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Abstract

This paper is an overview from a personal perspective on the various ways Lucas has shaped today's economics in general and in terms of 'Chicago economics' in particular. In honor of the 50th anniversary of its publication, much focus is given to his 1972 neutrality paper and its impact. I discuss how the paper was a trigger of the subsequent emergence of rational expectations macroeconomics. Further, I touch upon his fundamental contributions to growth theory, asset pricing and the characteristic use of the Bellman equations. After covering these topics, the paper concludes with a portrayal of the Money and Banking Workshop to describe the environment that Lucas established at the Chicago department, and to illustrate his enduring influence on the culture of teaching and discussing macroeconomics at the University of Chicago.

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Keywords: Robert E. Lucas, Chicago economics, Monetary Neutrality, Microfoundations, business-cycle theory, Growth Theory

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I am grateful to the continuous encouragement and guidance by Peter Galbács. He went beyond the call of duty not only in his cheerful enthusiasm and productive prodding, but also in offering exceptional guidance, detailed reading, many constructive suggestions, and much help. All errors and inaccuracies are mine, of course.

The lasting influence of Robert E. Lucas on Chicago economics

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1. Introduction

Chicago has had and still has an amazing share of great economists, of giants in the field. Nonetheless, the enormous and lasting influence of Robert E. Lucas on economics at the University of Chicago (and elsewhere) can hardly be overstated. His legacy lives on in so many ways. He shaped the Money and Banking Workshop, the main and longest living workshop in macroeconomics at the department, and it is still run much in that way. His philosophy, research methodology and paradigms in macroeconomics guide not only the key first-year sequence in macroeconomics for PhD students, called 'Theory of Income', but also much of the way macroeconomics is taught at the undergraduate level. His emphasis on a scientific approach to economics, laying out model assumptions first and to show on those grounds the emerging conclusions and their connections with clear patterns in the data, remains how questions of macroeconomics are analyzed and debated within the faculty. Section 2 describes this influence from a personal vantage point.

Without any aspiration or hope for a systematic overview, in what follows I highlight some key fields where Lucas has fundamentally shaped economic science.[1] Starting with his 1972 neutrality paper in Section 3, I summarize how Lucas revolutionized theoretical and applied macroeconomics, how he unified these two, and how the neutrality paper lives on in today's macroeconomic research. Section 4 explains how and why Lucas' interest in growth theory, a field where he has exerted long-lasting influence, developed over the years. The Lucas asset pricing equation is the topic of Section 5. Section 6 is about the Bellman equation, which became a standard tool for today's microfounded macroeconomics due to Lucas' influence. I describe it in a simple example and illustrate its role in teaching macroeconomics at the University of Chicago. Section 7 thus returns to the Chicago-specific vantage point of section 2 and provides an insider look into the atmosphere of the Money and Banking Workshop and how it was considerably shaped by Lucas' influence. That section is more personal in its tone of voice and hopefully offers an intriguing complement. Section 8 provides some brief concluding remarks. I shall stop here for no particular reason. One can accuse me of leaving out much, especially of Lucas' later key contributions. It surely would have been desirable to say more about Lucas and Moll (2014), to name but one example. I plead guilty as charged and just hope that the missing parts and aspects are eloquently described elsewhere.

2. Lucas and the Chicago school of economics: A personal vantage point

I came to Chicago in 2007. For me, the economics department of Chicago has always been the first among all departments, the intellectual center of the profession, the inner sanctum, the mountaintop, the place where one most wanted to be as a researcher in economics let alone in macroeconomics. An appointment to that department counts among the greatest honors of our profession. There was no doubt in my mind that I had to come when it was offered.

My deep bond to this department and to Lucas' school of macroeconomics should come as no surprise. My 1990 PhD is from the University of Minnesota. The economics department there then and perhaps still now offered a purified and more orthodox version of the approach at the University of Chicago, and the Lucas rational expectations approach to macroeconomics received a particular emphasis. At the time and probably still now Lucas had an out-sized influence on all graduate students in macroeconomics at the University of Minnesota. We were taught an early draft of Stokey and Lucas with Prescott (1989): it was the essential text in the first year, it was our bible. I learned a huge amount by studying his paper on asset pricing (Lucas, 1978) in great detail. I remember sending him a hand-written letter regarding

[1] Fischer (1996) provides a detailed and comprehensive account.

some aspect in that paper (and I wish I remember what it was). That paper and its approach were essential to my education. Lucas was a towering figure and the voice of singular importance in macroeconomics. I am proud to have had Chris Sims as my advisor for my PhD thesis. I admire Chris Sims deeply. He is my intellectual father. I owe him hugely and first of all. Describing his towering influence on me and the profession deserves its own special issue, but this is not the place for it. Let me merely point out that his approach to some key issues in economics disagrees sharply with Lucas. The inherent tension between these two perspectives has had and continues to have a considerable and productive influence on my research career (and not just only on mine, obviously). It allowed me to put much in perspective and increase the variety of angles from which an issue can be understood—for that I will be forever grateful. There is no contradiction then to state that Lucas' contributions continued to have a deeply powerful influence on me.

I believe I saw Lucas for the first time at a conference at the Federal Reserve Bank of Minneapolis in the late 1980s, while I was a graduate student at the University of Minnesota as well as a research assistant for Chris Sims at the bank. The bank then was a powerhouse for Minnesota-style macroeconomic research, and thus was considerably influenced by Lucas' work. I believe I met and spoke to Lucas first when I was on the job market in the winter of 1990 and was given the opportunity to give a talk at the University of Chicago. I remember mostly that I was a nervous wreck during that visit, and the visit did not lead to an appointment. I certainly consider myself fortunate to then land an appointment at Princeton University instead. The environment there provided a rather different perspective on economics, I learned a lot there and I am very grateful for that experience: it almost was like a second PhD training. As I moved from Princeton University, USA, to Tilburg University, the Netherlands, and Humboldt Universität in Berlin, Germany, and as I met and interacted with many colleagues over the years, it became pretty obvious that there was a distinct and Lucas-led Chicago school of macroeconomics.

The research done by Lucas and the leading scientists of that school such as Thomas J. Sargent, Neil Wallace and Ed Prescott and the principles and approaches advocated for by that group provided the guiding light for a substantial portion of macroeconomic thinking. Macroeconomics had to be built on the foundations of general equilibrium theory and generate clear quantitative implications. Policy discussions should be done on the foundation of a fully specified model. Deep and stable parameters describing preferences and technologies were key to make them compelling. One should assume that agents have rational expectations and solve optimization problems. Everything had to have proper micro-foundations. Sticky prices and sticky wages were frowned upon then as being against that approach. Lucas clearly put forward the idea that economic facts are to be understood in terms of individual decisions. If a researcher tried to argue that something seemed to be in conflict with individual optimization and market clearing, then perhaps she or he had not tried hard enough to think it through and to get to the bottom of it all. Rather and with more work, one often can ground the observations on rational choice.[2] Likewise, if one was to assume that

[2] In his own words, 'there is no question that social convention and institutional structures affect […] [observable behavioral] patterns, but conventions and institutions do not simply come out of the blue, arbitrarily imposing themselves on individual agents. On the contrary, institutions and customs are designed precisely in order to aid in matching preferences and opportunities satisfactorily. Taking into account theoretically […] the complicated arrangements we observe in actual labor and product markets would not be a step toward constructing an alternative model to the one Rapping and I used [in (Lucas & Rapping, 1969)], but toward an extension or elaboration' (Lucas, 1981, p. 4). To show how overwhelming this idea is, it is instructive to mention two examples. First, having abandoned the notion of social institutions as emerging independently of the individual, new institutional economics today shows great interest in portraying social institutions as the outcomes of the rational and deliberate considerations of the members of societies (Levinthal, 1988; Rutherford,

governments could do things that agents cannot, one better had to provide an excellent rationale. He was serious about exiling assumptions as unfounded if there were no explicit connections with individual behavior. That approach was and still is followed by many in the profession and other departments including, say, the University Minnesota, Northwestern University, Carnegie Mellon or the University of Pennsylvania. But it was clear that Chicago was its center, and it was clear that Lucas was its leader.

Coming to Chicago in 2007, it then was all the more remarkable and almost amusing to me how my colleagues emphatically denied that there is such a thing as the 'Chicago School of Economics'. I can see why they insist that there isn't. Yet, I argue, that there is. Of course, the label usually refers to the free market and price-theoretic philosophy epitomized by Milton Friedman. My focus in this paper is on the more specific albeit related school of the rational expectations approach to macroeconomics led by Robert E. Lucas. Needless to say, empirical labor economics led by Jim Heckman, price theory led by Gary Becker, generalized method of moments econometrics led by Lars Hansen or the efficient financial markets approach led by Eugene Fama, say, are still important schools of thought at the University of Chicago for other fields. Yet, all of these still have an overarching, common philosophy in their stringent pursuit of a scientific approach, the piercing scrutiny of any arguments, their emphasis on structural modeling and deep parameters, their respect for and search of empirical evidence, their foundation on optimizing behavior and the completeness of markets, and their skepticism towards government interventions.[3] No, we do not meet in the morning and swear on some bible of economic wisdom. Sure, there is lots of scientific disagreement among the faculty, and a great variety of vantage points. But yes, there was and still is a Chicago School of Economics. Robert E. Lucas was very much at its center, and still is.

3. The rational expectations revolution

Robert E. Lucas epitomized the rational expectations revolution in macroeconomics. Of course, he did not do it alone. He built on the concept of rational expectations as developed by Muth (1961). Fellow scientists such as Thomas J. Sargent, Neil Wallace, Edward C. Prescott, Robert Hall, John Taylor and Robert Barro were key players, helping to advance this perspective as part of macroeconomic theory. Sims (1980) provided the empirical counterpart, arguing that expectations of future variables enter as functions of all present and past data. The collection edited by Stanley Fischer (1980) gives a great overview of the main characters and the variety of directions towards which they extended the core idea of rational expectations first applied in a macro-context. But there is no doubt that Lucas was the leader of this movement.

The assumption of rational expectations posits that models need to specify the random processes by which variables are generated. When agents need to form expectations about them, the assumption stipulates that agents should be assumed to use expectations as defined by the mathematics for random variables. The rational expectations revolution tossed the key

1994). And second, New Keynesians' staggered wage and price setting literature (Fischer, 1977; Phelps & Taylor, 1977; Taylor, 1979; 1980; Ball & Romer, 1987) (Ball & Cecchetti, 1987; Mankiw, 1985) came out as a response to Lucas' microfoundational program to show that the absence of instantaneous market clearing and wholly flexible prices may be the outcomes and not the failure of individual rational optimization (Ball, Mankiw, & Romer, 1988, pp. 1-2).

[3] In many private conversations, I often find it remarkable how eagerly people wish the government to impose certain choices on others but would steadfastly refuse others to have the government impose choices on themselves. There are exceptions, of course. Traffic rules are a good example for mutually acceptable restrictions on choices. I only wish that people would more generally only seek to impose on others, what others should then feel free to impose on them in turn, and which are not tilted towards the preference of the person in question. Not everyone likes broccoli, but some do. Personal tastes and preferences are worth respecting.

tenets of the then reigning Keynesian paradigm overboard, though a subsequent literature then carefully reconstructed much of them (Mankiw & Romer, 1991a; 1991b) respecting and building on the rational expectations paradigm as spearheaded by Lucas.

Lucas started using rational expectations as early as in 1965 (Fischer, 1996). But perhaps Lucas (1972) is the most celebrated early paper with a full-edged form of rational expectations macroeconomics, pointing out the neutrality of money arising out of a then standard framework, once rational expectations are imposed. The paper emerged in the context of the still ongoing controversy over the Phillips curve, a purported trade-off between inflation and unemployment. Lucas (1972) put a stake through the then-popular argument that the Phillips curve is a stable relationship that can be exploited by policy to keep unemployment low per engineering higher inflation. He argued that the public will then end up rationally expecting higher inflation, and that unemployment will just keep fluctuating around its natural rate. Rather than pursuing this futile agenda, monetary policy should focus on keeping inflation low and stable. This is not the place to discuss the immense literature investigating it and building on it. Here suffice it to say that a simple scatter plot of US postwar data on inflation versus unemployment produces a Phillips cloud with practically zero correlation between these two variables. Benchmark New Keynesian models imply that different shocks drive inflation as opposed to unemployment (Fratto & Uhlig, 2020). Much of the current macroeconomic literature build on a New Keynesian framework in which a New Keynesian Phillips curve is absolutely central (Galí, 2015) as a short-to-medium-run trade-off, and acknowledge the insight by Lucas (1972). Some have interpreted the lack of correlation as simply reflecting a remarkably successful monetary policy (McLeay & Tenreyro, 2019) rather than a lack of a systematic relationship.

As a macroeconomist, one can have one of at least three perspectives on this. The first is to simply do something different entirely, thus avoiding having to take a stand on the issue and perhaps finding something potentially more interesting to do in any case. The second is to argue that the Phillips curve absolutely is ‘out there’ in the data and thus deserves to be a key component of any debate on macroeconomics and monetary policy. One should just look hard enough for it and build models that appropriately suit the data. The third is to shake the head in some disbelief and observe that the profession is more content building models with ever more epicycles to keep a beloved paradigm alive rather than discard it and look at it all with fresh eyes. In the latter case, one is, of course, welcome to try. I shall not adjudicate here which perspective is the correct one: allow me to point to Uhlig (2005) and Uhlig (2012) instead. Let us just say that the Phillips curve took a licking and keeps on ticking.

And a licking it took! It might be good to recall the sequence of events in a bit more detail. Back in the ‘Old Keynesian’ days the Phillips curve was thought to be a stable relationship which could be exploited systematically by, say, monetary policy. Wages were nominally fixed at some level. Thus, when the central bank eases on its monetary policy and creates some inflation in the product market, the nominal wages look cheap to employers and they hire more workers, reducing unemployment. The Keynesian policies following these ideas and enacted in the late sixties and early seventies proved to be a disaster. Inflation supposedly starting from the supply side rose and unemployment, if anything, got worse. The resulting stagflation (i.e., the combination of high unemployment and high inflation in the 1970s) was difficult if not impossible to interpret in the Keynesian framework. A growing number of professionals gave up the cherished trade-off (Diebold, 1998, p. 178).

Friedman (1968) and Phelps (1967; 1968) fronted up in the theoretical and applied policy scrums to famously attack the Keynesian line of thinking, but it was Lucas (1972) who ultimately destroyed it with his seminal contribution on the short-run non-neutrality and long-run neutrality of money. This story of the short-run real effects of monetary changes was a

riddle that Lucas, unlike David Hume, wanted to solve without suspending the rationality of market participants for the short run. Rationality is so overwhelming an idea and drive for behavior, he argued (Lucas, 1996), that it seemed more tenable to understand any apparently contradicting phenomena not as effects of its absence but as disturbances to its smooth working. In the neutrality paper Lucas (1972) placed information deficiencies regarding local and global price dynamics into the role of a mechanism that could drive a wedge between fully informed and confused forms of rational decisions. By so doing Lucas showed how unexpected nominal shocks result in an inflation–unemployment trade-off that cannot be exploited by anticipated policy changes. Lucas brilliantly maintained the idea of a short-run Phillips curve while ruling out the option of any activist countercyclical economic policy built on the trade-off. The short-run Phillips curve emerging from a correlation between aggregate-level price shocks and the resulting real adjustments starts fading away the moment economic policy feels tempted to apply it for systematic policy purposes.

This perspective led to the firm establishment of the Lucasian version of an expectations-augmented Phillips curve[4] as a key tool for much of macroeconomic research at the time. This scientific analysis together with the powerful advocacy by Friedman (1962) for monetary policy rules led to the key insight by Kydland and Prescott (1977) that monetary policy is better off when sticking to rules rather than exploiting such trade-offs in a discretionary manner. With that, it led to the formulation of the now ubiquitously invoked Taylor rule for conducting monetary policy, originally suggested by Taylor (1993) as a way to describe rather than prescribe monetary policy practice and becoming a prescription for monetary policy in much current macroeconomic research.

These intellectual developments, in turn, largely contributed to the modern framing of central bank mandates to focus on inflation first and foremost, notably enshrined in the Maastricht treaty of the European Monetary Union. Lucas thus deserves a large part of credit for having given rise to the intellectual framework for the phenomenally successful monetary policies from, say, 1980 to 2020, reigning in the specter of inflation which has haunted many economies before that. Indeed, these monetary policies have been so successful that we may have forgotten as of late how painful it was to get there, how they worked in the first place and why there were much needed. Nowadays, there are all kinds of calls upon monetary policy and increasingly often sympathetically emphasized by central bankers themselves to solve problems related to financial stability, inequality, climate change and other objectives deemed more noble than worrying about inflation, dangerously ignoring the warning of Friedman (1962) that monetary policy rules may be 'the only feasible device currently available for converting monetary policy into a pillar of a free society rather than a threat to its foundations.' Moreover, in the recent decade, the risks of deflation rather than inflation weighed more heavily on the minds of many policy makers and in many countries beyond Japan. There is a risk that the lessons of the 1980s need to be painfully relearned (Sargent, 1996). 'Those who forget their history are condemned to repeat it,'[5] and perhaps we will indeed. At that point, we may repeat another cycle of recalling the important insight of Lucas' work and his followers. His impact may be cyclical, but in one way or another it will be lasting.

There was thus more to the neutrality paper than the re-establishment of Friedman's famous *k*-percent rule. Even if on the surface the paper seemed to be the slam-dunk case of reformulating Friedman's point for a shock-free monetary policy (Lucas, 1981), the notions of

[4] As opposed to its Friedmanian version (Friedman, 1968; 1977) where the short-run trade-off did not disappear but moved upwards relating higher and higher levels of inflation to the same rates of unemployment.
[5] This quote is often attributed to Santayana (1905), though the exact quote there is somewhat different.

a Phillips curve falling apart and agents adjusting their behavior to economic policy changes directly affected the then conventional macroeconometric practice. Lucas assembled his related ideas in the paper commonly referred to as the famous Lucas critique (Lucas, 1976). As he argued, one should not estimate relationships in the data and then suddenly seek to exploit them with policy. Such relationships could then shift, as did the Phillips curve. Stable or deep parameters were needed instead.

This was often summarized and actually misunderstood as stating that one cannot calculate the impact of policy changes by examining such relationships in aggregate data alone. The counterargument to that latter interpretation has been provided by Christopher A. Sims (1980). In order to estimate the impact of a policy change such changes need to have occurred in the past. With sufficient data and compelling identification of policy changes such estimation is entirely feasible. That line of thinking led to a large and still evolving literature on vector autoregressions and their use in policy evaluations, and sometimes evoked heated debates on whether regressions are useful and what it means to change policy.

At one extreme, some argue that the term 'policy' should refer to the entire stochastic process governing the policy variables. Rational expectations macroeconomics mandates that agents can form expectations and know the entire stochastic process of the relevant variables: 'policy' must go beyond simply setting policy variables here and now. A 'policy change' means to change that entire stochastic process. From that extreme perspective a policy change has never occurred and thus its impact cannot be estimated for lack of data, yet it can be considered in a fully specified model with deep parameters. The inherent contradiction in this line of thinking is that the *a priori* of having agents being aware of the entire process renders such a policy change now impossible or as an uninteresting zero probability event. Moreover, one ought to be worried as a researcher that perhaps even the deep parameters are not all that deep and subject to change should such a policy change occur nonetheless, rendering the entire exercise futile.

At the other extreme, policy changes are the random occurrences and surprises inherent in the stochastic description of the policy process itself. They happen all the time and are thus amenable to an econometric analysis of cause-and-effect using e.g. vector autoregressions. One can then use these estimation results to guide policy deliberations, effectively helping the policy makers in choosing the most desirable stochastic surprise in a given situation. The inherent contradiction in this line of thinking is that one then has to wonder about the fundamental nature of the randomness and why one could not simply replace the policy making decision body with a random generator—after all, the detailed deliberations should generate the original stochastic process in the end, or one runs into the Lucas critique once again.

The tension between these perspectives has never been fully resolved: it has remained a paradox.[6] I remember thinking hard about it in graduate school, using a sequence of coin tosses as a stripped-down environment to examine the issue, but without reaching a satisfying conclusion at the time. It remains a vexing issue.

[6] This paradox may not be confined to economics. With the development of quantum theory and thus the random determinants of everything in nature, physicists, psychologists and philosophers have grappled with the vexing notion of a 'free will' and its relationship to the laws of physics. Should a murderer perhaps go free, because one cannot really fault him for some unfortunate sequence of random quantum fluctuation stochastically leading to the outcome of a horrible crime? Clearly not, but a logically consistent argument is tricky to arrive at from a physics perspective. Planck (1923) provides an early and prominent take on the issue, Conway and Kochen (2009) provide a more recent one and the most humorous take is the poem 'Ein Wirkungsquant fliegt durch das Dorf' by B. Hassenstein. An economist would instead think about incentives, and predict resulting behavior.

For now, one ought to accept that both perspectives are important, despite their inherent tension and paradoxical contradiction. C'est la vie. Unfortunately, that tension has led some macroeconomic theorists to disregard the useful role of macroeconometrics and policy estimation, while some macro empiricists have dismissed the Lucas critique too easily. As evidence of the debate then, check the critique of vector autoregression in the conclusions of the landmark paper by Lucas & Stokey (1987) which comes as an add-on to the beautiful theoretical framework laid out in the body of the paper and which may or may not be consistent with Sims (1980). Note also the development and endorsement of calibration over estimation in the seminal contribution by Kydland and Prescott (1982). In retrospect, some of these debates of calibration versus estimation were perhaps led with too much religious fervor on both sides. Good applied macroeconomic theory benefits from careful quantification, and thoughtful econometrics can surely help, possibly guided by a priori views on what is reasonable. The successful developments of Bayesian estimation techniques for dynamic stochastic general equilibrium (DSGE) models are one of the success stories emerging from these debates, establishing the benchmark models by Smets and Wouters (2003; 2007) and more. At the same time, it must be kept in mind that no technique should ever be followed blindly or treated as an effective remedy to all kinds of theoretical or empirical problems. One can both appreciate these developments and applaud their successes while remaining skeptical about some of the purported insights.

The neutrality paper thus exemplifies a theoretical research line turning out to have far-reaching implications for applied economics. Certainly, there is no doubt that Lucas has always treated data with considerable respect and giving it more importance in his work than some of his purely theoretical papers might suggest. As one example, one may wish to examine his thoughtful and careful treatment of money demand in this Nobel lecture (Lucas, 1996). The same can be said about Ed Prescott, often thought of as one of the fiercest critics of using regression analysis. Indeed, the calibration approach of Kydland and Prescott (1982) can be given a generalized methods of moments interpretation as developed by Hansen (1982) and shown to apply here by Christiano and Eichenbaum (1992). We may ultimately be simply discussing degrees of involving the full apparatus of econometrics rather than a stark dichotomy.

These debates nonetheless persist. Some macroeconomists have resolved to stick to a largely calibration-oriented or purely theoretical approach. Others have followed the rather nonstructural estimation approach postulated by Sims (1980) and developed it into a considerable and remarkable apparatus. Finally, some macroeconomists have rather undertook to tease out relationships from microeconomic data and natural experiments, foregoing both camps altogether. Be that as it may, this state of macroeconomics is Lucas' legacy as well, at least to some degree, and it will surely inspire many fruitful debates in the years to come.

4. Resurrecting growth theory

While Lucas is well-known for his achievements in business-cycle research, he should perhaps equally be celebrated for his contributions to growth theory. What we today regard as the accumulation of human capital theory of growth that has sprung from Lucas' (1988) research and has some roots in Chicago (Uzawa, 1965). Understanding the driving forces of long-run growth is obviously a topic that can best be understood at the same microlevel as served as the basis for Lucas' business-cycle theory. Solow (1956; 1957) pointed to a residual as the key component of economic growth. It was interpreted as measuring 'technological progress', given the lack of any other viable alternative. Soon it also became obvious that decisions on capital investment cannot be separated from innovations (Solow, 1959; Lucas, 1967), but relating technical progress to capital accumulation still left too much unexploited

information in the Solow residual. At the University of Chicago Theodore Schultz (1961) and Zvi Griliches (1963) were amongst the few at that time who first pointed out that the size of the unexplained part of economic growth could be substantially reduced by proper measurement that took into account how human and other forms of investment augmented traditionally interpreted and measured inputs. This line of research is a nice example for a fruitful interaction between Lucas and other Chicagoans.

The literature on economic growth and its determinants lay dormant for a while, until it was resurrected in the mid-1980s with the seminal, initial contributions by Paul M. Romer (1986a; 1987) and Lucas (1987; 1988; 1990) himself. Lucas (1987; 2003) argued that the welfare costs of business cycles are negligible compared to the benefits of increasing long-run growth even by a small amount. Romer (1986a) argued that aggregate constant returns to capital were key to understanding the engine of economic growth. It is hard to overstate the influence of these papers at the time. I remember listening to the job market presentation by Paul Romer when I was a graduate student at the University of Minnesota. After that presentation and with that Lucas calculation, many of us sought to re-orient their macroeconomic research towards endogenous growth theory, and quite a number did it successfully so. The question as to what determines economic growth and, especially, how to increase growth rates, perhaps even a little bit, clearly was first-order and of major importance, and it still is. It was an exciting time for macroeconomics as we all eagerly awaited the latest rounds of insights and research progress on these questions, even as we were working on other topics. Many papers have been published, many careers established, many books have been written as a result. For a few scattered examples, see Barro and Sala-i-Martin's (2004) or Aghion and Durlauf's (2005) enormous volumes.

Ultimately, that literature never quite succeeded in the way we had hoped back then. There was no magic formula of how to make higher growth come about. Growth regressions by Barro (1999) and others held some answers. Education was important, though the political environment much less than one might have thought (and perhaps hoped). Some countries seemed to be aligned with others in convergence clubs (Quah, 1996; 1997). But research gradually re-oriented itself back more to the question of the overall level of economic development rather than the long-term rate of economic growth rate *per se*. It was clear that things can be messed up pretty badly. Much of the differences between countries were then typically captured by some aggregate productivity level, and this is thought to relate to the available level of technologies and to answer Lucas' (1990) question about why capital does not flow from rich to poor countries. It is fair to say that this is still very much the benchmark and baseline view (Jones, 2005; 2016; Acemoglu, 2009). But with many technologies available globally and globally operating firms seeking the least costly places to produce this cannot be the entire story. Researchers such as Gregory Clark (1987) or Bloom and Van Reenen (2007) have thus tried their hands at understanding why the same technologies are operated at such different degrees of efficiencies when examining, say, India versus the UK or the USA, and what, if anything, can be done about it. Even there, it seems, we have not quite gotten to the bottom of it all, but it certainly seems worth pushing farther in that direction.

The importance of these cross-country differences remains as important as they ever were since that seminal publication by Lucas (1990). We now have a deeper understanding as to what to examine when thinking about these questions. It is fair to say that we still do not have the answer. Much, still, can and should be done. Be that as it may, it surely is also fair to state that Lucas substantially advanced our knowledge of the topic and urged us to get this far where we are now.

5. Asset pricing

The Chicago Booth School of Business is the business school at the University of Chicago. There are many close contacts between the faculty there and the faculty at the economics department, encompassing joint workshops, graduate student supervision, co-authorships and more. A substantial portion of the faculty there but also at the department itself devotes the bulk of its research to the topic of asset pricing. Active research there has largely moved into the empirical realm. As far as both theory and empirics is concerned, there is quite a list of giants, often from the University of Chicago, on whose shoulders these researchers stand and on whose seminal contributions they are building.

Regarding that foundation, it seems only fair to point out the key role of the 'Lucas asset pricing equation' arising out of Lucas (1978) as a central, organizing framework. In mathematical terms, it is stated as

$$1 = E_t[\mathcal{M}_{t+1} R_{t+1}], \qquad (1)$$

where $\mathcal{M}_{t+1}$ denotes the stochastic discount factor between period t and $t+1$ and where R_{t+1} denotes the return on an asset from *t* to $t+1$. Both should be stated either in real terms or in nominal terms.

One may wish to refer to this result as the Breeden – Lucas – Rubinstein asset pricing equation or even consider it as an application of the calculus with Arrow securities (Arrow, 1964) and general equilibrium reasoning as by Debreu (1959). Nonetheless, that key equation is most often simply attributed to Lucas. In many ways, equation (1) encapsulates the very essence of the rational expectations revolution for which Lucas is rightly famous. It gets to a question of considerable practical interest: when should an investor buy stocks and when should he or she sell them? After aggregating these decisions on a macroeconomic level, one arrives at the determinants of investment and entrepreneurship and thus business cycles and economic growth. Keynes (1936) famously and perhaps derisively attributed these determinants to 'animal spirits'. From his perspective, investors for some unbeknownst reason suddenly feel the desire to buy and invest and sometimes they inexplicably cool off to that idea entirely. This taking on the matter is still remarkably popular in newspaper reasoning about stock markets and investor behavior. Many surely can recall the cartoon where a whisper campaign with misunderstood words leads to buying and selling frenzies. Politicians and journalists sometimes reason that economics is 50 percent psychology: if only political speech can improve the animal spirits of those fickle investors out there, the economy will get humming again.

This animal spirit perspective is superficially appealing but falls apart upon closer inspection. Dear reader, ask yourself: have you ever met an investor who would describe himself as following such animal spirits? Would corporate boards happily cheer on their CEOs if animal spirits were her rationale for pursuing some particular investment strategy? Do you follow animal spirits, or do you try to manage your hard-earned money as well as you can? If you still insist that animal spirits determine what you do, then would you want some more clear-minded advisor stopping you from taking your own decisions, and if not, why not? There may be evidence that people do not spend much time on some of the most important decisions in their lives such as buying a house or making choices regarding retirement fund investments, but that does not mean that they do not have good reasons for proceeding. Finally, even purchases on impulse may simply be a reflection of underlying preferences and choices.

Thus, the Lucas asset pricing equation (1) throws cold water on that animal-spirits perspective. Rather than buying and selling due to some random psychological impulse, the equation can best be understood as stating that investors think hard about whether the asset is

worth buying, weighing the pros and cons and taking into account as much as they can reasonably know about their future performance. They form rational expectations about the future, rather than being overly optimistic or pessimistic. When investors role a fair dice, they understand that each side might come with equal probability, rather than fooling themselves that it will always be a six or always be a one.

Equation (1) implies that investors do not mind risky assets *per se*. Rather, what matters is whether high pay-offs happen in times when they are particularly welcomed or not. This is encapsulated by the stochastic discount factor, and a complete specification in a given context will then give rise to risk premia or the negative thereof, insurance premia.

Equation (1) does not imply that mistakes may not be made on occasions or that people will not regret their choice ex post. Perhaps, it was alright to invest in some asset at date t, but then the asset does not pay off in $t + 1$. Why would an investor not regret ex post of having taken that choice? I certainly have done so. But then it is good to remember that I did not know better at the time when I took that decision.

As Cochrane (2005) recapitulates, equation (1) together with $\mathcal{M}_{t+1} \geq 0$ can be shown to be equivalent to the statement that there is no strict arbitrage, i.e. that it is impossible to invest in assets in such a way as to pay nothing today and make money for sure tomorrow. Rather, rewards and risks go hand in hand, and investors better carefully weigh the trade-offs.

Equation (1) can be understood as the basis on which much of the field of finance builds, both theoretically and empirically. Cochrane (2005) offers a terrific source on that perspective, summarizing key strands of the finance literature. That book is a key source for young researchers to learn about asset pricing. The material described therein is the basis of much of asset pricing research. At the risk of attributing too much to Lucas, one could thus argue that a substantial portion of finance research at the University of Chicago is the result of thinking about an equation which he created and popularized. Given the central importance of that equation, of course, the profession has become creative and developed alternatives, incorporating animal spirits after all as well behavioral motives, deviations from rational expectations, learning, loss aversion and the like. This is not the place to review it all. Suffice it to say that equation (1) remains the benchmark against which everything else is compared, even if increasingly important developments happen without it. I view it as a mistake to discard too readily the possibility that agents act in their own best interest. Indeed, I have gotten to know only very few people who would claim such a lack of self-interest about themselves other than perhaps emphasizing some degree of ‘selfless’ altruism and their contributions to their family and community (and they usually surely do not wish to be prevented from that, either). In sum and upon closer inspection, people may be far more rational than casual discussions and sloppy newspaper reports would suggest.

Finally, equation (1) is present in any rational expectations macroeconomic model as it arises in practically any intertemporal decision problem. It is only when multiple periods need to be taken into account that the question of expectation formation arises at all. Lucas advises us to formulate these expectations as rational. One can think of the stochastic discount factor $\mathcal{M}_{t+1}$ and the mathematical expectation as all the psychology required to be thinking about that intertemporal problem. As a pointy summary, subject to that caveat and subject to acknowledging the literature developments of alternatives described above, equation (1) teaches us that the statement that ‘economics is 50 percent psychology’ is 100 percent rubbish.

6. Bellmanize!

Much of Lucas' work emphasizes and employs the tools of dynamic programming, drawing on the Bellman equations. The equation is named after Richard Bellman (1953; 1957) and is part of his dynamic programming approach to analyze intertemporal optimization problems. It has found applications in numerous fields, but it has become of particular importance in economics, starting with a contribution by Martin Beckmann and Richard Muth (1954). But it took a while for these methods to take hold and become part and parcel of macroeconomics. Indeed, Lucas (1978) may have ultimately been as influential in establishing this tool and analytical apparatus for building and examining macroeconomic models as with the resulting asset pricing equation (1) itself. The methodology together with an amazingly useful collection of fixed-point theorems and other mathematical tools have been collected in the classic book by Stokey and Lucas with Prescott (1989). It is still considered the current bible on these issues by many who continue to use these tools.

The Bellman equation approach may perhaps be best described by example, using a version of the 'cake eating' problem in Gale (1967), and which is a simple version of the consumption-savings problem first analyzed using dynamic programming in Beckmann (1959). There is no new contribution here: it just serves as a simple and well-known introduction to the methodology.

Let's say, an agent has a cake of size $k \geq 0$ and enjoys eating cake. Eating all of it at once may be a bit much, plus it will be nice to have some cake left to eat tomorrow and the day after tomorrow. So, let us say that the function $u(c_t)$ captures the joy felt by an agent of consuming the amount of cake $c_t \geq 0$ at date *t*. More cake is better, perhaps up to a point—so let us assume that $u(\cdot)$ is strictly increasing. An additional little piece of cake is a lot more appreciated if the agent did not have a lot of it yet. So, $u(\cdot)$ should be concave and it might as well be twice continuously differentiable. All else equal, the agent would rather consume cake today than wait until tomorrow: there is thus discounting of the future.[7]

If the agent consumes the cake in the course of two days $t = 0$ and $t = 1$, say, the problem would be

$$V(k) = \max_{c_0 \geq 0, c_1 \geq 0} u(c_0) + \beta u(c_1)$$
$$\text{s.t. } c_0 + c_1 \leq k$$

and one can do one of several approaches to solve this, e.g. using a Lagrangian approach or substituting out one of the variables. Over four days, the problem would have to be stated as

$$V(k) = \max_{c_0 \geq 0, c_1 \geq 0, c_2 \geq 0, c_3 \geq 0} u(c_0) + \beta u(c_1) + \beta^2 u(c_2) + \beta^3 u(c_3)$$
$$\text{s.t. } c_0 + c_1 + c_2 + c_3 \leq k$$

and that becomes increasingly cumbersome. There is a better approach. First, think about the cake k_0 left over[8] at the end of the day after eating the piece c_0 on date $t = 0$,

$$k_0 = k - c_0.$$

This residual amount of cake is the amount of cake available for date $t = 1$. We get a very similar looking equation there,

[7] This is an essential difference to Gale (1967), see Romer (1986b).

[8] There are sometimes passionate debates whether that amount should have the subscript $t = 0$ for the date at which it is determined, or $t = 1$ for the date at which it can be used next. I lean towards the first for reasons of compatibility with measurability notation. Others lean towards the second, ultimately motivated by the corresponding continuous-time approach. Either way works with appropriate care.

$$k_1 = k_0 - c_1,$$

and likewise for future periods. Second and perhaps remarkably, it is easier to think about infinitely many periods rather than four or five. Therefore, write the problem as

$$V(k) = \max_{(c_t \geq 0)_{t=0}^{\infty}} \sum_{t=0}^{\infty} \beta^t u(c_t) \quad \text{s.t. } c_t + k_t = k_{t-1} \tag{2}$$

where $k_{-1} = k$. Third, notice that everything from $t = 1$ on forward looks remarkably like everything from $t = 0$ on forward. For example,

$$\sum_{t=0}^{\infty} \beta^t u(c_t) = u(c_0) + \beta \sum_{s=0}^{\infty} \beta^s u(c_{s+1})$$

We can therefore reduce the daunting-looking decision problem of equation (2) to something much more digestible. Suppose that we already somehow managed to solve that decision problem from date $t = 1$ on forward, given whatever cake k_0 was left. We then merely have to ask, how to choose k_0 and c_0 in an optimal way,

$$V(k) = \max_{c_0} u(c_0) + \beta \max_{(c_{s+1} \geq 0)_{s=0}^{\infty}} \sum_{s=0}^{\infty} \beta^s u(c_{s+1}) \quad \text{s.t. } c_0 + k_0 = k \text{ and } c_s + k_s = k_{s-1}$$

This formulation incorporates the principle of optimality that the decisions for all future periods from $t = 1$ on forward are optimal, given the state or cake k_0 left over resulting from the first consumption decision. Thus, rewrite this more conveniently as a dynamic programming problem or Bellman equation,

$$V(k) = \max_{c,k'} \{u(c) + \beta V(k') | c + k' = k\} \tag{3}$$

A lot can be said about this equation. Lucas (1978) and Stokey and Lucas with Prescott (1989) provide excellent guidance. For example, and rather unsurprisingly, the first-order conditions are

$$\frac{\partial}{\partial c} : u'(c) = \lambda \tag{4}$$

$$\frac{\partial}{\partial k'} : \beta V'(k') = \lambda \tag{5}$$

where λ is the Lagrange multiplier for the constraint $c + k' = k$.

But now, what should one do about the derivative $V'(k')$ of the value function? For that, we can take the derivative of $V(k)$per its definition in equation (3). In order to calculate that derivative, the following mind experiment is useful. Suppose the decisionmaker had some tiny extra amount of cake. What should he do with it? Consume it all? Save it all for tomorrow? Split it up somehow? It turns out that it does not matter. The decision maker is already assumed to have optimized the split between c and $k'$for the given amount k: he is just indifferent between consuming that extra amount or saving that extra amount. Taking the route of assuming that he consumes the extra amount gives us the envelope condition

$$V'(k) = u'\big(c(k)\big), \tag{6}$$

where $c(k)$ is the decision rule of how much of the given cake k to eat right now. Combining the two first-order conditions with the envelope condition, and using the original notation invoking subscripts of time yields the consumption Euler equation

$$1 = \beta \frac{u'(c_{t+1})}{u'(c_t)}. \tag{7}$$

This equation is actually rather specific. Note that a piece of cake left over today will be a piece of cake tomorrow, i.e. the real return on saving cake is equal to unity. One can slightly generalize the problem and allow the cake to randomly shrink or even increase from one period to the next, generating the real return R_{t+1}. The same logic above together with taking expectations, given all currently available information yields

$$1 = E_t[\mathcal{M}_{t+1} R_{t+1}], \tag{8}$$

where

$$\mathcal{M}_{t+1} = \beta \frac{u'(c_{t+1})}{u'(c_t)}. \tag{9}$$

In other words, we obtain equation (1) above, stated in real terms. Indeed, that was much the point of Lucas (1978).

As a last remark on the original cake eating problem above, i.e., when the return is equal to unity as in equation (7), suppose that the utility function is of the constant relative risk aversion variety

$$u(c) = \frac{c^{1-\eta} - 1}{1 - \eta} \tag{10}$$

for some constant $\eta > 0$. Conjecture then that the consumption decision rule takes the form

$$c(k) = (1 - \alpha)k \tag{11}$$

so that the amount $k'(k) = \alpha k$ will be left over for tomorrow. Equation (7) then delivers that

$$\alpha = \beta^{1/\eta}. \tag{12}$$

It should be noted, though, that equation (11) is not the only decision rule satisfying the first-order conditions (4), (5) and envelope condition (6). Indeed, and with α as in equation (12), a similar rule,

$$k'(k) = \alpha(k - \kappa) + \kappa$$
$$c(k) = (1 - \alpha)(1 - \kappa),$$

which leaves some cake $0 < \kappa < k_{-1}$ forever also does the trick. It is intuitively clear, though, that only the decision rule with $\kappa = 0$ and where all cake is eaten eventually is optimal, and one can show that formally by examining the Bellman equation (3). What one learns here is that the first-order conditions and envelope condition are necessary, but not sufficient for a solution to the dynamic programming problem.

There is much more to be said. For all that and more, I strongly advise the reader to meticulously study Stokey and Lucas with Prescott (1989). Suffice it to say here that this approach of thinking about intertemporal decision problems as dynamic programming problem is now part and parcel to the way macroeconomics is taught at the University of Chicago. Fernando Alvarez, for example, then and now may admonish a graduate student in a workshop to 'Bellmanize!', and everyone there knows what is meant. Thus, this methodology developed or, at least, promoted by Lucas is still key to educating graduate students in

macroeconomics at the University of Chicago, and surely not only here but at many other places as well.

The undergraduate program at the University of Chicago likewise owes an enormous debt to this approach. The program famously does not shy away from imposing a formal apparatus on its students in the key central courses in macroeconomics and microeconomics, providing students with an analytical sharpness and the ability to analyze economic questions with a degree of mathematical and logical precision that may not be required in many other places. It provides these students with a unique advantage no matter where they turn afterwards. The key people developing the program in this manner drew on the key advances by its own faculty, and on Robert E. Lucas in particular.

7. The Money and Banking Workshop

Having roamed the various fields where the economic sciences owe a lot to Robert E. Lucas, it is time to look into the atmosphere of the Money and Banking Workshop. This is the central workshop for presenting and discussing macroeconomic research at the department of economics at the University of Chicago. What follows are some personal observations from my vantage point. Needless to say, some or perhaps many of my colleagues may disagree with that perspective.

There are a variety of workshops at the department as well as at the business school of the University of Chicago, and the Money and Banking Workshop is one of them. There certainly is not a common style nor has their style stayed constant over time. Some are small, some are large. In some, the speaker does most of the talking, but that may not be true in others. With some frequency and in departure of the workshop style at most other places, a discussion may erupt among members of the audience or audience members may explain the key insight. But mostly, the workshops these days share much with workshops elsewhere. A speaker presents his work, and the skeptical audience probes the arguments presented. Comments by audience members help to discern what is going on as well as occasionally disruptively prevent the speaker from getting to the key results. What is special about nearly all workshops at Chicago, however, is the seriousness, the intensity and the depths to which papers and their arguments are investigated. I have always been drawn to this unique environment of the University of Chicago.

Some of the workshops and their style were even more unique some decades ago. I was honored and excited to have been given the opportunity to become a visitor in 1993. There is much that I remember about that visit. The Applications Workshop, run by Gary Becker, was particularly intriguing. Participants were supposed to have read the paper beforehand. The author would give a brief introduction of perhaps 15 minutes or so, just to get going and to frame the debate. The leader of the workshop—usually Gary Becker or, in his absence, Sherwin Rosen—would then ask the audience if there was a question regarding page 1. If there was none, the leader would proceed to page 2. If a question arose, it was debated more among the audience rather than awaiting an answer from the author. The author could chime in, of course, but mostly he was simply one of the possible participants in the debate and often perhaps learning more from the audience than teaching and clarifying the issues. It was a remarkable way to proceed and to discuss papers, but it involved a considerable commitment by the audience. The author had the chance to learn a lot through that interaction, but whether this was always a pleasant experience is another question. A joke kept circulating in the profession that a sink was needed in those seminar rooms so that the audience could wash the blood of its hands after killing the paper and hurting the author. It is a mean joke: while criticism always hurts, there truly was so much to gain from the amazing and dedicated audience willing to engage in the details of the analysis. If there were deficiencies to the

paper, they would be uncovered. One should not be inclined to shoot the messenger, even if that unfortunately may often be the all-too-human reaction.

The Money and Banking Workshop is the central workshop at the department, where faculty from outside or inside and, occasionally, PhD students present their latest research results regarding matters of macroeconomics. I believe that fifty years ago and during the heydays of Milton Friedman it was run a similar style as the Applications Workshop as described above. When I arrived in 2007, Friedman was long gone, and it now really was Lucas' workshop. There was a large, long table in the middle of the room, with faculty sitting around it and with the speaker standing at one of the ends. Lucas often sat at the other end. The style of the workshop was no longer the style of the Applications Workshop in 1993. Rather, the workshop proceeded by the presenter presenting the paper, and with questions asked about the details of the model or the analysis. Unusually for me, these questions may then be answered by other faculty members present rather than the speaker: perhaps a remnant of the old workshop tradition. The questions and discussions were always on the subject and never *ad personam*: it was the science that mattered, not opinion or personality, though obviously, some had more forceful personalities to speak up and make or insist on a point than others. Lucas would typically not ask or say all that much, but somehow the eyes were always on him: what does he think, what will he say? And he would usually speak up at the very end, giving his thoughts on the matter. To every speaker, his views mattered much more than the views of any other participant. It often reminded me a bit of a Roman Colosseum with the emperor pointing his thumb up or down after the performance of a gladiator. The difference here was that Lucas was perhaps a somewhat unwilling emperor. He was simply the most eminent person in the room, and so his judgement mattered, whether he wanted that or not. I believe he was rather keenly aware of that, often being careful and kind in how he would phrase a remark, rather than speaking forcefully and in a commanding tone. He might summarize the key insight of a paper in a few sentences, reducing it eloquently to its key essence. It was always clear that his remarks commanded respect, and it was always clear if a paper did not quite cut it in his view, and that was that. Perhaps the author would have wished for a better outcome. But it was fair, it was appropriate, and that is why presenters and everyone else sought it out. Not everything is a grand achievement, and everyone knows that.

Lucas would practically always come along to the dinner with the speaker afterwards, where the discussions usually kept going: that, still is very much the tradition. There are good restaurants in Chicago, and we frequented some of them. I remember Lucas mildly objecting to that on several occasions. To him, it seemed, a simpler place perhaps with decent beer would be just fine. It was the intellectual conversation that mattered and not the fine food, and I agree. A good restaurant and an intense dinner table conversation about the paper just presented honors and celebrates the speaker and the depth of the scientific discussion, though, and usually is only appropriate.

A few years after my arrival, Bob Lucas graciously decided to give the reigns of the workshop in the hands of the next generation. It was his decision entirely: no one would have objected if he had kept control of the workshop for many more years. He knew, I believe, that by handing it over, the vibrancy of the workshop would be kept alive as the next group of faculty members would take ownership and mold it as they see fit. The workshop is still the central macroeconomics workshop at the department, and still one of the two or three most vibrant and frequented workshops in the department. Lucas made sure that it was not dependent on a singular individual at its center. Indeed, it is no longer anyone's workshop. There is now a handful or so of faculty members, that are reasonably central to the activities there. Some are more central and more eminent than others, as one would expect, but none are as central as Lucas once was. Perhaps the time of a singular eminent scholar at its center has passed,

though I do not wish to predicate the future. But the style of the workshop which he helped shape lives on.

8. Final remarks

There is no doubt that Lucas has influenced macroeconomic research in ways like few other macroeconomists have done. There is no denying the influence of Keynes or Friedman, for example, but in terms of methodology and the evolving research frontier, Lucas looms large. One can check the citations in e.g. the various chapters in Taylor and Uhlig (2016) and find the considerably higher influence of Lucas' work in terms of references to his work and in terms of methodological choices than that of Keynes or Friedman. That may not be the best measuring stick, though. There is no doubt that the key contributions and ideas of these three authors as well as others simply have become part of the regular fabric of economic discourse and research without the necessity to be citing them at every point and sometimes perhaps even without the awareness of who created the underlying framework and contribution in the first place. Dostoevsky once said that 'We all came out of Gogol's Overcoat'. It is in the same vein appropriate to claim that no economist other than Lucas exerted such a profound influence on modern macroeconomics. This paper could offer no more than a bird's eye review of the various field where Lucas shaped today's economics science.

There is also no doubt that Lucas has influenced Chicago macroeconomics in fundamental ways. It is fair to say that the current faculty builds on his advances and insights to a substantial degree, both in terms of advancing research as well as teaching our students, even if the particulars have moved on from the specific topics or frameworks originally elaborated and emphasized by Lucas, and without denying other key influences and giants in the field. The way in which macroeconomics and macroeconomic research is debated and discussed at Chicago has been shaped to a considerable degree by Lucas. He may no longer be as central as an active participant of the money and banking workshop or the research frontier as he once has been. But through his body of work, his outsize influence on the field and his shaping of the department and its workshop culture, Robert E. Lucas has created an enduring and remarkable legacy at Chicago economics and beyond.

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