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The Secret Life of Institutions : On the Role of Ideas in Evolving Economic Systems, Entretien avec Mark Blyth

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The Secret Life of Institutions: On the Role of Ideas in Evolving Economic Systems Entretien avec Mark Blyth

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Mark Blyth is an Associate Professor of Political Science at the Johns Hopkins University in Baltimore. He received his Ph.D in political science from Columbia University in 1999. His research interests lie in the fields of comparative and international political economy. He is the author of Great Transformations : Economic Ideas and Institutional Change in the Twentieth Century (Cambridge: Cambridge University Press, 2002). He has recently finished several joint projects. An edited volume on institutional change in American city school districts "The Transformation of Great American School Districts"; a volume on constructivist theory and political economy called 'Constructing the International Economy'; and a volume that surveys IPE around the world entitled 'IPE as a Global Conversation.' He hopes to finally finish a book on party politics in advanced welfare states called The New Political Economy of Party Politics (with Riccardo Pelizzo and Jonathan Hopkin) that has taken far too long so that he can begin working on a book called The End of the (Liberal) World? His articles have appeared in journals such as The American Political Science Review, Perspectives on Politics, Comparative Politics, and World Politics

RR : *We would like first to improve our knowledge of your work. Could you present the core propositions of your institutional and ideational theory?*

Mark Blyth : To do so I have to turn this into an evolutionary story. I went to graduate school in the US, at Columbia University, in the early 1990s, a time when the idea that 'ideas matter' in explaining political economy outcomes was anathema. Rational Choice theory was sweeping political science in the US and the prestige of economics was at its neoclassical high point. Prior to this I had come from Scotland where I grew up under Mrs. Thatcher, and to anyone who lived through that time the proposition that 'ideas matter' was blindingly obvious since the Thatcher project was predicated upon the reconstruction of political interests, by recasting through ideas about 'how the economy works,' the institutions and outcomes of the post-war institutional order as an unmitigated failure. As such, much of what US academia was producing; institutions as 'efficient,' voters with rational expectations etc., made no sense to me. What little that did resonate was the emerging historical institutionalist literature; the work of Sven Steinmo and Peter Hall in particular.

What I liked about Historical Institutionalism was its 'hidden story'; the mechanism through which agents get their preferences from the institutional context. This seemed to me, despite the view of many historical institutionalist scholars, to be a primarily cognitive phenomena. But thinking this way confronted a problem. If agents get their ideas about their interests from their institutional context, why would they ever get the 'idea' to change the context? That is, how can we explain institutional change. The usual answer; exogenous shocks, seemed problematic since the fact of institutional disruption said nothing about the path institutional

reconstruction should take. Thinking about this problem led me to focus on uncertainty and how agents think about their environment in moments of crisis.

The problem here was most of the US literature treats uncertainty as a problem of risk, which while making it tractable, defines the problem out of existence. So I went back to Shackle, and then Keynes, and then Knight, and began to think about conditions of Knightian uncertainty. That is, situations where agents are unsure of what their interests should be since the situation gives no guidance on how to rank priors; it is an a-probabilistic uncertainty. Mathematicians usefully call this 'wild type' uncertainty. This led me to think about the process of institutional change under conditions of Knightian uncertainty, which became the core of my first book, *Great Transformations*.

In this work I posited a model where once an institutional order is destabilized, that is, 'normal' behaviour no longer produces convergent expectations among agents (deflation is a classic example), political entrepreneurs introduce ideas about 'what has gone wrong, who is to blame, and what to do about it' into this situation in an attempt to reduce uncertainty, promote collective action, and provide the blueprint for new institutions. I liked this model, and I still do, but something about it bothered me. This mechanism seemed to fit the 1930s really well, but were the 1970s really a period of Knightian uncertainty? Surely ideas were important in both periods, but perhaps in more different ways than the book admitted?

At this juncture I became interested in randomness and emergence as serious issues in studying political economy, primarily through meeting Nassim Nicolas Taleb. The problem I got from Taleb was 'the non-observability of probability distributions' and how humans deal with this. At base this resolves to the problem of probability distributions' parameter estimates and the problem (and complexity) of non-observable generators in dynamic systems. The basic story is this. Agents assume they can see the generator of outcomes in the social world and plan accordingly (in a probabilistic sense) given the sample of events they have. In some cases they can in fact do this. The US Congress, for example, is pretty predictable. In some cases however they cannot. Take the global economy as a generator of outcomes, for example. You literally cannot see the thing. Most people fail to make the differentiation but still assume that sampling the past is a good guide to the future regardless of the observability and complexity of the generator(s).

The key problem here lies in the notion of an 'adequate sample.' To get an adequate sample of events, one assumes that one knows where the parameter distributions lie, yet to know where they lie one needs an adequate sample. Ignoring this problem, we further assume more information is better than less and that the $E(x)$ and $V(x)$ lies within some 'knowable' distance of the real mean and variance of the observed distribution. Thus, by sampling we converge upon the world 'as it really is.' We then build a nice Gaussian world (even if its skewed) by sampling more and more of the past and assume far more stability than is warranted given our assumptions of normalcy. Unfortunately, we then get smacked by 'Black Swan' events that none of information in the sample could tell us before the fact (think the end of the cold war, the recent oil price spike etc.) that blows our ideas about how the world works apart. Now, here's the really interesting bit. If you can directly observe the generator you can predict (within tolerances) because we sometimes live in a world of risk. Direct observation of generators and risk go together. You can then really assume that agents behave like a classic rational agents and you can model this with whatever behavioral rule you like. If you cannot, then you live a world of uncertainty, where assigning a logic of probability with simple behavioral rules is doomed to failure.

Now assuming this is plausible, how do agents deal with this? My answer is by becoming 'default constructivists.' It strikes me that no-one (including the most hard

core neoclassical economists) really make the type of calculations risk models assume in making decisions. But agents do act in the world in what are sometimes predictable and sometimes wildly divergent ways in supposedly similar circumstances. That stability, I posit, comes from the human desire/need/viscera to reduce uncertainty. Whether through ideas, ideologies, institutions, cultures, laws, Bat-Mitzvahs, or dinner parties; all that holds the world together is the social stuff that people put out there in the process of 'being human.' Human beings are for very practical reasons 'default constructivists.' The ideas people have about how the world works and the convergence in expectation and actions that this allows clearly matters all the time, but their valence and variance is a function of the observability and complexity of the generators agents face. Some parts of our world are directly observable and are governed, more or less, by risk. However, it then occurred to me that the more one moves from situations of risk to situations of uncertainty, the more constructivist one becomes, the more one gets over the binary that 'risk/uncertainty' implies that I had been struggling with for so long. This is not however, I stress, to imply that being 'more constructivist' is somehow better than being less constructivist. Only that some parts of the world we seek to explain may be more socially constructed than others. This is basically how I see the world.

R.R. : How do you analyse both the articulation between institutions and ideas and their relative autonomy?

M.B. : I have actually moved a bit away from such a formulation of the problem. To posit a relative autonomy of ideas assumes that they are tied to materiality in some way, at some point in time, but are somehow 'more free' and therefore 'more important' at other points/times. This is consistent with the way I used to think about the problem; in periods of uncertainty ideas matter more since their relative autonomy is greater. I would now say that such a position is not wrong, but incomplete. Let me give you an example as to how I try to think about this problem now from a wonderful new book by Charlotte Epstein, a French scholar who works in Australia. She asks the following question (I paraphrase here); why can't we kill whales? We can fish every last tuna and cod out of the ocean, walk on the backs of Minky whales from the US to Japan without getting our feet wet, and yet we can never kill them again; why? Is it because they really do 'sing' or are 'intelligent' above all other species? Probably not. For such anthropomorphisms show, the degree to which we have socially constructed whales as species uniquely worthy of protection is critical here. Our ideas about whales constitute whales 'out there' for us to the extent that, as Epstein argues, to be a 'civilized' nation means to forswear killing whales ever again.

Now, what does this tell us about ideas and their 'relative autonomy'? I think it shows how in some cases our constitutive notions of what an object is creates a subject-position so strong that the idea and the object are completely intertwined. Whales, like mountains, have no intrinsic value or meaning apart from the ideas we have about them. Given this, the relationship between ideas and institutions is similarly constitutive. Institutions 'work' to the extent that they reduce uncertainty and coordinate expectations to produce outcomes within defined parameters. Ideas are both the blueprints behind institutions (could you have the ECB without a monetarist theory of inflation, for example?) and the media out of which institutions are constructed (why do you have to be a conservative central banker? Because that's what the markets expect given their ideas about the role of central banks.) These days I see these relationships as much more generative and mutually constitutive than 'relatively autonomous.'

R.R. : In your work, you apply interestingly the notion of path dependence on ideas but not specifically on economic science. Could you give us some illustrations of intellectual locking in the field of economic ideas? What are the factors of increasing returns and what is the role of contingency in this field?

M.B. : Actually, I have written a bit about the path dependence of economic ideas. On great example was the hold of ‘normpolitik’ in Sweden in the late 1980s and early 1990s. Swedish economists had gotten into a panic about inflation in the late 1980s, which was ironic since it was caused, in part, by the liberalization of credit markets that they themselves supported. In seeking to control inflation, and indirectly put pressure on wages and tax levels, an external anchor was sought in tying the crown to the D-mark. The problem was Germany unified, the European economy slumped, Germany jacked up interest rates and the Krona collapsed, which caused a massive contraction of the real economy. Despite all this the government held to the idea of fighting inflation by maintaining the ‘normpolitik’ despite there being a deflation going on all around them.

Another example comes from US financial markets in the early 1980s. Monetarism had gained popularity and became the prism through which bond market actors viewed events. By the early 1980s the US economy was in recession and the deficit was rising. As such, movements in M1B (the money supply measure) were increasing. This (according to the theory) portends higher inflation, despite the fact that the economy was in a slump. Thinking that there would be higher inflation in the future made bond markets demand ever greater interest rates in the present, which fed through into higher real rates in general, thus deepening the recession. This in turn led to more government spending, more monetary growth, and more upward pressure on rates.

Presently, the movement from equities to real estate to commodities as a series of investment bubbles shows the same dynamics. Each bubble becomes a bubble because there is a shared story among agents as to why this sudden rise in price is singularly not a bubble (‘the new economy,’ ‘historic house prices/demographics,’ and now ‘Chinese demand’ become the villains of the piece). With this shared narrative in mind over-investment becomes rational and reasonable, thus stoking a bubble. If this isn’t cognitive path dependence (herding) I don’t know what is. Increasing returns sets in until the bubble bursts, and eventually all bubbles burst, but when that happens is the moment where contingency strikes, and being contingent there can be no a priori theorization of that moment as per my comments about randomness and sampling above.

R.R. : What are the factors of changes in ideational sets? How does an ideational referential enter into crisis?

M.B. : There is no easy answer to this. In terms of referentials, a great way of thinking about these issues that hasn’t really made it out of the French literature into the Anglo literature, and how enter into moments of crisis, I really think this is where contingency gives way to emergence as a property. If we think of moments of crisis as moments of a-probabilistic uncertainty where expectations are no longer fulfilled due to the failure of institutions, then referentials are a combination of entrepreneurship (someone has to diagnose what has gone wrong and what to do about it) and the ideas ‘laying around’ in that moment. You can have essentially very similar conditions in different cases and yet have a huge variety of diagnoses, and hence potential institutional resolutions, in play at the one time. Which one ‘wins out’ is always underdetermined. As such, which ideas ‘win out’ is best thought of as an emergent property of the moment rather than the quality of the idea or the

structure of the situation. If, for example, one common factor, deflation, can give rise to ideas as diverse as ‘kill the Jews’, ‘invade China’, and ‘invent compensatory macroeconomics’, and the 1930s did, then such questions again offer no a priori solution.

R.R. : How does a process of convergence toward one referential happen? What are the social processes behind it?

M.B. : The American way of asking that is to say “why does one idea win out over others?” My honest answer to that is, as per above, if I could tell you that as an a priori apart from the specifics of the context beyond what I have said already about how ideas, uncertainty and institutions interact, I would be a billionaire. This would be the case since a corollary of this line of thinking is an a priori theory of technological adoption “which technology will win out” since, in a sense, technologies are ideas too. If I could do this, I could clean up in equities markets worldwide and retire to my private island. Since I am still working, I can only say such knowledge alludes me.

R.R. : Has your approach practical and even normative implications?

M.B. : The practical implications are many; but I will just mention three. First, no one really knows as much as they think, but the fact sometimes most actors think the same way tells us an amazing amount about why systems remain stable or become unstuck. Second, and relatedly, it really doesn’t matter what you think agents think, what matters is what they think. If seventy percent of Americans think they are middle class, an army of intellectuals can tell them otherwise and inform them about what their ‘real interests’ should be, and it will not matter one iota. They are what they think they are, and will act in ways that reaffirm their self-conception regardless of what academics tell them. Third, the world is much more deeply socially constructed than we think and rests upon foundations far more malleable and fleeting than materialist approaches assume. Since the distinction between the normative and the positive is a false and untenable one, I would merely say that a variety of normative stances are possible from these positions. However, I do not think one particular stance is theoretically preferable to others. It’s up to the individual to judge how their own normative stance affects their work and to accept that on its own terms. What is dangerous and anti-intellectual is when normative claims such as “my work focuses on the poor/downtrodden/others/subalterns etc. and your doesn’t” becomes a claim to normative superiority; a trend I sometimes seen in work in International Political Economy. Crudely put, such a stance may be morally preferable, but I am on no obligation to embrace a particular politics by virtue of being a scholar.

R.R. : Both conventionalists and regulationists have been struggling to understand the role of ideas in societies and in the transformations of basic institutional forms. We would like to hear your opinion about these research programs and how they relate to your own work.

There is currently a vigorous debate on the “performativity thesis” of economics between Michel Callon and Daniel Miller, among many others. In your research, you give many examples of the practical failures of some dominant ideas that turn to be at odds with reality. At the same time, you show for instance that the belief that deficits cause inflation will produce the self-fulfilling law that deficits cause inflation, regardless of the empirical reality. Could you qualify your position on what performativity means in economics?

M.B. : This is a huge and important question. Yes, the deficits cause inflation example is both an example of ideational path dependence, as per above, and also an example of performativity. I wish I had coined the term, but I didn't. Instead, I have recently been writing about ‘reflexivity’ and ‘recursive looping’ (I think I got that term from Ian Hacking), which is really the same thing as performativity. The work of all these scholars marks, for me, one of the most significant advances in scholarship in this area in many years. But the question ‘how performatively constituted is the economy’ is a bit like the broader question of ‘how socially constructed is the economy.’ The answer is, it depends. Some moments are particularly good examples. I find McKenzie’s work on Black-Scholes’ options models and Langley’s work on the construction of financial subjectivities particularly strong examples. But in a general sense we all perform the economy every day. When I kiss my wife goodbye in the morning and head off to work I do not say “see you later darling, I’m off to replicate the structures of late capitalism” even if that is exactly what my action, in combination with everyone else’s actions, ends up doing. But if everything is performance, then the concept loses its ‘bite’ and it ends up a clever re-description of everything and an explanation of nothing. I think a similar fate befalls a lot of concepts. Consider what happened to the concept of credibility. Originally narrow and specific and meaning it has been applied so widely that everything from civil wars to pension reforms become generic ‘credibility problems.’ I hope a similar fate does not befall performativity.

R.R. : In a seminal work for the French sociology and the ‘economics of conventions’ school, On Justification: Economies of Worth, Boltanski and Thevenot examine a range of situations where people justify their actions. The authors argue that justifications fall into six main logics. Although they are competing, each of these logics of justification displays a high degree of internal coherence. Do you think that ideational regimes need to exhibit a strong coherence or, on the contrary, that a “catch-all”, contradictory ideational set can boost the power of ideas?

M.B. : I think that the evidence cuts both ways, but that there are limits to how coherent or incoherent sets of ideas can be and still have traction. Boltanski and Thevenot’s multiple logics is in a way getting at these limits. I will try and illustrate this with three examples. Great Transformations is essentially a conventionalist account of the economy, especially in its understanding of how institutional stability is constructed. To the extent that economic ideas can coordinate expectations, and to the extent that this becomes self-fulfilling, I see a clear link between conventionalist and performative scholarship. Now, turning to coherence/incoherence, a great

account of how incoherence as ‘multiple voices’ can in some circumstances ‘work’ for actors comes from Nicolas Jabko in his book *Playing the Market*. Jabko shows how the European Commission, a weak actor at the heart of the EU, acted as the generator and transmitter of some pretty amorphous ideas. For Jabko, the Commission “sold” the project of integration to different constituencies according to logics those constituencies wanted to hear. Creating constituencies by being deliberately vague with its ideas, the Commission was able to bring other actors on board and create momentum for a series of institutional reforms that was not reducible to the supposed material interests of the actors in whose name these reforms were made, including the Commission itself.

On the other hand ideas can become too coherent, and thus fragile, as technologies in the world. A great example of this comes from Jacqueline Best’s *The Limits of Transparency* where she discusses the breakdown of the Bretton Woods system. For Best, while Triffin Dilemmas and dollar overhangs all mattered in explaining the collapse of the system, what was also important was the original flexibility, what she terms the ‘constitutive ambiguity’ of the Keynesian ideas that informed the system’s architecture. However, as the neoclassical synthesis codified Keynes and threw out that which could not be formalized, so ambiguity was reduced to information, and as these ideas became more coherent and more rules based, they became less able to enable actors flexibility of response in a changing environment.

As such, I think that there are ‘fat tails’ to the distribution of possibilities on both ends. Ideas can be more or less coherent and still have effects; but they cannot be completely on one side or the other. There are limits on each tail, but where that limit lies is not knowable as per the discussion of sampling above.

R.R. : Régulation research started from a rather materialistic approach of institutional analysis but the impressive come back of free market ideologies have triggered a specific area of research on the role of ideas. Hence the following questions:

Analysing economic policies, Frédéric Lordon argued that – contrary to the model of rational expectations, which assumes high cognitive capacities – representations are most often informed, shaped by elementary schemes. Do you agree with Lordon’s “strength of simple ideas”?

M.B. : In a word, yes. The idea that agents have rational expectations was only justifiable to the extent that it was supported by the efficient markets hypothesis and the second fundamental theorem. That we see ‘miles out of equilibrium’ markets in assets-classes all over the world today has somewhat diminished the credibility of these supporting claims. If this fails then next standby supporting argument is usually ‘well, its only a useful modelling assumption; ‘as if’ and all that.’ But this fails on two grounds. Its not useful for either explaining outcomes or for policy, since even if it were right it would be basing policy on random correlations, which is dangerous at best. Apropos what I said above, I think the work of people like Best and Jabko show that simple ideas can be strong. Indeed, one of monetarism’s strengths was that it was a simple and clear idea – ‘Money Matters.’ But this was also its fragility. If it was right, the answer was easy too; control the money supply. And when that proved much more difficult the theory (as policy at least) proved vulnerable.

¹ Lordon F. (2000), « La force des idées simples. Misère épistémique des comportements économiques », *Politix*, vol. 13, n° 52, http://frederic.lordon.perso.cegetel.net/Textes/Textes_recherche/Textes_PolEco/Politix.pdf

R.R. : What do you think of Lordon analysis' of "competitive disinflation"? He is stressing the fuzziness and constantly evolving intellectual construction that has been mobilized in order to justify the end of the past economic policies which sustained the Fordist regime and were inspired by Beveridge and Keynes²

M.B. : I agree with Lordon on this point too, but would amplify it in the manner of Jabko to point out that by being fuzzy and constantly evolving, such ideas in part derive their power.

Bruno Théret and Bruno Jobert have attributed a key role to the European Commission and debates in the diffusion in France of neo liberal ideas³. According to your own understanding, what is the role of European and international networks in the diffusion of free market ideas?

This is a literature that I am a consumer of rather than a producer of, but I think that scholars such as Théret and Jobert, as well as Jabko and Vivien Schmidt all point to these European, specifically EU networks, as incredibly important conduits and generators of these ideas. I really have nothing of my own to add to what they have said.

R.R. : By contrast, Amable & Palombarini follow Gramsci and analyse the formation of dominant social blocs. Actors, expectations and interests are moulded, mediated by ideologies. For them, it is only the interaction of interests and ideas mobilized by social groups that explain institutional change. Does it fit with your own analysis?

M.B. : Here I differ. I find Gramscian scholarship to be problematic for three reasons. First of all, the empirical cases presented by people such as Jobert and Jabko show that how things actually 'come to be' is a process that is certainly not in the hands of a given self-appointed hegemonic block of actors. Projects wander, go off track, move across institutions, and come to fruition in odd places. Cornelia Woll's wonderful discussion of how firms form their interests in her book *Firm Interests*, clearly shows that far from being already constituted actors with class derived and asset produced interests, many firms have no clue what their interests should be and actively eschew lobbying, or worse even lobby for what would be, from a materialist standpoint, the wrong things. Second, Gramscian scholarship often falls into what I call the 'corner of the hermeneutic circle' problem. If everyone is walking around in a fog of false consciousness called hegemony, what makes these guys immune to it? The answer, that they have the correct theory of history to know 'what's really going on,' presumes an Archimedean point of 'truth' and independence of subject and object that I find unsustainable. Third, the type of scholarship I produce as a constructivist is predicated upon there being no theory of history apart from the one actors themselves believe. As such, academics can put people into classes, social forces, and historic blocs all they like, but unless the actors on the ground identify with these categories, it's meaningless. Unless one attributes to them a 'false consciousness' of their real interests, which falls, as above, at point two. I was once a Gramscian, but I found these problems too difficult to overcome and had to move on elsewhere.

² Lordon F. (1999), « Vers une théorie régulationniste de la politique. Croyances économiques et pouvoir symbolique », *L'année de la Régulation*, vol. 3, http://web.upmf-grenoble.fr/regulation/Annee_regulation/AR3-1999-06LORDON.pdf

³ See for instance, Jobert B., Théret B., (1994) « France : La consécration républicaine du néo-libéralisme ». - p. 21-85. In Bruno J., ed., *Le tournant néo-libéral en Europe. Idées et recettes dans les pratiques gouvernementales*. - Paris : L'Harmattan, 328 p.

R.R. : We would like now to have your assessment about the relevance of regulationist approaches in general and how they eventually relate with your own research agenda.

When have you first heard of the French Regulation school? Is Regulation theory actively discussed, in some circles in the United States? If so, which ones?

M.B. : American academia is very dynamic, to that point that one might even call it ‘ambulance chasing.’ Fads appear and disappear with regularity. The Regulation school was a bit of a fad in the late 1980s and early 1990s, but unlike most fads it has had a lasting impact along three metrics. First of all, on a personal level, when I was in graduate school I attended two political economy seminars in my second year. The first was run by Helen Milner. It was thoroughly orthodox and neoclassical. The second was run by Mark Kesselman and was heterodox and historicist. It was there I first read Boyer, Aglietta, Drache, and other kindred spirits such as Jessop and Harvey. I used to call this class ‘my antidote’ to the other one, but with reflection I would now say that both were most valuable when read against each other. Along a second level, although it has its fans in comparative political economy in the US, regulation school approaches and concepts are more popular among sociologists and geographers, in the sense that they employ the framework in more full form than in political science. These days the effect of the regulation school is mainly seen in its (somewhat unacknowledged) influence on the so called Varieties of Capitalism literature. Third, its lasting effect has been a more general appreciation of capitalism not as ‘politics and markets’ as two separate things, but as a set of interrelated institutions that evolve and replicate over time. In this most general sense it has been very influential, even is most people don’t know the origins of the ideas themselves.

R.R. : The Regulation approach, like your own approach, is part of historical institutionalism. Which are, according to you, the most important convergences and differences between your approach and the regulationist one regarding for instance the role of crises, factors of change, comparative economic systems analysis?

M.B. : Doing this question justice would require an essay in its own right. I would put it this way: ‘We are all regulationists now.’ That is, the view of capitalism as a set of interdependent institutions that go far beyond the politics and markets binary is now so commonly shared as an operating assumption, that we start from the same position. Where I differ is that at higher levels of abstraction such as regimes of accumulation and modes of regulation, I tend to stress the constructivist dynamics of such systems more. Since economic outcomes are always mediated (the stock market may have fallen and inflation may have went up, but what that means needs to be interpreted and shared to be acted upon) agents responses are not simply a function of their institutional position, especially in moments of crisis and change, which I tend to see as more random and under-determining than most. Also, regarding separate systems and their analysis, I think we have to be more careful these days on making and marking theses boundaries. For example, stock markets around the world are becoming increasingly correlated with one another. As such, investors want to maximize their diversity in a portfolio with small variation (they want to increase their beta relative to their alpha). So what moves counter-cyclically to equities? Commodities, of course. But if we are all thinking the same thing about the same serial correlation, then we all move at the same time to get more diversification, we end up shrinking beta across all portfolios, hence the commodities bubble of the

moment. Such phenomena are increasingly cross-systemic and increasingly important.

R.R. : Besides, are social scientists not always lagging behind the evolution of economic systems, praising one model at the very moment when it will enter in an endogenous crisis or even depicting a model the “same as it never was” to quote the title one of your articles?

M.B. : Yes, and that’s why I want to be careful about these boundaries. The positing of social equilibria, especially as regards very large institutional complexes, needs to be done with great care. If one thinks about such things evolutionary, one can posit the US as a particular variety of capitalism and Germany as another, and give lots of good reasons as to why these forms are stable and efficient. And then along comes China and a housing bubble (random events) that changes the environment for both systems and destabilizes the equilibrium.

R.R. : Do you perceive a convergence with regulation analyses, when they point out that one early warning of major crises is precisely the diffusion of the belief about the opening of a new epoch by a model immune from any fragility?

M.B. : Yes, but if I had a dollar for every time a political economist predicted an impending crisis, which then either fails to materialize or is traversed with much more ease than expected, I would be a very rich academic indeed. This can indeed be one source or prelude to a crisis simply because portfolio diversification is a good idea in any complex system. If you have all your ideational eggs in one basket and your drop the basket, then there can be no more omelettes. However, I do think we need to pay more attention to the capacity of capitalist systems to absorb such shocks. That to me is the really interesting question.

R.R. : As we said, in the 1990’s, the French Regulation theory was led to integrate more and more the role of ideas, beliefs and legitimization. Lordon argued that this “hermeneutic turn” is compatible with the open structuralism of Regulation theory. What is your own (meta)theoretical framework regarding ideational theory?

M.B. : I think I would have to point you to my answer to the first question. That’s really my meta-theory to the extent that I have one. The world is more random than we think. Complex generators and emergent causation is real. We construct the stability we seek to explain. Start there and move on.

R.R. : It is interesting for us to know how you analyse the contemporary transformations of academic research and what should be the strategy of the heterodox community facing a strong orthodoxy

M.B. : It is sometime hard to grasp for a young French heterodox economist that our heterodox counterparts in the USA are more to be found in departments of political science than economic science. In a recent interview, Geoff Hodgson, spoke of becoming a refugee in business schools. What is the evolution of economic heterodoxies in American universities compared to European paths?

American heterodoxy is a bit like American Catholicism; honoured in the breach. Some of the most heterodox of American economists, such as Bowles and Gintis, made their peace with the neoclassical mainstream a long time ago. To the extent that there is a heterodox tradition I would point to American Post-Keynesians such as Paul Davidson, more general mainstream critics such as James Galbraith, and

political economists in political science departments. There is no evolution so much as a scattering. Indeed, at the top universities political economists more and more try to copy mainstream models and techniques to the point that they become economists. As such, the long term value added of being a political economist when there are already lots of economists comes into relief. On the other hand, the orthodoxy is changing with behavioural economics and the like, so there is now more room for different approaches. “This is not to say that there is no heterodoxy in the US. Indeed there is, but it looks very different from the post-Autistic movement in France. Its less organized and is found, to a large degree, in political science departments.”

R.R. : What do you think of the newly flourishing neo-classical production on institutions and institutional performance, which has a very impressive “strike force” and is able to ignore overall the earlier achievements of heterodoxy in this field?

M.B. : I’m not sure if ‘able to ignore’ is completely fair. The way neo-classicals understand institutions is a thin and reductionist understanding, but it is at least a relative broadening and point of contact with others, and that can only be a good thing. As for ‘ignoring earlier achievements’ there is a line in Saul Bellow’s novel *Ravelstein* when he says something like “a happening is not a happening until it happened in Paris.” I think this can be usefully extended to America. Academics constantly reinvent the wheel. That they do so in ignorance of what has come before I do not doubt. But if that ignorance were deliberate, surely they would have copied a better version than the one they ended up with? Moreover, there is a simple language issue here. I wasted my life learning Russian and German and have a terrible and inadequate grasp of French, which I actually want to read. There is a double standard in the global academy that we have to accept even if we don’t like it. There is no obligation for French authors to publish in English, and none for Americans to publish in French. But to the extent that “a thing is not a thing till it happens in America,” if its not published in English, it can never be acknowledged, at least publicly, as a ‘thing’.

R.R. : Let us have an institutionalist view on economic science itself. How could we develop supporting rules and organizations in order to promote alternative indicators and interpretations of the economic world? What could be done further to give more institutional and organizational flesh to historical institutionalism?

M.B. : I’m not convinced that we should go down this path. True, different indicators such as HDI over GDP etc. are good and useful, but their instantiation as practice by everyday actors is extremely hard to fashion. How do you make, for example, a pension fund use human development indicators, and to what end? Similarly, I’m not sure what organizing an institutionalism entails. I like to think of political economists as ‘the Bullshit Police.’ We go round sniffing claims about the world made by agents and examine the degree of bullshit involved in them. It may not be a world-historical role, but it is I suggest a very important one nonetheless.

R.R. : Finally, we would like to ask you how your personal itinerary has shaped your research.

As a born Scott and an “American-by-default”, you are a kind of go-between the USA and Europe. In your student and researcher curriculum, you have been travelling between political science and economics too. How has it shaped your work?

M.B. : It has given me the confidence to be both anti-European in some respects (insofar as I refuse to have my work nailed to a particular ‘good’ politics that I am supposed to embrace as an academic) and anti-American (insofar as I reject the naïve positivism that characterizes such work). In a way I am a walking ‘proof’ for post-structuralism. When I go to conferences in Europe, particularly in the UK, I am often seen as a positivistically inclined mainstream liberal with dodgy politics. When I go to some conferences in the US I am seen as a baby-eating relativist with no standards. How has this shaped my work? Why do you think I take the social construction of categories so seriously?

R.R. : A very anecdotal question: you are quite an expert in football game analysis and you even did some participant observation in Berlin at the last football world cup. Douglass North made a simple analogy between institutions as rules of the game and organisations as players. Could not more be won out of the examination of concrete games for institutional analysis?

M.B. : I wish it could. Writing about football is more fun than writing about markets. The analogy ultimately breaks down though when you consider that the rules of the game often shift, quite unexpectedly, and actors struggle to reinvent the game while they are playing it. This is what we study. Football and physics are easy by comparison.

R.R. Have you got any thing that you would like to add, perhaps in terms of a summary statement?

M.B. : Nothing but to say thank you for this opportunity. One day I hope to be able to do so in French.

R.R. : Thank you very much for your participation!

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