general theory completely. By the time of Gustav Schmoller, there was a recognition that both were required. We know that Veblen read Schmoller, and I think he goes further than Schmoller, because he talks about post-Darwinian principles and the need for what he calls "metaphysical presupposition." He foreshadowed Willard Van Orman Quine, who demolished mid-century empiricism by saying that any empirical investigation requires ontological commitments.

The trouble with Veblen was that he was not a systematic thinker. He did not set up a research project that involved thinking about what empirical investigation might be needed. Veblen did do some empirical investigations, but the lack of a system to his

thinking was partly a reflection of his personality and partly a reflection of his circumstances. He was generally isolated, not because he lacked colleagues, communicants and people with whom he could engage with in discussion, but because he was seen as an outsider and treated himself as an outsider. He was the critic, and he never established himself in a strong position in academia where it would have become important to manage a set of resources or a department, or to recruit teams of researchers. Unlike John R. Commons, who did try to develop systematic thinking, Veblen tended not to. But I think we do see in Veblen an intuition of this sort of complementary importance of concrete investigation alongside guiding principle or argument.

One of his contemporaries who understood this very well was Alfred Marshall.

Ironically, what happened with Marshall was that all his cautions and caveats about the limits of abstraction got written out, and people took the abstract theory, the thing he warned against, and mathematised it, making it the core of textbook micro theory.

Marshall developed the partial equilibrium apparatus, alongside general equilibrium, but always saw the limits of these models. And now students are told that this is basically the end of the story, and that it comes from Marshall, without the caveats, without the warnings. People forgot that Marshall valued historical analyses of individual firms.

Why is historical specificity so important, and how it has informed your thinking?

It is important because there are very few rules that are useful for all circumstances. There are some scientific laws that are relevant, which supersede and transcend human society. But while they are important and maybe indispensable, they can only go so far. It comes back to this question about what is specific about humans, about how we understand human motivation. Since there are particular characteristics which are unique to humans, general principles that apply to everything (like complex system theory) do not tell us anything specific about human systems unless they are framed particularly to suit those systems. General considerations about all human societies cannot tell us anything specific about different forms of human society or different cultures. As we pare down, we end up with fundamental questions, for example about the nature of capitalism. Whether we call it capitalism or not, a major change occurred in human society starting about 300 or 250 years ago, which has led to an immense explosion of wealth, something like 17 times the average GDP per capita in developed countries, which has dramatically altered human circumstances, increased human longevity, and so on. The explanation cannot be one which applies to all human history, or even to human civilisation, because it is something unique: something special happened, some set of circumstances, some new arrangements emerged about 300 years ago, which actually laid the foundations for this major development in human history. That is why historical specificity is important.

We need the combination of general arguments to frame the way we approach the problem and specific arguments applying to particular things. For example, the answer that I would suggest to what constitutes capitalism, to what makes it unique, is in part to do with the development of financial institutions. While financial institutions, including banks and money, go back thousands of years, there is a particular set of institutions,

involving the high development of particular features, which appeared in North-West Europe about 300 years ago, and it could be argued that these are the reason why the take-off occurred. If we understand that there are both specific questions we need to answer, alongside general questions about frameworks that we need to address, and combine those together, we get more insight about what is going on. By contrast, mainstream economics confines itself to a super-general framework that covers all circumstances.

Your approach is quite Aristotelian. These are methodological implications of Aristotle's distinction between essences and accidents.

Yes.

You now use the label "legal institutionalism" to describe your work. There are clear connections with what Warren Samuels referred to as the "legal-economic nexus."

The basic idea behind legal institutionalism is that law involves both the state, in its legislative and judiciary aspects, and private ordering. Legal institutions matter not just for crime and punishment but also for the functioning of the economy, and for understanding key things like markets, exchange, property, the firm and so on. These are very simple propositions, but they are in a sense radical because law is defined in historically specific ways. Consequently, it is a challenge to frameworks of institutional analysis which are ahistorical, like some uses of game theory or some versions of transaction cost analysis, which is not to deny the contribution of those areas. The idea

of historical specificity also goes way back in my thinking, more or less to the beginning. So legal institutionalism is just a term, a title, which encapsulates that. Being able more recently to create dialogues with people in law departments, to improve my understanding of legal categories and concepts, and collaborating with others in that area, has given the idea its recent momentum.

I drafted some early pieces when Warren Samuels was still alive. I always got on very well with him. He was very complimentary. I knew much of his work already, but he pointed me to essays where he had made very similar points himself. It has been a pleasure to discover precursors, precedents, people thinking similarly. For example, Daniel Cole, who is a lawyer, pointed out a critique of economic conceptions of property he had published long before my effort in that area. I now cite that because I am aware of it, but I am sure that many other people have been there before, particularly Commons. To some extent Veblen too, but Commons much more so. There are also people within the self-described new institutional economics who have said that legal institutions are important, although they do not always run with that idea. North, to his credit, talks about the role of ideology, and part of ideology, which is a motivator, is observance of the law. Ronald Coase also insisted on the importance of law. But they did not develop these insights very far.

What are the key areas of policy you have been interested in?

There are areas of policy-making where present or future institutional economics may have limited capability, but also important areas where it already has proved useful. In my 2013 book on *Pleasure Machines*, I explored a number of these. The study of

corruption is a big open door for institutionalism, because we can define corruption as the breaking of some particular rules, and organisational corruption as the breaking of organisational rules that involves more than one agent. Some incentive, some bribe or whatever, is used to break those organisational rules. This is a huge area of relevance for policy in both the developing and the developed worlds. Having a richer understanding of rule-following and rule-motivation, informed by the perspectives we have been discussing, would have potentially important implications. If getting rid of corruption were simply an issue of giving people incentives not to break rules, we would have a rather weak remedy. If it were possible, people would simply adhere to the rules because they would see some pecuniary benefits to do so; it would not be out of any commitment to the rule. To get more people committed to rules and to make rules reputable and viable in society – having good rules, in other words – involves some moral commitment from people.

A second example, which I have explored to some degree, is in health economics. Health economics is dominated by mainstream approaches. There are relatively few non-mainstream people working in it, but it is quite obviously an area where different motivations come into play. We know generally that in places of work, simple pecuniary motivation is not always effective. Human resource practitioners will tell us that payment by results, for example, is often counter-productive, particularly in instances of teamwork and cooperation. These considerations are amplified in the health sector, where people will often say that they became a nurse or a doctor because they want to alleviate suffering and help people with illnesses or injuries. There are layers of motivation which are not unique to that sphere but are dramatised by the nature of the service that people are providing: they are dealing directly with human suffering. Yet

mainstream health economics considers the whole thing as a utility maximisation game; this completely eliminates key issues. Also, while mainstream economists focus on micro-motivation, the context and complexity of health systems are so great that a more general comparative institutional analysis would be useful. This is an important potential policy area.

The third area is global warming and climate change, where in any attempt to deal with the problem the current generation has to make sacrifices and change patterns of behaviour dramatically, without necessarily reaping the rewards. If we are to rely on compensating people for not driving a car or consuming less, then it would be very difficult to arrive at a viable policy. We have to appeal to people's solidarity with others and with future generations, which is a big ask. And yet, much of the policy design in the area focused on narrow incentives, neglecting that big ask. It is partly a political question and partly a question of motivation. Incentives are important. Supermarkets putting five pence on the price of a plastic bag is a way of making people aware, but we cannot just rely on those incentives to get things done. To that important armoury of incentives, we have to have a discourse about solidarity with future generations, and this means widening the agenda in ways that economists are reluctant to do so far.

Any operationalisation of your ideas involves thinking about historically-specific complementarities between legal institutions, economic incentives and moral commitments.

I think so. One of the problems here, which is a generic problem in all social sciences, is that there is rarely any sort of knock-out experiment which shows decisively that one

thing more than another is true. This is very different from physics, where you can look at the equations, work at them, tweak them and conjecture that, theoretically, there could be something called the “Higgs boson particle,” which is then discovered some decades later. One of the problems illustrated most dramatically with macro is that people can interpret things in different ways and see very different remedies as necessary for dealing with the particular problems of macroeconomy. Because there is no way of corroborating this by repeating experiments, it is very difficult to say.

What value do you accord to thought experiments, including Carl Menger’s account of the origins of money?

Menger’s account is not based on anthropological, historical evidence and, as far as we know, is not compatible with that evidence. Still, it is a device to generate and explicate important principles. The Menger origin of money story is a heuristic, and I think Menger understood and explained it in that way. It is valuable because it addresses questions which are not answered by a different approach. For example, the fact that we see a monarch’s head on a note or a coin does not necessarily explain the essence or nature of that coinage or bill. The value of Menger’s claim that money is a spontaneous institution is that it gets at something which may be un-designed in the nature of the institution. But the fact that he uses a heuristic to get at the essence of money also shows its limits, and Menger himself reached that conclusion in “Geld” in 1892, when he considered the problem of the quality of gold. The appearance and weight of gold might seem right, but how does one know that it is not an alloy? It would be very difficult to test the degree of purity of the gold, so the quality authentication problem is

inevitable. Menger concluded that maybe there was a role for the state after all. When the state stamps the monarch's or emperor's head on a coin it is not giving it its nature but is authenticating its quality. It is an informational solution to an informational problem, signifying, imperfectly, the value of the coin. And there are other reasons for the state's involvement. There is the question about the unit of account. How could that spontaneously emerge? We might all agree on silver being the medium of exchange but how do we all agree on the unit of silver? Is it the weight or the size, and what metric of weight or what units of measurements should we use? It is more difficult to see how that could emerge spontaneously. Once you probe the heuristic, its limitations become revealed. That is the way I criticise the Menger story.

I think some post-Keynesian economists get it wrong by simply dismissing the Menger story on the grounds of historical inaccuracy. It may be true that we have no clear examples of developed or extensive barter but that does not falsify what Menger was doing because he was answering a different question. Menger was basically an Aristotelian. He was asking, "what is the difference between something that is accidental about money, like the emperor's head on a coin, and something which is essential to money?" We need some way of understanding the difference, and the heuristic is a method which allows just that. The distinction between what is accidental about money and what is essential to money is a legitimate question. Unless we address it, we can never understand money.

You are a friendly but robust critic of heterodox economics.

Heterodox economics is heterogeneous, and despite attempts by Fred Lee and others to create a common narrative, those attempts are very much a bits-of-this-and-bits-of-that story. Institutions are important, so we tick that box; effective demand is important, so we tick the Keynesian box. For me this does not work. It is just a wish-list of things without an integrative narrative. There are two other problems with heterodoxy. One is that it is politically motivated. I need to qualify what I mean by that, because all economists are politically motivated. Heterodox economists and Milton Friedman all have political agendas, and that is not necessarily a bad thing. All or most good economists, with a few exceptions, have some kind of political driving vision or concern about the world. There is a kind of vague, largely unspoken, left-wing politics which motivates heterodox economists – to be clear, I am not anti-left in any general sense; I am sympathetic to many of the things associated with the left – and which lead them to making the mistake of defining mainstream economics politically rather than analytically.

With some truth, they point to the way mainstream economics is used to validate a certain set of policy positions, particularly pro-market positions. People at Cambridge talk about their kind of tradition as being left-wing economics. I think it betrays what they are doing. The basic mistake is that while all economics is affected by the ideology of the time it is created and developed, neoclassical economics, the basic apparatus, could be used for planning or for markets, and in fact you could criticise it as not adequately understanding markets rather than being a pro-market setup. One of the defects of defining themselves in political terms is that heterodox economists have a problem with the Austrians. But the Austrians have a lot to teach us about information, subjectivism or spontaneous order. All these ideas in the Austrian tradition are very

important. We do not have to agree with them, but we need to understand their significance because they address relevant problems and questions. Yet Austrians are not regarded as allies by many heterodox economists because most of them are politically in a different camp.

Core mainstream theory has more adaptability than heterodox economists give it credit. Instead of looking at their own core theory and their own unifying basic assumptions, they define themselves in part by some political position, which often is not spelled out, except in macro policy. This explains why most heterodox people are macro- rather than microeconomists. Discussions of institutions can go both ways. They can focus on individual incentives and free-rider problems, and view institutions as solutions to incentive problems. From Ostrom's work and the work of others, we know this is not necessarily the case. There can be another agenda on institutions which points to a different set of values, such as cooperation, altruism and so on. And that kind of micro is not looking one way or the other politically. In the *Journal of Institutional Economics*, we have articles which are hugely varied in terms of the kinds of political outcomes they may point to. Keynesian macro points in a particular left direction towards state intervention. The *Cambridge Journal of Economics* seems more keen on that.

Heterodoxy celebrates variety and discussion for good reasons. Everyone involved has been frustrated by the lack of discussion of many important things in mainstream economics departments: inequality and power, or the fact that your standard course on money and finance will not discuss the nature of money. Discussion of the state theory of money is not on the mainstream agenda. By contrast, heterodox economists rightly celebrate the importance of discussing these things. What they miss,

though, is that, to advance, the discussion needs consensus-forming mechanisms. Now, consensus-forming mechanisms can be dangerous because, as we know from the history of science, they can ingrain, they can fuse together systems of thinking which are defective in some way; every system of thinking has its own limitations. But without consensus you simply have discussion. Science works partly through organised scepticism. But if all you have is the scepticism, but you cannot move on from that, you simply have argument after argument, which is characteristic of much of heterodox work. While it does make some important developments, it is not really cumulative, with limited exceptions. The basic post-Keynesian stuff has moved on a little, but it is not that different from what it was 50 years ago.

You are critical not just of some aspects of heterodox economics, but also of the label “heterodox” itself.

I do not think people should define themselves simply by opposition to orthodoxy because I think there are many good achievements, even today, in orthodoxy. To define oneself in opposition to something, particularly when one lacks a consensus about what one’s alternative is, is deeply problematic. The label is not wrong, because there is an orthodoxy and therefore by definition there is a heterodoxy, but it is a wrong way of organising and signalling oneself. If you choose to say, “we are pluralists,” you have to be very vigilant about inviting both mainstream people and heterodox people you might not like, such as Austrians. Pluralism is the claim that we gain by conversation, but then you have to open up the conversation. You have to acknowledge where the orthodox approach might be useful and where it is not useful. Unless you do that, pluralism fails.

It is important to understand the role of consensus. As an illustration, consider what I call the “Joan Robinson problem.” Robinson’s first published paper was on the Shakespeare authorship controversy. It was a brilliant little paper in which she reviewed the evidence to challenge the claim that the man who lived in Stratford called William Shakspere wrote Shakespeare. It also created a connection between the community which was created over the authorship controversy and what was happening in Keynesian economics. The problem with dissent is that you attract cranks: there are not only people who say that perhaps the man from Stratford did not write the plays, but also people who propose elaborate fantasies about what happened, without any evidence. The reason why the cranks can get away with it is there is no academic discipline restricting them, or judging that this or that is a plausible argument based on the evidence. This is the problem with heterodoxy, of organising around the label “heterodox.” Because there is no disciplinary authority, you attract all sorts of views, with different degrees of flakiness. Some of it is very solid and very useful but a lot of it is not. A lot is based on misunderstanding. One misunderstanding is that neoclassical theory is pro-market and equals neoliberalism. Papers of this ilk profoundly misunderstand what neoclassical economics is.

How do you create a consensus? You create it by having incentives to actually accept some limits to the discourse, to avoid continually arguing about everything and accepting any view as a legitimate entrant to the conversation. Those incentives relate to the quality of journals, promotions in departments and so on. Some heterodox economists have tried to organise under the label “political economy,” separately from economics departments. But you cannot produce a viable alternative science alongside the established science. Alternative journal metrics and alternative rankings also fail.

Every time these have been created, they have been quickly forgotten. Everyone ignores them, including the people that might want to publish according to alternative rankings. These mechanisms fail to establish a consensus based on a position of power, and that is the reason why heterodoxy cannot cohere or forge agreement on key principles, despite making some important departures.

The issue is not simply a matter of using versus refusing to use the conventional tools of the trade. We talked earlier about the rise of modern capitalism. Part of that story was the institutionalisation of science. In Western Europe before the 17th century, science was the pursuit of those fortunate enough to have funding to conduct experiments on the side, and to be tolerated for what they said. The institutionalisation of science created an environment in which there was some discussion and peer review. Isaac Newton, for example, had a set of peers who were evaluating his work, who created inspiration and guidelines for development. Institutionalisation of science is crucial for science's own development. It creates opportunity for division of labour, for funding larger-scale projects involving multiple scientists or complex setups or multiple observations. These basic needs for a scientific discipline are proven to have value in history of science.

Perhaps this is a good moment to address your own involvement in the institutionalisation of science.

I have always been interested in getting things done. My mother was a very good organiser and I, to some extent, take from her. It struck me there were gaps to be filled, things that needed to be done, networks that needed to be created. Of course, some

networks already existed, but there were new ideas bubbling up, new approaches and new possibilities, and for that reason I became involved in several things including, in the 1980s, the formation of the European Association for Evolutionary Political Economy (EAEPE). Later, through EAEPE, the *Journal of Institutional Economics* was founded. More recently, I was involved in setting up the World Interdisciplinary Network for Institutional Research (WINIR). What I have learned from those experiences is that they are very time-consuming. They require a lot of effort, and extensive cooperation from other people is required to get these things going. There were mistakes and there have been successes in all these cases.

The thing I currently am most proud of is the *Journal of Institutional Economics*, which has a relatively high impact factor and is growing in size. It is not just confined to one particular approach but transcends disciplines and bridges different perspectives within disciplines, and I get a lot out of doing that. I think it will possibly have an important influence, not just in economics but also institutional research generally. When we drafted the aims and considered the title, there was a question mark about the term “economics” in the title, because this is perceived in certain ways and puts you in a certain position. The journal appears in certain lists because it says “economics” in its title. But we always thought that the journal would be multi-disciplinary. Rather than bringing a particular view of what the discipline is and what its methods are, we decided to identify an object of analysis, mainly institutions, and invite researchers from different perspectives, using different methods, to help us understand the nature and role of institutions. “Economics” was retained in the title because we are talking about economic institutions, about institutions in the economy.

Institutional economics has always had difficulty defining itself. New institutional economics defines itself more or less as the application of mainstream and close-to-mainstream approaches to institutions. There are big differences within that group, but most approaches are either mainstream or have come out of the mainstream and have been adapted beyond it, like Oliver Williamson's work on uncertainty and information problems, for example. The original institutional economics has even more difficulty defining itself. At one time it was, at least in the United States, a prominent school of thought, while economics itself was globally much more broad as a discipline. Proponents of this approach are now pushed to the margins of economics departments and have some difficulty agreeing on what their approach means. They used to have discussions about whether it was all in Veblen or Commons and so on, but now they have more or less gravitated towards a kind of post-Keynesian eclecticism, which is not necessarily a bad thing, but draws less on its original roots in institutionalism.

We are doing things differently with the *Journal of Institutional Economics* by declaring that it is about institutions. The journal has been relatively successful in that regard, because we do get submissions from different kinds of institutional economists and also from other disciplines like law, political science, philosophy and anthropology. There is a quite varied contributions coming into the journal. One reason for the journal's success is that, strangely, it has relatively few competitors. It has competitors on the so-called heterodox side, but these are typically generalist journals, like the *Journal of Economic Issues* and the *Cambridge Journal of Economics*. There is some overlap. An author producing some institutional study could submit it perhaps to the *Cambridge Journal of Economics*, the *Journal of Economic Issues* or the *Journal of Institutional Economics*, so there is some degree of competition, but I think the general

profile and orientation of those journals is different, and the competition is not intense. I would welcome some healthy competition between the kinds of debate being conducted and the quality of the articles themselves.

How do you expect things to evolve?

You could ask a slightly different question: what disciplines do I think are more useful? Another question could be: what do I think might happen in terms of the disciplinary contributions? It is difficult to say which disciplines are more useful. In fact, I would say that some of the best insights we have in modern institutional research are through combinations of multiple disciplinary understandings. North's most recent work, for example, combined political theory with economics, which enriched both his own approach and our understanding. We have a number of lawyers contributing to institutional research. Institutions are also a major topic within social philosophy, philosophy of social science, and philosophy of economics. I think the best contributions will come from people who can talk across disciplinary barriers.

In terms of what is likely to happen, it really depends on how disciplines develop. Things may change, but economics as it currently stands is very much concerned with technique and less concerned about particular real-world problems. I do not say there is no such concern, but that in terms of getting publications and promotions in economics departments, technique is very important. That was not the position 50 or 100 years ago, but that is how economics has evolved. There are certain technical problems in institutional analysis which economists need to solve, but there are relatively few economists who try looking across disciplines and take a broader outlook. I have

mentioned one or two already, but these are the exceptions rather than the rule. I am concerned not only about economics but also about the development of sociology. The problem with sociology is that it is very fragmented. It lacks a dominant paradigm, and often uses terms in its own way. The biggest connection sociologists have with other disciplines may be in organisation studies, which mobilises scholars from sociology departments but also business schools. I would like to see more work in this area submitted the journal.

Based on an extrapolation of what has been happening in the social sciences in the last 20 or 30 years, I think we may see the growth of centres of research outside the strict boundaries of disciplines. There are whole areas, like strategy, which have spun off and are completely disconnected from economics, although that was in part their origin, with the work of Richard Nelson and Sydney Winter for example. But there are also areas, like innovation and technology studies, that have maintained some connection with the previous disciplinary base while acquiring their own autonomy. The strategy for institutional analysis might be to create semi-autonomous areas like that, some organisational identity within academia, either as sub-departments or main departments, rather than as separate entities in themselves. An obvious reason why organisation studies has grown in prominence is the growth of business schools in the last 20 or 25 years and the importance of the organisation to business analysis. It is difficult to see how institutional research would match that degree of attention. On the other hand, given an already existing area of innovation studies, why not organise around institutional innovation studies? Institutional innovation is just as important as technological innovation.

Many funding councils in Europe say that preference is given to interdisciplinary research, but in practical terms, there is not very much done. One reason is that interdisciplinary research is actually very difficult. Science proceeds, in large part, by specialism, by people getting interested in a particular problem, creating a community of researchers that probes the problem further, does experiments, and gathers data. By honing down, by refining the question, by focusing on particular phenomena, science makes progress. Occasionally, syntheses occur. Also, there are huge costs to interdisciplinarity. To ask a scholar to be interdisciplinary, is like asking them to learn another language: it is very hard. It takes several years to learn one particular sub-discipline. This is not something everybody can do. And it is not something that everybody should do, because we need specialists. Given the costs and risks associated with interdisciplinarity, despite all this lip service, it should not be pursued universally. Multi-disciplinarity is less ambitious, where an attempt is made at conversation between people addressing a similar problem.

Multi-disciplinarity has been successful in some areas, as with the study of human cooperation, where there have been important conversations between economists, biologists, psychologists and primatologists about its evolutionary origins. But those are rare cases, and even then people tend to silo into their own areas and specialisms. We should value interdisciplinarity but also be extremely cautious about it. It is something which we have to promote, but within its own limits, within its own boundaries. Any attempt to create a multi-disciplinary forum to discuss institutions must recognise that there will be specialisms, and that conversations will be limited because there will be problems of communication.

WINIR is a forum for people from different disciplinary backgrounds, speaking different disciplinary languages. WINIR conferences and symposia seem to have been quite successful. But I think that we have to be careful about being over-ambitious and trying to achieve things too soon. Perhaps the way forward is simply to signal the possibility, by keeping this organisation going, and hope for a time where greater resources will be mobilised in this direction. The journal is a testimony to the viability of the project of attracting multiple disciplines to create a conversation. Much of the growth in the citation impact is through conversation in the journal. In earlier phases, citations were to a minority of articles but as the journal has grown and received more attention, it has created a conversation within its own community. I think we can cautiously move forward on that basis. It is not going to be easy to get huge amounts of money, and I would not expect large numbers of research centres focusing on institutional research, but if we are patient, I think we may well see that in the near future.

What would be your advice to prospective PhD students?

I think the most important thing for a PhD student is to have some view of what is viable in research and what will offer them a career. I think one of the strategic mistakes made by heterodox economics is to try to change economics, without sufficiently acknowledging that before this is achieved the career prospects on offer for PhD students are very poor. You cannot ask students to sacrifice or diminish their career prospects for the sake of a venture which may or may not succeed, or may or may not be welcome. Students must have some plausible career plan now, as things currently

stand. That said, there are places which allow a broad-minded, well-established PhD student interested in non-mainstream perspectives to actually prosper, and that includes business schools. It is no accident that many dissident economists, in the UK at least, are in business schools. It is a different story in North America, where the character of business schools is different, and it is a different story on the Continent, where there are fewer business schools relative to academia.

We would like to thank you, Geoff, for your time and your contributions.

Thank you, not just for this interview but for this wonderful initiative in putting this *festschrift* together. I am very honoured and very grateful.

NOTES

- 1 This chapter is an edited transcript of two interviews with Geoff Hodgson conducted on 11 and 25 October 2016 at the University of Hertfordshire.