THE NEW SCHOOL
Economic Review

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10th Anniversary Issue

past

present

future

The New School Economic Review

The New School Economic Review is a student-run journal and blog that publishes original contributions in economics and worldly philosophy. We aim to strengthen our view of economics and the related social sciences through the lens of current events in academia and around the world. Both the journal and the blog are maintained by the NSER editorial team.

Website: www.newschooljournal.com   Twitter: @NewSchoolEcon

Editorial offices

The New School for Social Research
Economics Department – 11th floor
6 East 16th Street
New York, NY 10003

Front page

The cover displays a part of "The Passage". This is a digital clock, which is part of the Metronome public art installation on the South end of Union Square, around the corner from The New School for Social Research (NSSR). The clock displays the time in 24-hour format, with the seven digits from the left displaying the time of the day and the seven digits from the right displaying the time remaining in a 24-hour day, as tenths of a second. The center digit represents hundredths of a second, and appears as a blur.

Design: Brandt Weathers
Photo: Ian Howland

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Letter from the Editors

The New School Economic Review is now in its 10th year, which calls for a celebration! We are using this opportunity to take stock of where we stand. Over the past year, the NSER has moved forward leaps and bounds by revamping the website, updating the blog, and establishing social media outlets. For the first time in NSER history, this volume (7th) will be available in print as well as online. Moreover, this publication now has an ISSN, another first. As per the highest academic journal standards, all submissions to this volume underwent a double-blind peer review process.

This anniversary issue presents perspectives on significant contributions to the economic profession from the New School economics community including student papers and highlights some of the discussions we are currently having at the Economics Department, The New School.

While the heterodoxy has divergent views and approaches to the economics profession, Heilbroner's "worldly philosophy" continues to guide our academic work and has fostered academic engagement amongst students. An example is the student-run conference that the Economics Student Union organized in spring 2014, which brought together New School Professors, students and alumni to share visions of heterodoxy today and its place in the economics discipline. This issue of NSER includes the transcriptions of all sessions of the conference, "The State of Worldly Philosophy at the New School in 2014".

In the first session of the conference, entitled "Past: Visions of New School Economics," professors David Howell, Mark Larrimore, Edward Nell, and Anwar Shaikh reminisced and reflected on what our department used to be and drew parallels to the present. We learned that a constant at the New School economics is that we ourselves experience persistent institutional crises while we study recurring economic crises.

The conference featured panels of New School alumni from policy and academia discussing career paths and providing guidance. Both panels were clear that there are certainly challenges involved in choosing a road less traveled by, but the alumni also emphasized that the critical training that The New School offers is invaluable.

Furthermore, the panel on the future vision of New School economics happened to also be the most vigorously debated. This session touched on the issues of where the school is going, where we want it to go, how to widen the space for heterodoxy and pluralism, how to increase funding without compromising our intellectual integrity, and on *the very definition of heterodoxy* (alas, you will not find a clear definition in this journal).

This issue also features the work of economics graduate students at The New School. The Lucas critique is examined in light of the 2008 financial crisis and the “Reversal of Fortune” thesis by Acemoglu, Johnson, and Robinson is critically reviewed.

Last, but not least, this volume features essays that celebrate and discuss some of the contributions of the Economics Department Professors to the profession. Anwar Shaikh’s “Humbug production function,” which elegantly tore apart the Solow production function forty years ago, is discussed by Geoff Harcourt and Randall Wray. Thereafter, Willi Semmler’s contributions to macroeconomics is discussed by Professor Emeritus Ed Nell.

We take this opportunity to thank everyone involved in this issue; the participants at the colloquium who let us transcribe their speeches, the volunteer transcribers, the students and the professors that submitted papers to the journal, and the reviewers who, as always, sacrificed time and energy to provide invaluable feedback. Finally, we thank you, the readers, and we hope that you will find this issue engaging!

Ingrid Harvold Kvangraven and Abid Raza Khan

Executive Editors

Lucas Critique After the Crisis: a Historicization and Review of one Theory's Eminence

By Brandt Weathers

Abstract

This paper re-examines the Lucas Critique (LC) in light of the 2008 financial crisis and recent scholarship. Inspired by the theoretical reassessments of the Lucas Critique by economists (Anwar Shaikh) and historians (Daniel T. Rodgers), this paper takes on two separate tasks: 1) to understand the historical context that gave rise to Robert Lucas' infamous 1976 paper now commonly called the 'Lucas Critique', and 2) to examine relevant literature (as it addresses issues of theory, policy, and statistical techniques) since the recent US financial crisis to find out if the Lucas Critique has been subject to greater scrutiny in the economics discipline. Using the SSRN database, this paper concludes that little has changed in the perception of the Lucas Critique since 2008; however, a large quantity of associations with the theory that diverge from the content of the paper itself makes clear the need for another project to contextualize the Lucas Critique since its publication (not simply up to its publication, which is performed here).

Introduction

In early December 1995, University of Chicago Professor Robert Emerson Lucas Jr. was awarded the Nobel Prize in Economic Sciences.[1] Besides maybe the John Bates Clark Medal,[2] no award honors an economist with such prestige in the popular imagination, likely due to its association with the other renowned Nobel prizes. Here at the event Professor Lars E.O. Svensson of the Royal Swedish Academy of Sciences introduced Robert E. Lucas with these words (translated from Swedish):

> Robert Lucas is the social scientist who has had the greatest influence on macroeconomic research since 1970. The main objective of macroeconomic research is to study fluctuations in total production, employment and inflation. Lucas's contributions have transformed macroeconomic analysis and deepened our

[1] Then officially called the 'Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel' and now officially called 'The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel'.

[2] Which is granted to an American economist under the age of forty who is "judged to have made the most significant contribution to economic thought and knowledge".

> understanding of economic policy. They have led to a more realistic appreciation of what economic policy can, and cannot, achieve. Lucas has also given us more reliable methods to evaluate the effects of changing economic policy.

Professor Svensson goes on to obscurely reference Lucas' infamous 1976 paper, "Econometric Policy Evaluation: A Critique" as evidence of Lucas' achievements. Today, this paper and Robert Lucas' theoretical claim-to-fame are mutually referred to as the 'Lucas Critique' (or LC). Fascinatingly, Robert Lucas did not set out to have the "greatest influence on macroeconomic research" per se. He set out to eliminate macroeconomics altogether.

This assumption leads to many fascinating questions. What is the significance of this goal? What was going on in the world when Robert Lucas wrote his now famous 1976 paper, which earned him his 'Nobel' prize? What was going on in his life? How has his reputation fared since the most recent financial crisis? These are the questions this paper aims to assess; this journey begins in a place known for its distinct refutation of Lucas' theories for many years: The New School for Social Research.

In the Spring of 2012, Anwar Shaikh issued advanced copies of select chapters of his upcoming book, thirteen years in the making. In it, major topics central to current methods in the economics discipline are taken to task, peeling apart their histories, implications, and relation to basic facts. In the third chapter, at the time entitled "Microfoundations and Method", Shaikh laid out some of the framework for this grand project; as grounds for this paper's literature review, contentions from this chapter are summarized below.

Shaikh starts off "Microfoundations and Methods" by stating that his previous chapter had demonstrated the presence of powerful long-term patterns in capitalist economies over space, time, and a wide range of cultures. He offers two questions in light of this knowledge: 1) how could such different societies produce similar results in this way?, and 2) what notions of equilibrium/adjustment processes/dynamics are appropriate for these discovered patterns?

To answer the first question he guides the reader to an important discussion on micro-processes versus macro-patterns. He then goes into some detail on their differences and implications, but ends up with two razor-sharp conclusions: 1) many roads lead to Rome ("a correspondence with the aggregate empirical facts does not privilege any particular vision of micro processes"), and 2) *homoeconomicus* is a particularly bad one.

Here, Shaikh provides important context, in which he offers information about the origins of this division, which was first set by J.M. Keynes (in *The General Theory*). This set him apart from the previous way economic theory was organized; before Keynes, it was the classical theory of price that dominated such concepts now subsumed under macroeconomics.

The perspective offered by Keynes conflicted with the view subsequently proffered by Robert Lucas in the 1976 paper "Econometric Policy Analysis: A Critique", and by his later writings and talks. Shaikh here provides four central propositions of what we now know as 'The Lucas Critique' (or from here on—as a theoretical concept or as the paper—may be interchangeably referred to as the *LC*). These briefly are:

1) structure derives from individual decision rules of agents,

2) a change in the environment or policy also changes individual behavior and structure,

3) therefore models based on past patterns cannot predict effects of changes in environments because the structures will also change, and

4) "It follows that we need a theory of micro-behavior to predict how macro-outcomes respond to a change in environment"

Shaikh teases out the central conclusion of these propositions from a discussion Lucas had years after his famous paper when he stated that if done properly, "the term 'macroeconomic' will simply disappear from use and the modifier 'micro' will become superfluous. We will simply speak … of economic theory".

Shaikh goes on to outline the Neoclassical paradigm that was built on this theoretical cornerstone. He then walks the reader through a brief history of rationality as an idea from the Enlightenment to economists like Walras, Arrow-Debreu, and Lucas as well. This discussion connects Walras' aim in establishing an alternative 'perfect economy' to Arrow's 'general equilibrium', whose features are not ideal in the sense of social interaction or human complexity, but in that they "provide the foundation for the claim that the market is the ideal economic institutions and capitalism the ideal social form."

Connecting Lucas' theories to this long history of thought is an important project, first shown by looking at his graduate education, which was at the University of Chicago and highly influenced by Milton Friedman and his graduate 'Price Theory' course; a man who took staunch stances against practically all Keynesian thought (and even described his scholarly project as a "counter-revolution in monetary theory").[3] Lucas pit these traditions against one another in the 1976 paper that earned him the Nobel Prize; like all influential works, it was published at a time of profound social change. Such historical context has begun to be more thoroughly uncovered outside the economics discipline, particularly by Daniel T. Rodgers and a smattering of policy and business historians. Contributing to the project to contextualize the LC, as is done here (up to its publication), provides a firmer foundation to disentangle any potential idea from ideology, and reassess what technical merit it may have for the future.

Speaking of technical merit, since 2008, (and much like how the depressed 1970s changed the way people thought about the discipline) our recent Financial Crisis has provoked a call for change in the present, eminent theories of our discipline. Much to the relief of market participants, classic Keynesian methods were put into use to tame the potential market volatility and destruction. Little wonder given that the Keynesian revolution was born amid the Great Depression's unrest and that our 'Great Recession' had similar destructive potential. In short order, the profession was asked to answer for the crisis; why didn't the economic mainstream predict and prevent such a catastrophe? The responses, particularly from University of Chicago economists, were often dumbstruck and insufficient.[4]

Did all this mean that, for the first time in a generation, papers, models, and theories inspired by the Lucas Critique (LC) were on the way out? That their foundation, the Lucas Critique (LC) itself, was being questioned and criticized?

"No," said New School Professor Duncan Foley. "If anything," he stated at a panel on Heterodox Economics in 2011 at The New School, "it seems to me, one might say, that the Neoclassical mainstream of the profession has begun to 'circle the wagons' around their theories and privileges."

Research Question, Methodology, and Structure

This question, of the state of the Lucas Critique (LC) after the crisis, is the second of two key issues this paper aims to understand; has the LC (in its implications for theory, policy, and statistical technique) been reexamined and/or subjected to scholarly criticism since the

[3] Friedman, Milton. "The Counter-Revolution in Monetary Theory", IEA Occasional Paper, no. 33. Institute of Economic Affairs. First published by the Institute of Economic Affairs, London, 1970. Available online at: [http://0055d26.netsolhost.com/friedman/pdfs/other_academia/IEA.1970.pdf]

[4] Cassidy, John. "Interview with Eugene Fama". The New Yorker Online: Rational Irrationality Blog. [http://www.newyorker.com/online/blogs/johncassidy/2010/01/interview-with-eugene-fama.html]

2008 Financial Crisis; and if so, to what extent? The first issue that this paper aims to address through historical methods is to ask: what was the wider context that birthed the LC? It is in researching both of these issues that a more holistic understanding of the LC's implications for future economic research can be fully vetted.

Toward the second goal (state of the LC, post-2008), a literature review was conducted from a sample of academic papers in three scholarly databases. These databases were self-selecting; one in particular (SSRN) matched the necessary criterion and provided enough results to get a nice sample on this paper's topic (the LC) since the financial crisis.

The structure of this paper follows this general format: sections III and IV completes our first major task by establishing the historical context that birthed the LC, from a grand and narrow view. Section V completes the second important task by assessing how the LC has fared (in three ways: theory, policy, and in statistical technique) since the 2008 financial crisis. The paper then is completed with a few parting words reflecting on its findings in section VI. Lastly one can find general appendices and sources.

General Context: A Great Shift in American Life

In 2007, Robert Lucas described his frame of reference for entering economics as a young student; he said in a podcast interview that, "as a history major [in the University of Chicago] I knew no economics; I was a pseudo-Marxist who thought economic forces were what made history go. And I still do!"[5] Most Marx scholars would cringe at this reduction of Marx's theory of history; however, most descriptions of the Lucas Critique (or LC) describe its key features without greater historical context or literal presentation. Therefore, here, this paper will place the LC in its historical context so that we might fully appreciate the greater task of reassessing the paper in light of the 2008 financial crisis.

With the hindsight of historical analysis, the period in which the LC was published, the late 1960s through the end of the 1970s, has become a period of remarkable significance for the United States; a paradigm shift can be seen from across a range of disciplines (including politics, economics, environmental sciences, histories of thought in each, and many others). It is a fascinating journey to reflect upon these changes, as diverse scholars have begun to do with earnestness.

To take a brief tour, first politically, this period is noted as the beginning of the great decline in labor union participation, as well as a rapid mobilization of business interests in the

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[5] Econ Talk Podcast. "Lucas on Growth, Poverty and Business Cycles", Interviewed by Russ Roberts. [http://www.econtalk.org/archives/2007/02/lucas_on_growth.html]

political process, seen within a wide range of metrics.[6] Socially the consequences of this disruptive period were viscerally experienced:[7] rapid urban decay, stark retraction of major features of the welfare state, a dramatic rise in: crime, violence, and imprisonment of minority populations (the "War on Drugs" was infamously 'declared' in 1971); as well as a de-coupling of productivity growth rates from real wages (and its resultant inequality).

This last feature (the growth in inequality) is particularly interesting as it bridges the discussion into the work of many (albeit popular) economists. Paul Krugman calls this separation the 'Great Divergence', whereas his ever-present, Austrian counterpart, Tyler Cowen, calls this moment in US history the 'Great Stagnation'. The difference lies in their claim for the cause of this economic division (Krugman blames political factors and financialization whereas Cowen points out that this period is noted for the highest point in US fossil fuel production). The 1973 oil crisis and the rise of the so-called "euro-dollar" market play important roles in both of these perspectives, and others.[8]

Speaking of financialization, though not addressed until quite recently by the mainstream economic profession, both Marxist and Sociological literature have provided serious analysis of this trend, which is substantial.[9] This takes off, in particular, around the same time as serious financial deregulation started to take place: the repeal of Regulation Q, the dissolution of the Bretton Woods System, and the first major holes punched into Glass-Steagall (otherwise known as the legal separation of commercial and investment banking, or four sections from the Banking Act of 1933) to name a few.

Public policy scholars and business historians too have developed a convincing set of perspectives on this great transition. To quote Iwan Morgan at length,

> To date, scholarly analysis of the late twentieth-century emergence of a conservative American political economy in place of the liberal one initiated by the New Deal has predominantly focused on the fiscal and deregulatory elements of the Reagan administration's antistatist agenda. However, there is increasing recognition among historians and social scientists that the

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6 Hacker, Jacob S., and Paul Pierson. Winner-take-all politics: how Washington made the rich richer-and turned its back on the middle class. New York: Simon & Schuster, 2010. Print.

7 Katz, Michael B. The price of citizenship: redefining the American welfare state. Philadelphia: University of Pennsylvania Press, 2008. Print.

8 Madrick, Jeffrey G. Age of greed: the triumph of finance and the decline of America, 1970 to the present. New York: Alfred A. Knopf, 2011. Print.

9 Krippner, Greta R. Capitalizing on crisis: the political origins of the rise of finance. Cambridge, Mass: Harvard University Press, 2012. Print.

> Volcker Fed played a critical role with regard to both the rightward turn of economic policy and the broader structural changes in the economy in this period. To some analysts, its draconian anti-inflation strategy completed the process whereby finance grew more significant and manufacturing underwent relative decline in the more open, increasingly internationalized economy of the 1970s. For others, its success in stabilizing the value of money was the prerequisite for the so-called financialization of the economy whereby business profits grew more dependent on the provision of capital than on production of commodities in the 1980s and beyond.[10]

Morgan adeptly discusses the economic undercurrents which led to such dramatic actions by political and economic elites as well.

> Unleashed by the Vietnam War's overstimulation of the 1960s full-employment economy, inflation worsened exponentially in the subsequent decade as a result of oil-price shocks, global rises in commodity and food prices, the falling value of the dollar after termination of fixed exchange rates, and declining productivity.

All of these features, from changes in our laws and cities to social relations and international exchange systems, meld into larger historical forces; they provide a rich tapestry of the crumbling of America's institutions and way of life. From this distance, such a phenomenon has been described aptly by Daniel T. Rodgers by the title of his recent book *Age of Fracture*. Central to this complete dislocation of American life is a profound shift in ideas; from "earlier notions of history and society that stressed solidity, collective institutions, and social circumstances [...] to a more individualized human nature that emphasized choice, agency, performance, and desire."[11] Central to Rodgers' analysis is the startling shift that has taken place in economic theory; something he credits to a 1976 paper by Robert Lucas.

Specific Context: The Lucas Critique in History

On the historically-micro scale, one may examine the life and times of the LC's author:

[10] Iwan Morgan. "Monetary Metamorphosis: The Volcker Fed and Inflation." Journal of Policy History 24.4 (2012): 545-571.

[11] I have noted elsewhere that there are certain material features lacking from Rodgers' analysis; notable is his exclusion of the rise of the 'public relations' industry or the rapid monopolization of US media conglomerates—both at rates that almost defy belief'; available upon request.

Robert Lucas. Robert Emerson Lucas was born in 1937 and raised by parents who he describes as Roosevelt democrats (amidst Republican extended family). Lucas came into the world in central Washington (in Yakima), but his family moved into Seattle when their ice creamery went under during the 1937 economic downturn. Yakima was a historic railroad town with radical racial divisions—a site of key significance in the Chicano labor movement—sitting in the heart of Washington's Yakima Valley, the source of much of America's beer hops.[12] Seattle on the other hand was deeply industrial; a city whose fortunes were often intertwined with the level of military activity overseas, producing large quantities of planes and ships.

Lucas attended public schools in Seattle and, since he received a scholarship, was able to go out of state for his secondary education to attend the University of Chicago. This decision prevented him from some expectation that he would become an engineer, akin to the work of his father (the school did not have an engineering program).[13] He migrated into history for his BA, then went to Berkeley for graduate training and transferred into economics, but had to go back to Chicago for funding purposes, where he eventually completed graduate training. Here he was taught price theory by Milton Friedman while Friedman was in his prime. At some point, Lucas experienced a political transformation: from the New Deal Democrat background of his family to a Chicago school-style, anti-government libertarian.[14]

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[12] Rosales Castañeda, O. "UFWOC Yakima Valley Hop Strikes,1971", Seattle Civil Rights and Labor History Project. Available at: [http://depts.washington.edu/civilr/farmwk_ch7.htm]; timeline at: [http://depts.washington.edu/civilr/farmwk_timeline.htm];
Print source available at: Alaniz, Yolanda, and Megan Cornish. Viva la raza: a history of Chicano identity and resistance, p. 287-308. Seattle, WA: Red Letter Press, 2008. Print.

[13] "After the war, my father found a job as a welder at a commercial refrigeration company, Lewis Refrigeration. He became a craftsman, then a sales engineer, then sales manager, and eventually president of the company. He had no college degree and no engineering training, and learned the engineering he needed from the people he worked with and from handbooks."
Quote from: [http://www.econlib.org/library/Enc/bios/Lucas.html].

[14] Politically, Lucas is libertarian. Asked by an interviewer in 1982 whether there is social injustice, Lucas replied, 'Well, sure. Governments involve social injustice.' Asked by another interviewer in 1993 to name the important issues on the economic frontier, Lucas answered, 'In economic policy, the frontier never changes. The issue is always mercantilism and government intervention vs. laissez-faire and free markets.'
More available at: 1) [http://www.econlib.org/library/Enc/bios/Lucas.html].
2) Arjo Klamer, Conversations with Economists (Totowa, N.J.: Rowman and Allanheld, 1983), p. 52; and
3) Interview with Robert E. Lucas Jr., The Region, Federal Reserve Bank of Minneapolis (June 1993), online at: [www.minneapolisfed.org/pubs/region/93-06/int936.cfm]

After graduating, Lucas went to work at Carnegie Mellon, under the looming shadow of Herbert Simon—a very active participant in the Cowles Commission and a Chicago Political Science PhD—who had a lasting influence throughout the school (particularly in human behavior and decision-making, the subject of which extends as far back as Simon's graduate dissertation). Here Lucas befriended his student Edward Prescott who—when Prescott went to work at University of Pennsylvania—Lucas joined in discussions led by Lawrence Klein in his 'Conference on the Micro Foundations of Wage and Price Determination'.

This must have been a site of fascinating debate and conflict, as it held two important traditions that were influencing Lucas throughout his academic career. These could be described as micro/logic/theory and macro/empirics/policy, but experienced by Lucas via the schools surrounding the *theory of rational expectations* versus the *Cowles Commission*, respectively (in the LC, the names assigned to these categories are the "traditional economic theory" and the "theory of economic policy", which from here on this paper may refer to as TET and TEP). These traditions were both present in Klein's Conference on Micro Foundations, and are literally posed against one another in the LC; Lucas himself draws out each one then states, "one of these traditions is fundamentally in error."

The first tradition, microeconomics (or 'traditional economic theory'—TET), is easily traced to his alma mater, University of Chicago, where he studied under Milton Friedman. These were the years leading up to Friedman's publication of his magnum opus, *A Monetary History of the United States*; this work established the foundations for the now out-of-style Monetarism. Maybe more importantly, Lucas studied in Friedman's price theory course during this time, the title of which operates as a pre-Keynesian division in theory, much of which was grounded in Friedman's earlier publication *A Theory of the Consumption Function* (1957). Of lasting significance, however, was the role John Muth played in Lucas' early career, in the mid-1960s; the first three years at Carnegie were spent with this man who is popularly described as the "father of rational expectations". The first paper Lucas co-wrote with Prescott came to being due to Muth's influence, as he describes (emphasis added):

> Edward Prescott had come to GSIA as a doctoral student in the same year I joined the faculty, and we were immediate friends. A few years later, when Ed had become a faculty member at Penn, I enlisted his help on a theoretical project I had begun

> on the dynamics of an imperfectly competitive industry. That problem defeated us, but in the course of failing to solve it we found ourselves talking and corresponding about everything in economic dynamics. In a couple of years we learned large chunks of modern general equilibrium theory, functional analysis, and probability theory, **and wrote a paper, "Investment under Uncertainty," that reformulated John Muth's idea of rational expectations in a useful way. During this brief period my whole point of view of economic dynamics took form (along with Ed's), in a way that has served me well ever since**.[15]

The other tradition, macroeconomics (or 'theory of economic policy'—TEP), that Lucas was being exposed to operated, at one point, in each of the universities he worked in. University of Chicago was home to the Cowles Commission, which was for a period of time run with contributions from Lawrence Klein (of U. Penn.) and Herbert Simon (with his notions of bounded rationality). Klein was of the generation of left-wing economists who were harassed during the cold-war era McCarthy trials (some have speculated that his denial of tenure at University of Michigan was due to his former communist ties). This era had a sustained influence on the discipline that has yet to be fully appreciated. For example, a lesser-known fact about the McCarthy red purges (under the auspices of the House Un-American Activities Commission) was its impetus in establishing our discipline's monumentally influential neoclassical synthesis. To quote a recent letter by The New School's Professor Velupillai:[16]

> [Paul] Samuelson is, by now, on record (the most pungent 'confession' is recorded in Robert Clower's unpublished 'Perugia Lectures on Monetary Theory') as having 'confessed' that he had to coin the phrase 'neoclassical synthesis to keep McCarthy off [his] back'.

In the era of Senator Joseph McCarthy's show trials (the early to mid-1950s), faculty members in Chicago became hostile to the work produced by the Cowles Commission with its attempt to produce more general macroeconometric models, so it transferred its facilities

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15 "Robert E. Lucas, Jr. - Biographical", Nobelprize.org. Nobel Media AB 2013. [http://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1995/lucas-bio.html]

16 Vellupillai, V. Forthcoming as: Letters to the Editor, Economic & Political Weekly, Vol. XLIX, # 6. Distributed online to New School Economics graduate students on 25 January 2014. Also mentioned as footnote 15 in: After the Revolution: Kerry A. Pearce and Kevin D. Hoover. "Paul Samuelson and the Textbook Keynesian Model", currently available online at: [http://public.econ.duke.edu/~kdh9/Source%20Materials/Research/After%20the%20Revolution.pdf]

to Yale University (an event that took place in Chicago just as Lucas arrived for undergraduate studies). The tension between these two traditions in this history is palpable and competitive; the director of the Cowles Commission when it was rejected from Chicago, Tjalling Koopmans, received the Nobel Prize in economic sciences in 1975, the same year that William Phillips died, and just one year before the LC was published.[17]

Since the Crisis: Analysis and Results

The LC has been a foundational piece of Neoclassical orthodoxy for some time now, but many of the notions held dearly to the field have been shaken in light of the recent financial crisis. It is hard not to look at events just before the crisis without a sense of irony. Lucas himself reflected on the state of "modern depressions" in February of 2007, commenting on the monumental nature of the 1970s 'stagflation' and the fact that "modern depressions are nothing".[18] His stature as an economist was at its height in this year, as when in September 2007 Clemson University held a "Conference in Honor of Robert E. Lucas, Jr." After the crisis took place, Lucas made a defense of the field and accused critics of current modelling techniques that, "[they] have seized on the crisis as an opportunity to restate criticisms they had voiced long before 2008."[19] Have any of these features changed? Is Robert Lucas, or more relevantly the LC, viewed with different colored glasses than just five years earlier? Here this paper aims to answer this question with a review of economic literature on the LC since 2008.

Literature Review: Theory

To answer the question posed above (has the LC been reconsidered in light of the 2008 financial crisis) one must agree with Duncan Foley; there is very little evidence that a paradigm shift is happening in our discipline. Moreover, there is sufficient evidence of surprising interpretations and uses of the Lucas Critique.

For one, many papers in this literature review cite the LC with very little explanation or definition. It gives the impression that many scholars believe they are girding their research with legitimacy by association. Secondly, of the papers that attempt to define the LC, many provide descriptions that veer far from the content within the paper itself. Some go so far as to provide definitions that verge on homespun interpretations of various levels of

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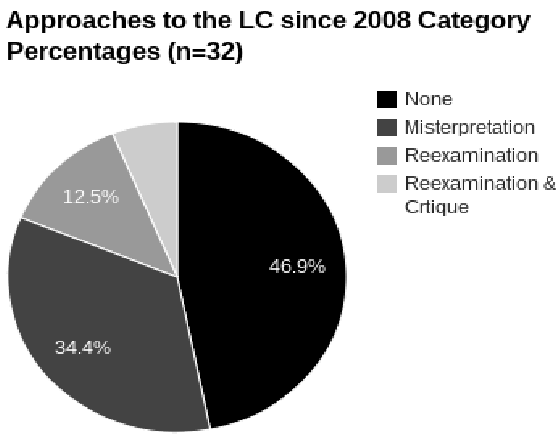
[17] The commission has gone on to be remarkably successful in its association with nobel prize laureates: [http://cowles.econ.yale.edu/archive/people/nobel.htm]
[18] Econ Talk Podcast (*op. cit*).
[19] Lucas, R. "In defence of the dismal science", The Economist Online. Posted 6 August 2009. Available at: [http://www.economist.com/node/14165405]

obscurity; by identifying these articles in the Appendix, it is left to the readers to sample these formulations further.

But more central to our research question is understanding categories and quantities of assessments of the LC post-2008. Here this paper has broken down the results of this literature review into four categories: None, Misinterpretation, Reexamination, and Reexamination & Critique. To explain briefly:

- 'None' is a categorization of papers that by no means reconsider the tenets or features of the LC—their treatment of this theory is non-critical;
- 'Misinterpretation' includes papers that provide definitions or uses of the LC that significantly diverge from or do not include content from the LC paper itself and/or historical context surrounding the moment of its publication;
- 'Reexamination' includes papers that consider whether or not the LC is a legitimate theoretical basis for economic research, but fall short of criticizing the LC or reinterpreting it in a transformative way;
- 'Reexamination & Critique' includes the short list of papers that question the LC and come to conclusions which alter the theory's significance in their framework.

From the 32 papers drawn from my literature review, my categorization of their treatment of the theory are as follows:



Here, the reader can see that almost half of the treatments of the LC since 2008 have not reconsidered their theoretical implications in any way. Fascinatingly, however, is the large percentage of papers that utilized the LC in a fundamentally divergent way ('Misinterpretations') often by citing the paper but providing little reference to its actual content or context (or by assigning a definition that seriously diverges from the original 1976 paper). Also, one can see a decent showing for 'Reexamination'; one should note that many of these offered hard empirical reconsiderations, some utilizing *a la mode* statistical techniques in this endeavor (covered in detail later in this literature review). Others often utilized *fascinating* techniques with or extensions of the LC without questioning its apparent legitimacy (Bowles & Reyes, 2009). Finally, among the papers that actually provided a 'Reexamination & Critique' we see only two results. Among these are a fascinating retrospective on the meaning of the Phillips Curve (Turnovsky 2009) and a hard statistical critique of the predictive ability of representative agent models (Chang et al 2010).

Literature Review: Policy

Beyond theoretical implications, it is important to assess the LC's perceived effect on policy and measurement. Is the LC still considered a legitimate foundation when formulating *policy recommendations* and *econometric models*? Here the literature review is expanded to assess these two considerations.

First, regarding policy, the LC has carried significant weight. Within his paper, Lucas offers what he believed were the key limitations for using econometrics in the method prescribed by the TEP (Theory of Economic Policy) for policy simulations: models that aggregate past experiences and behavioral characteristics are non-stochastic and arbitrary; therefore, infinite variances arise in the *long run* and behavioral parameters (which are "governing the rest of the system") react definitively to policy changes in the *short run*. In other words, as expressed by Soorea (2007),

> "[a]ccording to the Lucas critique, changes in policy affect the behavior of rational agents and such behavioral changes can invalidate the model relationships estimated under the previous policy regime [which means] shifts in economic policy change how policy affects the economy because agents in the economy are forward- rather than backward-looking and adapt their expectations and behavior to the new policy stance."

Based on the immediate historical context surrounding the paper's publication and the stated target repeated in the LC itself, such policy implications were intended to primarily affect monetary policy; instead, it affected economics in general.

But what about our subset of papers since the crisis? How have they considered the implications for using behavioral models that follow a random walk and provide the driving micro-grounded forces for macroeconomic phenomena, suggested by the LC? Of all the 32 papers reviewed in this study, 14 were policy-applied. Among these 14 papers we get a sense of what contemporary policy realms are considered relevant for the LC.

Unsurprisingly, the policy field most addressed (8 of 14) with the LC in these papers was *monetary policy*. The applications ranged widely, from Sweden's monetary policy (Jonung and Fregert, 2010) to firm behavior (Tepper, 2010; Mash, 2010) to large, international data sets (Petreski, 2010). The implications, however, though nuanced, provided some predictable results. One paper stated that the forward-looking effects inherent in the LC's prediction of policy neutrality could neither be proven nor rejected (Sooreea, 2008) and another stated that assessing international growth rates before and after the monetary policy shift brought on by the decline of the Bretton Woods exchange-rate system dismantles the LC's predicted policy neutrality (Petreski, 2010). But besides these, the other six papers all categorically accept the LC's policy implications—for firm behavior (Tepper; Mash), Swedish, Canadian, and US inflation-targeting policies (Jonung; Kitov and Kitov, 2011; Thomas and Nakov, 2011), and Fed policy 'conundrums' (Thornton, 2012).

Three other papers address policy topics tangentially related to monetary policy, which include Fed 'bubble' policies (Assenmacher-Wesche and Gerlach, 2010), policies related to contemporary macroeconomic 'control theory' (Turnovsky, 2009), and the politics affecting international monetary policies (Aizenman and Marion, 2011). With the exception of Turnovsky (who provides a more holistic assessment of the pros and cons of the LC's introduction to macroeconomics), these papers regard these realms as rife with policy-neutralizing, behavioral effects.

Lastly, three papers assess less traditionally LC-relevant policy realms: labor market policy effects (Chang et al, 2010), experimental education policy (Carrell et al, 2011), and international competition and trade liberalization in Australia (Karunaratne, 2012). One perceived the LC's policy-neutralizing effects as evident from their experimental observations (Carrell), while the other two posed serious critiques regarding the LC's supposed behavioral super-parameters (Chang) and empirical implications for neoliberal trade policies (Karunaratne).

Literature Review: Statistical Techniques

Finally, regarding econometric or statistical tools for measurement, the LC's impact was equally important, yet more diversely so. As explained earlier, the LC posed a challenge to the old way of assembling macroeconometric models, which was done on a large scale with a tremendous quantity of historic data and sectoral variables in a linear, time series analysis. This was exemplified in the work performed by the Cowles Commission and Lawrence Klein. These models aimed to be comprehensive in nature and capable of forecasting future outcomes of key economic variables. This approach's most popular embodiment was in the Phillips Curve, which negatively related unemployment with inflation. The publishing and promotion of the LC timed precisely with the decline in the Phillips Curve's historic relationships. He subsequently argued for economic models constructed on theoretically-confirmed relationships that are invariant to policy changes.

This is a long walk to rehash what has been stated earlier; however, it leads to a key point in understanding what happened to the field of macroeconometric modelling: a desire to build models fortified by fundamental relationships confirmed by economic theory, also known as 'microfounded models'. These were not constructed on the national level, but instead aggregated from the individual level. This seismic change contextualizes our task in this section: to assess (among the reviewed papers) what statistical techniques are considered on various macroeconomic problems, how they're used, their relationship to the LC, and how robust they're perceived to be in this light.

Below several statistical techniques, and how the reviewed papers perceive them against the LC, are summarized and discussed. The techniques, and their prevalence in the literature sample, are: Dynamic Stochastic General Equilibrium models (4 papers), Vector Autoregression models (4 papers), Kalman filter (1 paper), the Generalized Method of Moments (2 papers), and Agent-based modelling (1 paper).

The Dynamic Stochastic General Equilibrium (DSGE) models were the first major tools established after the LC; these simplified difficult equations primarily utilized (at least initially) aggregated, homogenous representative agents and aimed to establish cross-market, equilibrium forecasts based on prices derived from summed rational expectations. These ideas, like the nature of the agents they eventually chose to represent, evolved over time into two competing schools of thought (Real Business Cycle and New Keynesian Models) divided by their willingness to incorporate 'realistic' features into wage and price adjustments.

The discussion and use of these models in this literature review was, not surprisingly, substantial. What was surprising was the widespread sense of inadequacy with these tools. Some papers expressed an interest in increasingly sophisticated iterations of these classic models, such as incorporating heterogeneous agents (Levine and Pearlman, 2008), but also expressed concern for the continued prevalence of the time-inconsistency problem. Other complaints noted the irony that fundamental parameters (taste and technology) of representative agent models (i.e. DSGE) could equally be subject to changes in policy regimes (Chang et al, 2010), as well as a concern with frequent data fit and misspecification problems without wide range of ad hoc improvements (Feve, 2010), and a frustration in application such that not all relevant parameter values can be pinned down by microeconomic evidence (Jeanfils, 2010). This classic model, it seems, has been displaced by more modern statistical and computational alternatives.

One of these other methods was the Vector Autoregression model (or VAR). Built on processes from statistical AR (Autoregressive) models, the VAR model offered another rebuttal to the LC, since it provides an output based on the continuous stream of past values. This process permits adaptive variables that can be explained by lagged effects of their own or, other, variable values. The key difference here is that the relationship between variables does not have to be strictly defined—as prescribed by Lucas in his argument for deep parameters. Fascinatingly, this process has also been modified recently by Bayesian theory, altering the perceived randomness of the hypothesized variables to, as one might expect, prior probabilities.

Though the VAR models enable an econometrician to freely assess the relationships between any numbers of variables, they are, within this literature review sample, almost universally perceived as vulnerable to the LC. This is for a number of reasons, which include the ultimate bias resulting from long-run restrictions (Freve, 2010), vulnerability to structural economic changes (Jeanfils, 2010; Sooreea, 2008), and a perceived inability to capture important effects of important macroeconomic parameter changes (Benati, 2010). For these authors, even though VAR models permits one to represent aggregate data and conduct diverse quantitative tasks with few restrictions, the VAR models simply cannot overcome the overwhelming demands of the LC.

With similarities to dynamic Bayesian probabilities and no reliance on a body of past evidence, the Kalman filter, which operates at real-time, has provided one of the more exciting fringes of practical econometrics unbound by the LC. This two-step process operates recursively and is unconstrained by expectations of parametric error distributions.

Its implications, according to Turnovsky (the only author who addressed the Kalman filter in this literature review), are essential: “The idea is that the policy-maker does not need to be able to predict exactly how private agents will respond to its policies. Rather, it can simply use the Kalman filter and update parameter estimates each period. While this means that the policy-maker will always be one period behind in his perception of the private sector's behavioural responses, they argue that this may be good enough for most applications of macroeconomic policy." This opinion sample is certainly too small, but such a conclusion appears resoundingly hopeful for the Kalman filter's potential.

Generalized Method of Moments, a statistical method that earned its champion Lars Peter Hansen the most recent Nobel prize in economics, is a modification of much older ideas devised by the famed statistician Karl Pearson that had fallen out of fashion with the rise of maximum likelihood estimation. Using a concept derived from physics, GMM's "moment" operates as a metaphor for a population's parameters that under certain conditions (non-finite dimensional parameters) work beyond the capabilities of maximum likelihood estimation, while maintaining all of its desired statistical benefits (i.e efficiency, consistency, normality, etc). GMM, however, does not stand in contrast to models exhibited above. Its increased use has opened up econometrics to confidently impute new, potentially unorthodox information regarding previously held parametric assumptions as the "information contained into the population moment restrictions is used as the [tool's own] instruments." In other words, it prepares forecasts for the unpredictable, but it still operates with structural models, something Lucas would have, in theory, appreciated.[20]

This literature review discovered opinions of hesitant optimism toward the GMM's potential *overall*, but unrestrained optimism in its implications for the LC. The brief hesitancy was directed toward the GMM's strength in its specificity, as the GMM provides but a "partial specification of an econometric model", which is considered "both an advantage and a disadvantage," but, in general, "allows an econometrician to learn something without needing to know all the things (Feve, 2010)." Other drawbacks include the GMM's balancing of total number of instruments (which expand rapidly over greater periods) and the apparent statistical efficiency with greater bias. These shortcomings did not decrease the perceived reliability of the tool to enable the author to make strong statements regarding the validity of the LC (Petreski, 2009).

The last technical approach to be explained and assessed is as old as modern computers: Agent-Based Modeling (ABM). Connected to the developments behind game-theory, ABM attempts to provide a generative framework for achieving equilibrium (and/or emergent) solutions with a system of potentially diverse agents guided by a set of assumptions that provide initial conditions driving their behavior under bounded rationality.

[20] Handbook of Research Methods and Applications in Empirical Macroeconomics, edited by Nigar Hashimzade, Michael A. Thornton, p. 207. ISBN-10: 0857931016.

Clear from this description is ABM's bearing of the torch offered by the LC in establishing microfoundations to macro-phenomena, but with considerable more nuance and complexity, in both meanings of the latter term. It is here that one can see the seriousness by which the LC's challenge can be taken, while managing to shed its ideological trappings.

Here, in potentially the most sophisticated of trappings, the final frontier of computational tools is viewed with sober pragmatism toward the LC. In an effort to guard against the LC, Ashraf et al (2012) states that the ABM approach may be made partially-robust by using a sensitivity analysis to determine which behavioral characteristics are most important in determining macroeconomic outcomes; therefore, the model partially incorporating the LC but does not subordinate all dynamics to all behavioral characteristics.

Conclusion

Collectively, the post-2008 literature review and the historical context leading up to the LC are meant to help answer the questions posed at the beginning of this paper. These questions are: "What is the significance of this goal [referring to Lucas' intent to eliminate macro altogether]? What was going on in the world when Robert Lucas wrote his now famous 1976 paper, which earned him his 'Nobel' prize? What was going on in his life? How has his reputation fared since the most recent financial crisis?"

First, what was going on in Robert Lucas' life when he wrote his now famous paper? One can see that Lucas was young man in his academic career who was caught between two warring traditions. One was from the school where he was educated, the other was from where he was employed. One was politically associated with counter-revolutionary, anti-statist, laissez-faire monetarists; the other with public-planning, economic-forecasting lefties shortly after the end of the Vietnam War. Lucas had chosen the political ideology of his extended family and his education over that of his immediate family and the elites of economics academia during, what seemed like, an endless recession. In the process he formalized this conflict in his 1976 paper.

What was going on in the world when Lucas wrote the paper? Stagnating wages, high unemployment, high inflation, Alan Greenspan as the Chair of the Council of Economic Advisers, Arthur Burns as the Chairman of the Federal Reserve, Gerald Ford as President, and forever altered energy prices due to OAPEC oil embargo in a post-Bretton Woods monetary system. For labor advocates, the world had regressed to a crueler, uncertain, and economically savage place; for the new generation of financial power brokers, the world had become their digitized, interconnected, deregulated land of opportunity; for politicians, a movement was underfoot to reassert business interests at the summit of national priorities.[21] Lucas' paper was very much at the right place at the right time.

So what was the significance of Lucas' goal to do away with macroeconomics as a discipline, dissolved within microeconomics? His goal, and the paper that mobilized it, operated as a zeitgeist in economics. He took two ideas of how to deal with economic crisis with all their political, historical, and social trappings, pit them against one another, and announced a winner. His goal was also rhetorical (utilizing the law of excluded middle) in its hardened theoretical presentation. Lastly, his goal was victorious. Lucas became a Nobel Laureate, a full-named Professor at his Alma Mater, and, if one is to judge ideas by their ability to set the terms of debate, the LC has managed to become maybe the strongest held assumption in macroeconomics over the last quarter century.[22]

But what about now? What about the LC since the 2008 financial crisis? Here I'd like to repeat what the evidence suggests and then offer advice for future thoughts about this problem, to help us learn more about the history of our field.

The *theoretical and policy aspects* of this paper's literature review of the LC since 2008 suggests that, though maybe not as referenced as it once was, it is still both a largely uncontested assumption as well as an often fashionable idea to associate with one's work. The *econometric aspects* of the literature review offers how one might expect future discussions on the LC unless we live to see a major paradigm shift; we can expect the LC to continue to be perceived as deeply scientific, too demanding for antiquated techniques, but a proper ideal for the macroeconometrics field with the emergence of better methods and computing technology.

I would like to offer some advice on future research on this topic. First, a history of the LC since its publication is needed. Clearly so much more has been attached to this idea since it was first published in 1976, which cannot be explained by the pre-LC history provided here.

Second, we need to distinguish between man and manuscript. The term 'Lucas Critique' needs to either refer to Robert Lucas' (often incendiary) words and opinions over time or to his 1976 paper, "Econometric Policy Evaluation: A Critique". It cannot conflate both because this affects the way we understand what was said, when it was said, why it was

[21] Hedrick Smith states that the rubicon was crossed in favor of business interests in Washington in 1978. (Smith, Hedrick. Who stole the American dream. New York: Random House Trade Paperbacks, 2013. Print.)

[22] A simple search on the Google N-Gram Viewer shows the explosive rise of the use of the term "Lucas Critique" and the formal name of its relevant academic paper source.

said, and in what context. It shades our understanding of our discipline's history. Also, regarding the wide range of econometric techniques inspired by the LC, it is essential that if its non-normative characteristics are to receive their due, it should be without aid of any bald-faced political implications or associations.

Lastly, we need to demand Robert Lucas' complete papers be archived for future research. Many figures in the 'Chicago School' have carefully hedged what information becomes available about their careers and lives to the public and scholarly community. The best example may be the lack of an authoritative Milton Friedman biography—Lanny Ebenstein's 2009 bio was given access to Friedman's papers, but notoriously offered the Friedmans final editing rights. Unless these men of letters want to be associated with the long list of dark figures and institutions who forever limit access to their internal workings, they should open up, so that we young scholars can fully understand what's going on in this field we're committing our lives to.

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Appendix: List of post-2008 LC papers and assigned code

Symbol - *categorization*:

- no symbol - *None*,
- ## - *Misinterpretation*,
- ** - *Reexamination*,
- §§ - *Reexamination & Critique*.

Investigating Inflation Persistence Across Monetary Regimes
ECB Working Paper No. 851
Luca Benati
European Central Bank (ECB)
Date posted: January 26, 2008
Working Paper Series

The Credibility Problem Revisited: Thirty Years on from Kydland and Prescott
Review of International Economics, Vol. 16, Issue 4, pp. 728-746, September 2008
Paul Levine and Joseph Pearlman
University of Surrey - Department of Economics and London Metropolitan University - Department of Economics, Finance and International Business (EFIB)
Date posted: August 20, 2008
Accepted Paper Series

International Reserves Holdings with Sovereign Risk and Costly Tax Collection
Joshua Aizenman and Nancy Peregrim Marion
University of California, Santa Cruz - Department of Economics and Dartmouth College - Department of Economics
Date posted: October 23, 2008
Working Paper Series

The Failure of Models that Predict Failure: Distance, Incentives and Defaults
Chicago GSB Research Paper No. 08-19, Ross School of Business Paper No. 1122, EFA 2009 Bergen Meetings Paper
Uday Rajan , Amit Seru and Vikrant Vig
University of Michigan at Ann Arbor - Stephen M. Ross School of Business , University of Chicago - Booth School of Business and NBER and London Business School
Date posted: November 10, 2008
Last revised: August 15, 2010
Working Paper Series

***Are Taylor-Based Monetary Policy Rules Forward-Looking? An Investigation Using Superexogeneity Tests*
Applied Econometrics and International Development, Vol. 7, No. 2, 2007
Rajeev Sooreea
Pennsylvania State University
Date posted: November 24, 2008
Accepted Paper Series

Marketing Models and the Lucas Critique
ERIM Report Series Reference No. ERS-2004-080-MKT
Harald J. van Heerde , M. G. Dekimpe and William P. Putsis Jr.
Massey University , Catholic University of Leuven (KUL) - Department of Applied Economics and University of North Carolina (UNC) at Chapel Hill - Marketing Area
Date posted: January 17, 2009
Working Paper Series

Emerging Floaters: Pass-Throughs and (Some) New Commodity Currencies
Journal of International Money and Finance, Vol. 29, No. 8, 2010
Emanuel Kohlscheen
Government of the Federative Republic of Brazil - Central Bank of Brazil
Date posted: March 26, 2009
Last revised: April 10, 2011
Accepted Paper Series

An Incentive Theory of Matching IZA Discussion Paper No. 4145 Alessio J.G. Brown , Christian Merkl and Dennis J. Snower Kiel Institute for the World Economy , Kiel Institute for the World Economy and University of Kiel - Institute for World Economics (IfW) Date posted: May 3, 2009 Working Paper Series ***Economic Incentives and Social Preferences: A Preference-Based Lucas Critique of Public Policy* CESifo Working Paper Series No. 2734 Samuel Bowles and Sandra Polania Reyes Santa Fe Institute and University of Siena - Faculty of Economics Date posted: August 5, 2009 Working Paper Series *Evolving Phillips Trade-Off* ECB Working Paper No. 1176 Luca Benati European Central Bank (ECB) Date posted: April 26, 2010 Working Paper Series ##*Are Policy Counterfactuals Based on Structural VARs Reliable?* ECB Working Paper No. 1188 Luca Benati European Central Bank (ECB) Date posted: May 6, 2010 Working Paper Series *Monetary Policy and Financial Imbalances: Facts and Fiction* Economic Policy, Vol. 25, Issue 63, pp. 437-482, July 2010 Katrin Assenmacher-Wesche and Stefan Gerlach Swiss National Bank and Goethe University Frankfurt - Institute for Monetary and Financial Stability (IMFS) Date posted: July 20, 2010 Accepted Paper Series §§*Labor-Market Heterogeneity, Aggregation, and the Lucas Critique* NBER Working Paper No. w16401	Yongsung Chang , Sun-Bin Kim and Frank Schorfheide University of Rochester - Department of Economics , Concordia University, Quebec - Department of Economics and University of Pennsylvania - Department of Economics Date posted: September 27, 2010 Working Paper Series ##*Some Efficiency Effects of Inflation Under Nominal Wage Rigidity* Alexander Tepper University of Oxford - Christ Church College Date posted: October 17, 2010 Working Paper Series ***Dynamic Macroeconometric Modeling (La Modélisation Macro-Économétrique Dynamique) (French)* Banque de France Working Paper No. NER-E 129 Patrick Feve University of Toulouse 1 - Toulouse School of Economics (TSE) Date posted: November 6, 2010 Working Paper Series *A Guided Tour of the World of Rational Expectations Models and Optimal Policies* National Bank of Belgium Working Paper No. 16 Philippe Jeanfils National Bank of Belgium Date posted: November 6, 2010 Working Paper Series *Endogenous Indexing and Monetary Policy Models* Economics Discussion Paper No. 2007-36 Richard Mash University of Oxford - Department of Economics Date posted: November 29, 2010 Working Paper Series

##_Inflation and Price Adjustments: Evidence from Norwegian Consumer Price Data 1975-2004_
Fredrik Wulfsberg
Norges Bank
Date posted: November 29, 2010
Last revised: March 13, 2012
Working Paper Series

Inflation Targeting is a Success, so Far: 100 Years of Evidence from Swedish Wage Contracts
Economics: The Open-Access, Open-Assessment E-Journal, Vol. 2, 2008-31
Lars Jonung and Klas Fregert
Dept. of Economics, Lund University and Lund University
Date posted: December 18, 2010
Accepted Paper Series

**_Analysis of Exchange-Rate Regime Effect on Growth: Theoretical Channels and Empirical Evidence with Panel Data_
Economics Discussion Paper No. 2009-49
Marjan Petreski
School of Business Economics and Management, University of American college - Skopje
Date posted: December 18, 2010
Working Paper Series

§§_Stabilization Theory and Policy: 50 Years after the Phillips Curve_
Economica, Vol. 78, Issue 309, pp. 67-88, 2009
Stephen J. Turnovsky
University of Washington - Institute for Economic Research
Date posted: January 24, 2011
Accepted Paper Series

##_From Natural Variation to Optimal Policy? The Lucas Critique Meets Peer Effects_
NBER Working Paper No. w16865
Scott E. Carrell , Bruce Sacerdote and James E. West
University of California, Davis - Department of Economics , Dartmouth College - Department of Economics and Baylor University
Date posted: March 7, 2011
Working Paper Series

##_A Win-Win Monetary Policy in Canada_
Oleg Kitov and Ivan Kitov
University of Warwick and Russian Academy of Sciences (RAS) - Institute for the Geospheres Dynamics
Date posted: April 1, 2011
Working Paper Series

##_Real-Financial Linkages in the Canadian Economy: An Input-Output Approach_
Economic Analysis Research Paper No. 065
Danny Leung and Oana Secrieru
Statistics Canada and Bank of Canada
Date posted: June 15, 2011
Working Paper Series

##_International Reserve Holdings with Sovereign Risk and Costly Tax Collection_
The Economic Journal, Vol. 114, Issue 497, pp. 569-591, 2004
Joshua Aizenman and Nancy Peregrim Marion
University of California, Santa Cruz - Department of Economics and Dartmouth College – Dept. of Economics
Date posted: September 16, 2011
Accepted Paper Series

##_Optimal Monetary Policy with State-Dependent Pricing_
FEDS Working Paper No. 2011-48
Carlos Thomas and Anton Nakov
Banco de España and Bank of Spain
Date posted: November 16, 2011
Working Paper Series

Bank Capital Regulation with an Opportunistic Rating Agency
Swiss Finance Institute Research Paper No. 12-19
Matthias Efing
Swiss Finance Institute
Date posted: May 3, 2012
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Working Paper Series

Growth and Trade Dynamics Under Regime Shifts in Australia Journal of Economic Studies Vol.23 No.2 1996 Neil Dias Karunaratne University of Queensland - School of Economics Date posted: June 16, 2012 Accepted Paper Series *How Inflation Affects Macroeconomic Performance: An Agent-Based Computational Investigation* Quamrul Ashraf , Boris Gershman and Peter Howitt Williams College - Department of Economics, American University - Department of Economics and Brown University - Department of Economics Date posted: July 7, 2012 Working Paper Series ##*Agency Theory, Values, and Early Evolutionary Economic Theory* Gerhard Fink , Manuel Wäckerle and Maurice Yolles IACCM International Association for Cross Cultural Competence and Management , affiliation not provided to SSRN and John Moores University - Centre for the Creation of Coherent Change and Knowledge (C4K) Date posted: July 29, 2012 Working Paper Series *Greenspan's Conundrum and the Fed's Ability to Affect Long-Term Yields* FRB of St. Louis Working Paper No. 2012-036A Daniel L. Thornton Federal Reserve Bank of St. Louis - Research Division Date posted: September 18, 2012 Working Paper Series	*Hypothesis Testing and Finite Sample Properties of Generalized Method of Moments Estimators: A Monte Carlo Study* Federal Reserve Bank of Richmond Working Paper No. 90-12 Ching-Sheng Mao National Taiwan University - Department of Economics Date posted: November 3, 2012 Working Paper Series ##*The Lucas Critique and the Stability of Empirical Models* FRB Richmond Working Paper No. 06-05 Thomas Lubik and Paolo Surico Federal Reserve Banks - Federal Reserve Bank of Richmond and London Business School - Department of Economics Date posted: December 7, 2012 Working Paper Series ##*Exploring the Common Roots of Culture, Politics and Economics* Maurice Yolles and Gerhard Fink John Moores University - Centre for the Creation of Coherent Change and Knowledge (C4K) and IACCM International Association for Cross Cultural Competence and Management Date posted: December 8, 2012 Last revised: January 26, 2013 Working Paper Series

How Nations Succeed - A Review of the Reversal of Fortune Thesis

By Ingrid Harvold Kvangraven and Collin Constantine

Abstract

This paper reviews the methodology and theory supporting Acemoglu, Johnson and Robinson's (2001) famous "Reversal of Fortune" thesis. Their thesis provides a simple and linear explanation to why some countries are rich today, while some are poor. It argues that European colonialists settled and introduced "good" institutions in countries that were poor in 1500, while they did not settle in countries that were rich in 1500, and "extractive" institutions were introduced instead. AJR argue that the types of institutions introduced had persistent effects on economic growth in the countries colonized. They argue that countries where "good institutions" were introduced, meaning private property rights for a large section of society, were able to take advantage of industrialization opportunities, and develop, whereas those countries with extractive institutions are poor today.

This paper finds significant flaws in the methodology employed by Acemoglu et al., both with the proxies used for wealth in 1500 and with the oversimplified historical framework employed. Strikingly, re-running their regressions with better data causes much of the reversal to disappear.

Furthermore, by examining the development trajectories of countries of countries in the West, East Asia and Africa, suggests that it is the creation of a nurturing environment for industrial development that has allowed countries to develop, and institutions of private property only come into place after countries have reached a certain level of development. Thus, this paper recommends a closer examination of how countries have managed to successfully develop throughout history. Although this paper does not launch a thesis as simple and appealing as Acemoglu et al.'s, it uncovers important weaknesses in the reversal of fortune thesis, and suggests alternative policy recommendations.

Introduction

The "reversal of fortune" (RF) thesis as presented by Acemoglu, Johnson and Robinson (2001), henceforth AJR, is the most discussed contribution to development economics today (Austin 2008). Their thesis, which can be read in more detail in their book titled *Why Nations Fail: The Origins of Power, Prosperity, and Poverty*, provides some important insights to the role of institutions in development, building on Douglass North's Nobel Prize winning work on institutional economics (Bardhan 2010).

By and large, the RF thesis provides a simple and linear explanation to why some countries are rich, while some are poor. In short, it argues that differences in institutions established by European colonialists led poorer parts of the pre-colonial world to become rich, while it transformed some of the more prosperous parts into poor economies. AJR argue that the reason for this is that in colonies that were relatively poor, the colonizers were more likely to settle, and to introduce institutions that encourage investment. On the other hand, in societies that were relatively rich when colonized, the colonizers were more likely to impose extractive institutions. Societies with good institutions were able to take advantage of industrialization opportunities in the 19th century. The RF thesis fits well into the widely accepted neoliberal framework, where the state's role is considered to be to provide public goods such as infrastructure, and a stable macroeconomic framework for the market to operate freely and efficiently.

This paper starts by assessing the validity of AJR's methodology, before evaluating their central argument, namely that institutions such as private property rights are the fundamental driver of economic development. The paper discusses what a successful development trajectory involves, and explores some alternative theories. Finally, some policy implications are discussed before the paper concludes.

Flawed methodology

Studying long-term income trends poses several challenges, such as how to measure data and how to determine causality. AJR argue that their measures of urbanization and population density represent valid proxies for wealth in the 1500s. In the past decade, many scholars have questioned the validity of AJR's use of data, however. While some (e.g. Bayly 2008, Diamond 2012, Nunn 2012) have attempted to adjust or adapt the RF thesis to other settings, others outright reject it (e.g. Sachs 2012). Economic historian Gareth Austin (2008) argues that empirical evidence for African economies around 1500 is very limited; Glaeser et al. (2004), and Fukuyama (2012) criticize AJR's measure of political institutions for being inaccurate. Notably, if the data employed is unreliable or inaccurate, the RF thesis stands on shaky ground.

Sanghamitra Bandyopadhyay and Elliott Green (2012) from the London School of Economics are among a growing number of scholars that question AJR's empirical tests. They provide an alternative measure of urbanization with 71 observations from both Africa and the Americas, instead of the 41 observations AJR employ, whereof none of them are from Africa. Strikingly, when they replicate AJR's regressions with the more accurate data, the significant and negative correlation between pre-modern and temporary income disappears, suggesting that no reversal has taken place. Furthermore, they find flaws in the way that AJR measure population density, as it fails to measure density on arable land. When they correct the measure with more accurate data, the relationship is no longer robust.

Bandyopadhyay and Green (2012) and Austin (2008) argue that the data that exists on Africa suggests that the continent was poor even before formal colonization in the 18th century. Thus, there was no reversal of fortune for Sub-Saharan Africa, but rather a deepening of relative poverty. In fact, in replications of AJR's regressions without the Neo-Europes (US, Canada, Australia and New Zealand), the negative correlation between pre-colonial income and contemporary GDP disappears (Bandyopadhyay and Green 2012, Olsson 2004). Thereby, the thesis has little explanatory power for changes in income within Latin America and Africa.

Furthermore, AJR's bundling of colonies compresses history drastically (Austin 2008). Although the period of colonization lasted for 500 years, Nigeria, for example, was only under colonial rule from 1903-1960. Latin American countries, on the other hand, were colonized between 1500 and 1830, while African countries were colonized after 1885 (Olsson 2004). What’s more, the ways the continents were colonized were very different, with the first round being mercantilist and the second more imperialist. Between those two waves came the gradual colonization of the more developed Asian countries and the Neo-Europes.

Notably, dividing colonies into just two camps, settler and non-settler, creates an oversimplified picture of history, and AJR ignore that there was a clear distinction between settler and non-settler in African colonies as well (Austin 2008). Additionally, AJR's assumption that colonial rule was purely extractive in Africa and Latin America, does not correspond with historical facts. On the contrary, it was often in the economic and political interests of the colonial administrations, even in the non-settler colonies (with extractive institutions) to encourage local enterprise. Meanwhile, in the South of the United States settled Europeans promoted slavery, and in South Africa they established apartheid (Sachs 2012). The picture is clearly more complex than what AJR present. Although a reversal may have taken place for some countries, it does not appear to have been a generalized global phenomenon.

How nations succeed

Inherent in the RF thesis is the argument that private property rights are essential for economic development. The argument for a strong link between private property rights and economic development fits well into the vast economic literature on effects of competition on innovation and growth, which has been studied by many economists. For example, Schumpeter (1947) argues that competition hurts growth because it reduces the monopoly rents that induce firms to innovate, while Aghion and Howitt (1988) argue that growth increases with property rights protection, as the profit accruing to a successful innovator would increase with stronger protection of property rights. Meanwhile, Aghion *et al.* (2006) argue that trade liberalization may discourage innovation in backward firms, whereas it encourages innovation in advanced firms that are able to compete with foreign companies.

The argument of AJR is that societies with institutions of private property will be able to take advantage of industrialization opportunities, while societies with extractive institutions, where political power is concentrated in the hands of the small elite, will fail to do so. AJR (2001:9) define good institutions as those that "provide secure property rights…for a broad cross section of the society". This is a common definition of institutions in much of the institutional economics literature (Bardhan 2010). In the standard institutions view, institutions constrain the government or others from intervening in someone's property rights. The alternative view is that "enabling institutions" incentivize people and companies to make investments they otherwise would not have been able to make. Although private properties may be a part of the enabling institutions, this alternative view also requires a more active government. Examples could be social networks, community organizations, government services or a national innovation system facilitating training and technology absorption. Bardhan (2006) futher argues that constraining and enabling institutions often work together, and that even if private property rights are secure, enabling institutions might be necessary in order to overcome coordination failures. Moreover, various institutions may play different roles in different stages of development, depending on the context.

Meanwhile, economic historian Erik Reinert (2006) argues that it is a country's productive structure determines the type of institutions that emerge in society, and not vice versa. He finds that through history, only societies that have already achieved a certain level of manufacturing, or other activities of increasing return, have achieved what AJR call good institutions. Furthermore, he argues that institutions and economic activities co-evolve with causality in both directions, and finds that new institutional economists are exaggerating one direction of causality.

Similarly, Ha-Joon Chang (2002) finds that Western countries that are now developed acquired most of their institutions of private property after they had developed economically, thereby directly contradicting AJR's theory. While AJR argue that the reversal of fortune took place from the 18th to early in the 19th century, most developed countries established their intellectual property rights in the early 19th century, and trademark laws were established well into the second half of the 19th century. In fact, Chang *et al.* (2002) find that instead of property being protected, industrial espionage and pouching workers were all a part of a technology policy during the late 18th and early 19th centuries when Western countries were developing.

Notably, although Western governments did not enforce property rights before they developed, their governments made other types of interventions to promote industrial development (Chang 2002). The United States had the second highest industrial tariffs in 1820 and the highest tariff rates from 1875 – 1931. Notably, all developed countries used public support for domestic industry, including trade protection, until their industries became strong enough to compete in conditions of more or less free trade (Wood and Lall 2003). Even today, developed countries pursue active industrial policies to ensure that they retain capacity to produce top-end products, exemplified by the subsidization of the Airbus industry in Europe.

Next, consider the East Asian countries that grew at a miraculous pace in the 20th century. Strikingly, none of them had what AJR dub good institutions, but the states acted as catalysts, by creating environments conducive to development (Rodrik 2011). The governments assured heavy investments in key sectors, such as education, infrastructure and technological upgrading, which made economic development in East Asia in the 1960s and 70s possible. Notably, these countries also enjoyed access to technology, capital and global markets (Amsden 1991).

The evidence presented suggests a problem of reverse causality in AJR's thesis, as private property rights have not generally been a precursor to economic development in Western and East Asian countries. In other words, good institutions may be correlated with high levels of per capita income but not high rates of growth. Instead, history suggests that industries took advantage of the lack of private property laws in order to innovate and grow. This is consistent with Khan (2004) and Austin's (2008) argument that growth and rent seeking have been complementary forces through most countries' development processes. In fact, according to Austin (2008), the market imperfections that produce rents were often the means by which growth was achieved. Similarly, Khan (2004) argues that although secure property rights facilitate exchange in capitalist societies, in developing countries with small capitalist sectors, these policies are largely ineffective, and may hamper growth.

Having explored how advanced countries developed, it is worth also taking a glance at growth processes in today's developing countries, or what AJR call failed nations. While the colonizers could benefit from cheap inputs from its colonies, the colonized did not have this luxury. Although the African continent is often referred to as a “growth tragedy”, Chang (2010), Austin (2008) and Easterly (2006) present evidence to illustrate that Africa has not always been stagnant. Many African countries grew respectably after independence to the mid-1970s. However, with the introduction of structural adjustment programs in the 1980s, which aimed at strengthening private property rights as well as introducing free market policies, African countries' industries collapsed. Notably, the SAPs policies were more focused on institutions in the sense that AJR understands them, and not “enabling institutions” as defined by Bardhan (2006).

Policy recommendations

Now, to perhaps the most important question in the field of development – what policy lessons can be drawn from the RF thesis? Two policy routes immediately become apparent, either reform political institutions to establish property rights for all, or do nothing, because political institutions are deeply rooted in the past anyway. The latter is misleading, as countries have clearly managed to develop economically through history, by carrying out purposeful economic reforms and providing incentives for innovation and businesses. The former is a route tried and failed, as Easterly (2006:60) finds, “…free markets work, but free-market reforms often don't”. An investigation of SAPs being introduced in African countries provides ample evidence of how imposing what AJR call good institutions does not lead to economic development (Easterly 2006, Austin 2008, Chang 2010). What’s more, evidence from developed nations illustrates that enforced property rights has not been a precursor to economic development.

Times are changing, however, and there is less policy space for industrial policies today, compared to the 18th and 19th centuries, and even compared to the 1960s and 70s when the East Asian miracle countries had considerable room to maneuver to pursue national goals such as full employment and growth (Lall 2003). Notably, the current trade regime is a significant constraining factor for development today, as the leverage of governments over the economy is restricted by WTO-rules, bilateral and multilateral agreements and conditionality imposed by the international financial institutions (Mkandawire 2005). Rodrik (2004) suggests, however, that it is possible to get away with defying WTO rules.

For example, China demands local content requirement of foreign companies (similar to Taiwan's industrialization strategy) although this is no longer allowed under WTO rules. As investors are eager to invest in China anyway, they accept it.

Notably, there existed a range of restriction under the old regime too (Chang 2003). Korea often exploited gray areas in the GATT, for example by using the balance-of-payments clause, which allows countries to impose emergency tariffs on the grounds of balance of payments problems. This clause still exists under WTO, and almost all low-income countries can be seen to qualify for this today. Finally, WTO restriction only covers trade-related policies. Thus, countries can still pursue many domestic policies which can be used for infant industry protection purposes, such as subsidies for equipment investment, support for start-up enterprises and subsidies for investment in particular skills.

Conclusion

This paper presents serious flaws both with AJR's theory and their methodology. To critics who accuse AJR of oversimplification, Acemoglu and Robinson (2012) respond that a framework with too many important factors is no framework at all. It is certainly true that a simple framework is more appealing than a complicated one. However, evidence presented illustrates that AJR's framework does not explain differences in economic growth through history, and it does not provide useful answers for countries that seek appropriate tools to achieve economic development.

First, replication of AJR's regressions with improved data shows that a reversal has in fact not taken place, at least not on a global scale. Their method of taking a snap shot of the world in 1500, and comparing it to a snapshot in 2000 hides important developments within the continents of Africa and Latin America.

Second, AJR's idea that private property rights institutions is the main determinant of economic development is incorrect. Evidence presented from countries that have managed to successfully develop illustrates that they developed before private property institutions were introduced. Instead, the state's ability to create an environment conducive to investments has been key, but this requires enabling institutions and a facilitating state, as well as private property rights. Attributing successful development to private property rights obscures the complexity of development processes through history. Notably, AJR's understanding of good institutions fails to capture the type of institutions that existed in Western and East Asian countries before they industrialized. There is clearly a need to revise the mainstream understanding of what “good institutions” are.

Finally, economic development must be understood as a multi-dimensional dynamic process, in which political, institutional, and historical factors all play a role. Countries that have developed have followed their own paths and pursued different policies, but common to most is that they have had institutions that actively enable investment in place. Although this analysis is not as simple, and perhaps not as appealing as the RF thesis, it is more accurate.

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Willi Semmler's Macroeconomics: An Appreciation

By Edward J. Nell

The 25 years from 1948 to 1973 were pretty good for middle class and even poor households in the advanced world. Employment was high, inflation low, productivity grew faster than ever and real wages grew along with it. Innovation was strong and new sectors opened up everywhere, and the public sector was more fully funded than ever before. This was the Keynesian era. Governments – even so-called conservative ones, as in the UK - explicitly adopted Keynesian policies, and supported the development of econometric models that could help to design their interventionist policies. (Roy Harrod was an advisor to Harold Macmillan)

Since then there has been a move away from interventionist policy, and from econometric modeling. The models could not deal with the oil shocks, nor could they handle or explain 'stagflation'. And the political climate changed, first following the oil shocks, then with the election of Reagan and Thatcher. In the resulting confusion the simplicities of Friedmanesque Monetarism took flight and lit up the skies. By the time Keynesians had got their act together to shoot down Monetarism, it had been replaced, or supplemented, by New Classical thinking, together with Real Business Cycles, all draped in Rational Expectations.

What followed was an era that might best be described as ideological argument over how an idealized (and therefore unrealistic) economy should work. New Classicals, New Keynesians, and Real Business Cycle theorists – all taking Rational Expectations seriously – offered accounts of how an economy based on unrealistic, sometimes impossible, assumptions would move towards an equilibrium or end up cycling in an equilibrium path – in spite of the all-too-evident fact that actual economies – the US or the UK – were regularly exhibiting features wholly inconsistent with equilibrium, and behaving in ways contrary to the models. Post Keynesians and the various schools of Marxian and other heterodox economists, however, were simply ignored.

Keynes, however, was not ignored; he was vilified. He was not a real economist, according to Lucas. No real economist under the age of 40 takes Keynes seriously anymore, it was said. As for 'involuntary unemployment', Lucas intoned, speaking *ex cathedra*, 'it was not a fact to be explained, it was a hypothesis, offered to explain a real phenomenon, the variation in actual levels of employment'. There wasn't even a Keynesian problem anymore[1]

So from about 1980 to the Crash of 2007-8 there was a general atmosphere unfriendly to new research in macroeconomics – unless it was designed to show that macro policies were ineffective or damaging. Showing that interventionist policies were 'ineffective' was a particularly common argument, and it was often based on a demonstration that the multiplier was miniscule, or non-existent or offset by monetary developments. And of course, there was the crowding out story, and the effect of an expansionist intervention on interest rates, undermining the expansion.

These issues go back to the beginning of Keynesian analysis, but work on them had largely stagnated, or become repetitive. Willi Semmler and his associates have now developed new methods and new techniques to carry forward the work that had begun early in the Keynesian era, but had become stagnant and unimaginative. For example, they show not only that multipliers are different when the economy is expanding and when it is contracting, they have worked out how to calculate different but related multipliers for different states of the economy generally, and they examine how the multiplier interacts in these different states with monetary and financial variables[2].

This is pretty advanced, but it deals with issues that go back a long way right to the beginning. The story of the Keynesian multiplier can be dated to May 1929; Keynes gave two talks, one in London on May 28, another in Leceister the next day. Both were talks supporting Liberal candidates in the ongoing General Election, and in both cases Keynes was supporting the program of Lloyd George (a program he was largely responsible for writing). This program called for the creation of large scale public works to offer employment, putting the unemployed to work and getting them off the dole. But the Government with the support of the Treasury objected. First, the only way to pay for such a program would be to take away money now financing other projects, so there would be essentially no net gain (crowding out). And secondly, any such effort to expand would

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[1] Lucas at the time of his Nobel Prize, was the only economist to have the word 'macroeconomics' appear in his citation. The great macroeconomists, Hicks, Samuelson, Solow, Tobin and many others, were all cited for other accomplishments, not for their work in macro. For the mainstream establishment real economics is about scarcity, and how can there be scarcity if there is involuntary unemployment? Macro is the wrong way to go.

[2] See Stefan Mittnik and Willi Semmler (2012): "Regime Dependence of the Multiplier , *Journal of Economic Behavior and Organisation*, vol. 83, no 3: 502-522

certainly drive up interest rates. All too familiar … For the liberals to win, these arguments had to be overcome (and essentially the same arguments are out there today…)

Keynes's notes for the talks those two days cover the different ways money can be found or created, so that nothing has to be diverted from financing ongoing projects, and he seeks to show that interest rates do not have to come under pressure to rise. We would certainly rephase or refine his arguments today, but there can be no doubt that he opened the way. Willi and his colleagues offer us not only models, but also empirical methods to demonstrate these points.

Even more remarkable is the fact that Keynes sought to calculate the precise, numerical extent of the expansion in employment as the result of 'secondary' spending: this is the multiplier and it is the first time that we have it as the sum of an infinite series. In his notebook, he wrote out an arithmetical formula for an infinite series that summed to a finite amount. This was the secondary employment that would result from removing an unemployed worker from the dole and putting him to work – thus doubling his spending and setting up the series of respending. Moreover he then calculated the savings from the dole, as secondary workers were taken off it and put to work– this savings could be part of the financing of the public works.

Keynes considers several different possibilites of leakages, essentially different states of the economy. In other writings, and in a letter to Harrod he suggests that these conditions might be stable for long enough to study, but that over time they would most likely change, though slowly. He recommended empirical studies of the multiplier in marked contrast to his reaction to Jan Tinbergen's early econometric modeling, which he criticized very strongly at about the same time.

Keynes's views on the likelihood that the multiplier might vary in value with the state of the economy seems to be an early version of the argument that James Duesenberry presented in the 1950s and 60s. He proposed that the multiplier / accelerator mechanism in the upswing of the cycle would be different from that in the downswing, and every cycle would very likely differ in some ways from every other. Duesenberry asserts this but doesn't demonstrate it, nor does he present much evidence. But he makes a plausible case for looking into it.

This is what Willi and his co-authors have done. They have not only studied and answered these questions, but they have developed empirical methods of analysis to make their studies precise and workable. They have moved the Keynesian project forward after decades of stagnation.

Humbug and the Cambridge - Cambridge Controversies in Capital Theory [1]

By *G.C. Harcourt*

Anwar and I have been friends since the publication in *JEL* 1969 of "Some Cambridge controversies in the theory of capital". The survey influenced, I understand, Anwar's 1973 Ph.D dissertation at Columbia, "Theories of value and theories of distribution" and his 1974 Humbug article was based on a chapter in his dissertation. Here I concentrate on his 1974 *Review of Economics and Statistics* paper, Bob Solow's response to it, Solow (1974), and Anwar's development of the original ideas and his response to Solow's criticism in Humbug II, Shaikh (1980).

Apart from the technical elegance and ingenuity of Anwar's analysis, he was one of the first participants in the capital theory controversies to put the technical analysis within their proper conceptual setting. Anwar made explicit that a fundamental issue at stake was the 'vision' of the processes at work in capitalism, of how accumulation and profits arose and were related. Anwar implied immediately (and explicitly in Humbug II) that scarcity and choice in an exchange system transferred to the sphere of production underlie both the theory and empirics of Solow's response and the practice, then and now, of the mainstream generally. J.B. Clark's theory of distribution and Irving Fisher's consumer queen drive the action through her aim to maximise her lifetime expected utility, with all other actors in the economy being but the agents to allow her to achieve this. Whereas Anwar has the alternative vision of the classical political economists and Marx, of ruthless swash-buckling capitalists (all three sub-classes) producing and accumulating, with all the other actors dancing to their tune.

That increases over time in output per person and per hour at the level of the firm, the industry and the economy are the outcome of both "more" and "better" capital per person are technical facts of life which economists of all persuasion accept. Neoclassical

[1] In this note I have drawn on my paper, "The importance of HUMBUG in the Cambridge - Cambridge controversies in capital theory", published in the Global and Local Economic Review in 2013.

economists further argue that the effects of "deepening" and "bettering" are separable, at least in principle (this is of the conceptual basis of Solow's 1956 and 1957 articles). In contrast, Post-Keynesians ultimately came to argue that they were not, that the factors associated with accumulation bringing about the rise in output per person through embodiment were indissolubly mixed, see Kaldor (1957), Joan Robinson (1971). It is how the above underlying technical structure is married to the processes of accumulation and distribution that creates the impassable cleavage between the two sides.

Solow (1957) had set out an ingenious way in which to precipitate out the deepening function from the overall relationship between output per person and capital per person which contained both it and the impact of technical progress in the neoclassical version of Harrod's natural rate of growth, Harrod (1939). Solow covered himself by writing that if it were assumed that the time series data used were viewed *as if* they had come from a production function in which, under competitive conditions, factor prices were equal to their respective marginal products and which was subject to the impact of neutral technical progress which raised the whole function over time, he had devised a simple way to fit statistically a function to the points so precipitated out. As we know it was a Cobb-Douglas.

Anwar's criticism was to show that the function that was fitted was an algebraic identity – a law of algebra – in which regardless of how the values of the various variables were created – what processes were responsible for them – GNI would always be identically equal to the share of wages plus the share of profits. Solow's methods and results could neither refute nor confirm that a Cobb-Douglas production function was the originator of what was observed in the data.

Anwar's procedure was to show how a time series spelling out HUMBUG gave the same result – a very good fit of a Cobb-Douglas – as did Solow's adjusted data. Solow's answer was that "Mr Shaikh's article [so much for a Ph.D from Columbia when viewed from MIT] [was] based on misconception pure and simple." (121).

Anwar joined and was joined by economists from both camps, as it were. For example, Franklin Fisher (1971) carried out a huge simulation exercise in which he showed that if factor shares in the GNI were constant over 'time', a Cobb-Douglas function fitted well even though the conditions for aggregation from individual firms' Cobb-Douglas functions to the economy as a whole were ridiculously restrictive and demanding. The fit occurred because the shares were constant, not because a Cobb-Douglas was producing the observed statistics. Henry Phelps Brown (1957) had already discussed the short-comings of the Cobb-Douglas associated with Paul Douglas's seminal work but his setting out of the critique was rather obscure and was neglected in the literature. Herb Simon also made the same critique but again did not have an immediate impact, see Simon and Levy (1963). The person who has stuck most tenaciously to the task of propagating and developing Anwar's insights is John McCombie, more recently in the company of Jesus Felipe see, for example, Felipe and McCombie (2013). (Felipe has also collaborated with Fisher.) Despite all this continued and damning criticism, the mainstream goes merrily on its way, using Cobb-Douglas or its sophisticated cousins, for example, CES, in both modern macroeconomic analysis and in endogenous growth theory.

Anwar (1980), 93, points out that Solow tried to have his cake and eat it too." Having … said that his method … [led] him to conclude that even the Humbug economy is neoclassical, [he] next asserts the very opposite … he runs a [linear] regression … on the Humbug data [that] gives a very poor fit [and] a negative coefficient for his *k*. [Anwar argues] that linearity is … a convenient assumption whose applicability must be … *justified*, not … assumed." (emphasis in original).

Even if Cobb-Douglas described the behaviour of individual firms this does not logically imply that similar processes occur systemically, that is to say, a systemic theory of distribution does not have to match or reflect the processes at work at the level of the individual firm. The best illustration of this proposition comes from Kalecki's remarkable 1936 review article of *The General Theory*, then untranslated from Polish[2]. In it, Kalecki starts with a profit-maximising, cost-minimising firm, the production technique of which could well be Cobb-Douglas, situated in either a purely (freely) competitive or an imperfectly competitive market. He nets out raw material costs and splits the value added implied by the net revenue and net cost curves into wage payments and surplus (=profits); he aggregates the values added of all firms in the economy to the economy as a whole and shows how wage-earners spending what they earn and profit-receivers receiving what they spend, given the level of investment expenditure, results in the overall levels of activity and employment, and the distribution of income between wages and profits, being determined simultaneously.

This two-sided relationship between accumulation and distribution was extended by Joan Robinson to the long period (in a Harrodian sense) in 1962 in her banana diagram (Joan Robinson (1962), 48), and even further by Donald Harris (1975, 1978), who takes in the sphere of production in which the potential surplus is created as a result of the impact of

[2] The first full English translation was published in the December 1982 issue of *Australian Economic Papers,* see Targetti and Kinda Hass (1982).

the current state of the class war and the existing technical conditions of production. The realisation problem is analysed in the accompanying sphere of distribution and exchange in which the Keynesian “animal spirits” function and the Cambridge saving function interact to determine the rate of accumulation and the distribution of income and so how much of the potential surplus is realised.

An essential part of setting up this alternative approach is Anwar’s critique of Solow’s methodology, of his theory and its application, and Anwar’s recognition of the link between ‘vision’ and the specifics of theory, analysis and applied work. Mainstream analysis of firms’ behaviour by no means implies that the system need mirror it. Anwar’s contribution also puts paid to the late Charles Ferguson’s, Ferguson (1969), and the late Mark Blaug’s claims, Blaug (1974), that econometrics would decide how serious for neoclassical theory would be the results of the Cambridge – Cambridge capital theory controversies. Joan Robinson further refuted the claim that econometrics could ride to the rescue by her repeated demonstration that comparisons of equilibrium positions (differences) cannot throw light on processes (changes), see, for example, Shaikh (1980), 115, n2. The hegemony and ignorance of the mainstream keeps these findings at bay but surely truth will ultimately prevail. If, when, it does Anwar’s contributions will be a reason why.

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Who's The Humbug After All These Years?

By L. Randall Wray

Cambridge economists destroyed Neoclassical capital and distribution theory sixty years ago.[1] Unfazed, proponents argued that even if there is no theoretical basis for aggregate production functions, they are useful if they "work" empirically. Using a Cobb-Douglas production function, regressions found a fairly good fit to the data and appeared to confirm the marginal productivity theory of distribution. While some were puzzled by the seemingly too-good-to-be-true empirical confirmation of a theory with shaky foundations, Anwar Shaikh demonstrated forty years ago that "so long as aggregate shares are constant, an aggregate Cobb-Douglas function having apparently 'constant returns to scale' will always provide an exact fit, for any data whatsoever" and "will seem to also possess 'marginal products equal to respective factor rewards'". Any data! From any kind of economy, including a Humbug economy! The results are guaranteed not due to laws of economics but rather due to laws of algebra.

In a true Humbug[2] response, Robert Solow replied to Anwar as follows:

> The factor-share device of my 1957 article is in no sense a *test* of aggregate production functions or marginal productivity or anything else. It merely shows how one goes about interpreting given time series if one starts by *assuming* that they were generated from a production function and that the competitive marginal-product relations apply.
>
> [Solow 1974: 121, emphasis in original]

To paraphrase, he is saying about his famous 1957 article: Well, I was not testing anything, I was just showing how to interpret empirical evidence if one starts by assuming the data

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[1] For a good recap, see: P. Garegnani, 1970 "Heterogeneous capital, the production function and the theory of distribution, *Review of Economic Studies,* 37 (July), 407-436.

[2] Here, the use of the term Humbug is consistent with Anwar's use and with the Definition of Humbug from Merriam-Webster: 1 a: something designed to deceive and mislead; b: a willfully false, deceptive, or insincere person; 2: an attitude or spirit of pretense and deception; and 3: nonsense, drivel.

were generated from a good Neoclassical production function. This is in response to Anwar's proof that any production function would fit!

It is hard to know what to make of such an evasive response. Neoclassical theory is a Humbug. Neoclassical empirical work is a Humbug. And Solow is a Humbug for trying to interpret empirical evidence using a theory known to be Humbug. And so are all the other Neoclassical macro theorists who still use Cobb-Douglas as the basis for virtually all modeling and estimation. They are fraudsters, hucksters, snake-oil salesmen. And the funny thing is that most of them do not even know it. As Scott Carter hilariously points out, Greg Mankiw defends Solow by arguing: "I have always found the high R2 reassuring when I teach the Solow growth model. Surely, a low R2 in this regression would have shaken my faith…"(Mankiw in Felipe and Holz 2001: 281). What can you say about someone who's been Humbugged by faith?

What we do know is that Anwar is not a Humbug. Over the past quarter century I've drawn inspiration from his work and from his character. Hyman Minsky (who, by the way, estimated Cobb-Douglas production functions for Douglas as a New Deal jobs program worker!) handed me an early version of Anwar's paper on effective demand in Marx, Keynes and Kalecki, which helped to synthesize in my own head the approaches of Keynes and Marx. I was always impressed by Anwar's dogged determination to demonstrate the usefulness of the labor theory of value—not only theoretically but also empirically. And I was even more impressed by his courage when he took the stage at a conference after a series of remarks by heterodox brethren who were trying to demonstrate their distaste for all things communistic. Anwar chided them by saying that they wouldn't dare say such things if Marx and Engels were present! But, as always, Anwar delivered that assessment in the most polite and respectful manner possible.

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Proceedings of the ESU Colloquium:

The State of Worldly Philosophy at The New School

February 20th 2014 at The New School in New York

Past Visions of New School Economics

*David Howell: An Alumnus' Reflections**

Transcription: Ian Howland

Thanks to you [Isabella Weber] and Gregor [Semieniuk] and the rest of the organizing committee for putting this together. It is a very timely event to have and thanks very much for giving me an opportunity to participate.

It seems utterly unreal—surreal—for me to be sitting here, participating in this session in 2014 with two of my professors who I came here to be taught [by] and who tried to teach me macro and trade. I was in one of those first ten courses for trade and political economy almost 40 years ago and I feel like I am still a student—I think we all are—but especially this morning.

Thanks to Mark [Larrimore], whose history was fantastic, and Ed [Nell] on the '30s and '40s, and even quite a bit of what Anwar [Shaikh] had to say either brought back good memories or triggered old memories, some not so great. Anyway, I have learned a huge amount just listening to the three of them and I think my concerns will overlap with some of Gregor's points and Isabella's questions that she posed to us. As Anwar did, I want to start with a quick introduction. More than Anwar or Ed, many of you probably do not know me or have no idea why I am up here, so this will hopefully give you a little context for the remarks I will make about my vision of what the department was, why my generation of economics students came here, and how, as a faculty member in another division of this school, I remained closely affiliated with [this department] and with the Schwartz Center for Economic Policy Analysis through Lance Taylor, Will Milberg, and now with Theresa [Ghilarducci]. I want to talk about how I see the evolution of the department and what I see as the crucial issues that face it.

I came to the department in the fall of ’74, just back from a full year traveling around Europe in a VW van—not such a whirlwind tour because the maximum speed was 65 KPH so I was basically driving on the side of the road from Amsterdam to Istanbul and then back to London. I showed up here that Fall, having applied via the post office in Istanbul in January the year before, full of enthusiasm to continue the kind of coursework I had taken at NYU in ’72 and ’73 on Polanyi, Marx, and Schumpeter in courses taught by two guys, James Becker and Bertell Ollman who, with Ed, created a wonderful game called “Class Struggle,” which unfortunately did not seem to make it in our market—it should have. I showed up, like many of us in the old days, perhaps not like many of you, having never had much economics or math in college, arriving un-matriculated. The New School gave me that opportunity. I had a year to show that I could get through the courses. The courses I took that year were by David Gordon (two courses), Anwar Shaikh (two), Ed Nell, and [Robert] Heilbroner. I never came here with the expectation of a career in economics and I would not have [pursued one] if Gordon had not given me a great grade and some comments on a paper that I did on enclosures in that Socioeconomics 1 course that I took my first semester. [This] was a paper that was a critique of (of all people) Bill Lazonik’s *Radical Political Economy* piece on enclosures. I worked the whole semester on it and I still remember walking down Fifth Avenue after getting the paper back form David and just grinning ear to ear all the way back home. It was really the following summer, when he invited me to be one of five employees at his Institute for Labor Education and Research (whose main mission was developing political economy courses and teaching them to labor union locals in the metropolitan area) that led me to decide to come back in my second year. I matriculated and economics was not what I really wanted to do but this kind of political work made a huge difference and I ended up here because of it. There was a history [of working with unions at The New School] before David’s labor institute that goes back to the ‘30s and before [with Charles Beard and Alvin Johnson], which I had no idea about.

[Working for the Institute for Labor Education and Research] was particularly an exciting offer since Ron Blackwell, my student advisor during that first year and one of the smartest and most politically committed and honest people I have ever known, had also agreed to join the institute. A couple of years before I arrived in the fall of ‘74, Ron showed up here coming from the federal prison in Georgia because he refused to serve in Vietnam and spent several years there as a librarian, teaching Black Panthers Marx. That is what I learned my first day walking onto the economics department. He was the first person I ran into and, by the end of that conversation, I had this history—I was just completely blown away. That was in Heilbroner's office, which he was borrowing. Ron played a big role in my intellectual social life here and, I think, in the department's in the middle '70s.

Why do these details matter? Perhaps they don't a lot but I want to illustrate how much has changed in 40 years. The huge change in social, political, and intellectual context needs to be recognized when you listen to us old folks talking about our visions of how it used to be. You heard a lot about the Columbia strike. That was a setting which we lived in and came up through that you [current students] do not have and it is a different world in some respects. How could you take off for a full year and drive around in a van in Europe today? None of my kid's friends would even dream of doing that and she is about the same age that we were when we did it then. How could you enter an economics program now that is at all serious without having to take the GRE, without having had more than one economics course, without having any math? It was a really special place.

The economics department of the early and mid 1970's reflected a time of intense turmoil and conflict illustrated in that pamphlet [Snyder, Brian, (1985), Orientation Pamphlet for students of the Graduate Department of Economics,] very vividly and it was a time of student empowerment as also comes through in that pamphlet. Anwar and I joked yesterday on email about the pamphlet: that it underscores the sense that, if there is one constant you can rely on in this place, it is that the department always seems in the midst or on the verge of crisis. We study recurring economic crises and we seem to experience persistent institutional crises. I will have more to say on that but I also want to be clear that this kind of colloquium, this event, can make a huge difference. Students and faculty working together can make a big difference. [For example], we finally got fed up with Bob Kerry and, although his [version of TNS] history was that he took responsibility for the creation of the faculty senate, that was not the case; we formed the faculty senate because we did not like what he was doing to this institution. That was fortuitous because six or seven years later, it was the senate that was the institutional organizing force that allowed us to have a vote of no confidence and he was out eighteen months later. The students, shortly after we formed our senate, formed theirs: the student senate.

So, my conclusion is going to be that this department needs to settle on its niche, it needs to double the size of its faculty, it has to think about how to do that based on the recommendations that Anwar just made, and it needs to organize and fight for it in a constructive way and not the way that some of the fights were done in my years in the '70s and early '80s as it was described in the pamphlet -- which were very destructive in many respects. The factionalism, the people leaving classrooms in tears—that is not helpful. My sense from the people who I have met here who organized this is that you have an amazing opportunity given the quality of your leadership.

Why did we come to The New School in the '70s? It should already be clear: we were political, hungry for how to think about changing things, and it was at the end of the American Golden Age and a good job would be there, we thought, when we needed it. We could demonstrate and we could go to places like The New School because we would get a job. To say the least, this is a far cry from the pervasive insecurities students face today and I give you all the more credit for doing that in this period of insecurity. I want to say that this was not just the case for suburban kids like me. A lot of my classmates had bona fide, genuine working class credentials—Tom Michl for one had come out of several years of working at a steel factory to The New School and he ends up writing a book with Duncan Foley. Most of us did not know anything about the turmoil—even after we had been here for a year or two—that is described in the pamphlet but what we saw was an incredibly exciting, intellectual, and political place. In my second semester here I took a course on Marx with Paul [Sweezy]. Where else could you do such a thing? Along with seemingly perpetual crises, there have been other constants and I want to focus the rest of the little time I have left on six of them very briefly.

The second constant is that the faculty has always been tiny. I am going to differ a bit with Anwar on that; I do not think it was ever ten or thirteen and I am going to give you some statistics on that. I am also going to differ with Anwar in that I graduated here with $17,000 dollars in debt in 1982 [which would be 3-4 times that in today's dollars] and it was not until the mid-1990s that I worked off that debt. This place has always been expensive and you have to really want it. I run a doctoral program [in Public and Urban Policy at The New School] and this is the biggest issue I have: we get fantastic students there but our yield would be about 90 percent and not 60 percent [which is still excellent] if we could provide even half of what our competitors are providing in terms of scholarship. At what might be seen as the peak of the political economy program in 1979, just before the state review, there were exactly seven regular full-time faculty—two of them who are sitting here, together with three multi-year visiting faculty. Fifteen years later, according to the 1994-1995 catalogue, there were still just 10 full-time faculty. Today, I count something like nine with one on extended leave and another sitting in the dean's office leaving something like seven. So, if anything, it is smaller now than it has been since 1970. A range of seven to ten faculty is simply too small.

The third constant is that, since 1980, the department has operated like the New York Yankees: buying superstars, older superstars. I love them all but I do not think it is a strategy that works in the long run. Take a look at the roster in the bulletins, nearly all the faculty have been older full professors—white guys mostly—and this was not the case when Rick McGahey, Bob Pollin, and I started here. The faculty was much younger—Gordon, Shaikh, Heidi Hartman, Gausman, Ross Thompson, Gita Sen, Nancy Folbre—they were all starting out when we were here in ‘74, ‘75, ‘76. Nell had been here five years and even Heilbroner was just middle-age and quite young from my perspective now. Look at the photos in the bulletins; look at Anwar’s photo. This changed dramatically after the 1970’s. This department has been wildly unsuccessful at hiring good, young professors and even less successful at keeping them. Yes, Anwar is right: a lot of incredible people have walked through these doors and taught us but they do it for a semester, they do it for a year even when they are hired for a year or two- or three-year appointments or even tenure track, they don’t stay. There has been a dysfunction at the heart of the way the program is managed, the way that graduate faculty was managed, the way students interacted with the faculty—I do not have the answers but the product has been a failure to grow our own Derek Jeters after the 1970’s, when we were tremendously successful at doing that. In 1994-1995 there were 8 full professors all over 50, no associate professors, and just two assistant professors: Milberg and one who was denied—in my view, inexplicably—for tenure, Tom Palley. You can blame the university for not allocating enough faculty lines but, as an organization my cross-the-street, bird’s-eye-view is that the record of this department’s [hiring] is getting worse and retention for [professors] has not been as good as it should be. We need to do better.

The fourth constant is that, for all the talk in this radical department, the failure to hire and retain female faculty and faculty of color is rather astonishing. The orientation pamphlet inadvertently tells the story. How did we lose so many women in the late 1970’s?

The fifth [constant] is the trajectory of the department since the 1980’s, the curriculum has been increasingly narrow and mathematical. Maybe not blackboard economics but [the curriculum now] certainly would not have suited us in the 1970’s. Why did we come in the ‘70s? In 1979 the theory courses focused on Marx, the Cambridge Controversies, the paradigms of Kuhn, critiques by Lucas. There was the labor theory of value seminar by Shaikh, the theory and practice of market power by Ed Nell, worker control and worker management by Gordon, every year these were changing. It was incredibly innovative and different. In the late 1970’s there were no fewer than seven regularly offered history of economic thought courses and eight regularly offered courses in economic history. Think about it: 15 courses offered on thought and history. How does that compare to the offerings today? For us, courses like these were very cool but it is true, on the other side, that we were not particularly well prepared by this curriculum for mainstream academic jobs. There were opportunity costs—there are opportunity costs. And this is amplified by the smallness of the department. We had to do a lot of learning of standard economics outside of the class and this has probably hurt a lot of our graduates in the job market but the breadth and intellectual excitement of The New School's economics department has also been invaluable to the careers and lives of many of us who started in the '70s and '80s.

And this is my last constant. This department, to end on a positive note, has somehow, despite its tiny size and perpetual crises and hiring struggles, steadily drawn fantastic students and produced amazing researchers, policy analysts, advocates, and teachers. Among my classmates and my circle alone there's Bob Pollin at UMass [Amherst]; Tom Michl at Colgate; Ron Blackwell who ended up at the top of the AFL-CIO; Mark Levinson (also hugely successful in the union movement); Rick McGahey, who has had all kinds of top government research and foundation positions; Mark Levitan, who transformed the way we measure poverty who has worked out of the mayor's office. There were many more in following generations: Heather Boushay, who will be here this afternoon, at CAP and now running the Washington Center for Equitable Growth, which I am very happy to be a part of; Josh Bivens, research director of EPI; Jeanine Berg and David Kucera at the ILO—this was Gordon and Milberg's orbit. The list would be way longer if we included Anwar's, Lance [Taylor]'s, Duncan [Foley]'s, Ed's and Willi [Semmler]'s students. The program has drawn and then produced a fantastic group of people who are helping to make progressive change all over the world. Imagine what could have happened if we had been given those six additional lines that the state review committee demanded back in 1980.

This is my conclusion: the great news is that, despite the steady crisis, there has been persistent success in this regard. Based on what I have had the pleasure to teach, my experience in the last few years teaching economics in this department, this place is still attracting remarkably talented students and more, I think, from abroad than ever before.

I am concerned about the transition from 15 economic history and thought courses in 1979 to the current mix, in which there are just a handful of thought and rarely an economic history course. And I am concerned about the roster of courses which, from the titles alone, appear remarkably conventional, very different from those in the '70s: Advanced Micro I, Advanced Micro II, Advanced Macro—these are not the same titles. How are they different (from mainstream economics course titles)? When someone peruses [the course catalog], how would they know that these are distinctive? I am concerned finally when I see the syllabus of a required micro course that looks as conventional and uncritical as any of those I see at NYU or Harvard. I read the 1985 orientation pamphlet about a proposal for a course or concentration in the political economy of empirical research. I had never heard of that. I was here and I never knew that was going on. That would be something innovative to think about.

When the top schools have 30-50 full-time faculty, our seven to nine just can't compete in the long-run, so we need to double our faculty, we need to nail a very clear niche. One not tailored to my generation in the 1970s but to yours. This will be controversial and hard but we took down a president; it can be done. It is about figuring out how and organizing and confronting the university and it is about running the department well. It is about good management. And it needs to be to keep people from leaving, especially women and faculty of color. It needs to be a good, safe, supportive workplace for a diverse faculty where faculty do not leave classrooms in tears. So part of the power structure here – and the solution - is in the dean's office. We had, as usual, a great introduction by my old friend and colleague, Will Millberg. He is now the NSSR Dean, so there is no time like now to make a major change. I hope that you can follow up this colloquium and I, and I am sure Ed and Anwar, will do everything we can to help with this. Thanks a lot.

Mark Larrimore: Mythologizing and Demythologizing the History of the New School for Social Research

Transcription: Kendall Stephenson

I'm glad to be here. I'm not one of you (I'm sort of aware of representing 94 years of non-economics New School History and the present) but am here rather to learn from you. I'm not a historian either; I'm in religious studies. But I co-teach a course with Julia Foulkes, who is a historian, on the history of the New School. We've done two iterations of it and have found it to be a very fruitful subject for many different reasons. We are interested in it from a history of education perspective but it turns out to be tremendously important for an institution that is constantly forgetting itself, then rediscovering itself, and reinventing itself, and then forgetting itself again, and so on. I'm in religious studies, so when Julia and I do stuff together, I tend to tell the big stories and she tends to take them apart. She'll say, 'Well, yes, but there was also this, and this.' She would be here if she could and she sends her regrets. I'm going to try and be both of us.

What I've undertaken to do in my fifteen minutes is talk a little bit about mythologizing and de-mythologizing the history of The New School. What I'll do is mention a number of very common myths about the New School as a whole. I won't spend a long time taking them apart, I'll just sort of prick each of them once and you can watch it collapse. Then I'll try and offer you some other stuff – especially from the first 25 years, even the first 15 years of the school – which we found to be very helpful for fostering new thinking about directions in which The New School as a whole might go. This kind of thinking has been very helpful for various other projects. When gender studies, after having forgotten itself, reconstituted itself a few years ago, it was a wonderful thing that we were able to bring to the directors of the renascent gender studies program an entirely forgotten history about women leaders of this institution.

So, five myths.

The first one: 'John Dewey and Hannah Arendt had a baby and it was called The New School.' Not true for all sorts of reasons. John Dewey was here very briefly, we mention him a lot, but he was at a lot of other places too. We are inspired by him, but lots of other places are inspired by him. He took a furlough from Columbia to come here, he didn't leave there. Hannah Arendt arrived here in 1967, when she had already written almost all of her most important work. Her arriving here was the confirmation of an affinity rather than helping to build that culture. I'm trained as a philosopher, and it pains me a little to say this, but we were not set up by philosophers.

Second myth: 'we are a pacifist and leftist school, or were founded by pacifists and leftists.' We think pacifists because of the pacifists at Columbia who were fired and in response to which historians from Columbia started the New School. But the historians who started the New School were not pacifists. They objected to any kind of restriction on political expression by professors. And certainly the University in Exile was not pacifist. Leftist: You are, some of us are, but it would be more interesting to understand the history of The New School in terms of more time-appropriate phrases like progressivist, in the meanings it had in the first two decades of the century, which is quite different than what we mean by leftist. Liberal, anti-totalitarian, radical, never just leftist.

Related to that, myth number three: 'The Frankfurt School came here.' It didn't. It went to Columbia. Deeply embarrassing, people think that, continue to think that, and people continue to say it and preach it because we are two institutions that are *called* institutions of "social research." Keep that phrase in mind because figuring out what the words social research meant to the founders of the New School (since it *wasn't* the Frankfurt Institut für

Sozialforschung that they had in mind) is one of those things that we're trying to understand; I suspect there might be knowledge in this room that can help us answer it.

Fourth myth: 'we were meant to be a research university in social science.' We were not meant to be a *university*. The founders didn't think that Columbia was a bad university, and they wanted a better one; they thought that Columbia was about as good as a university could get, and yet even a university at its best turned out to be a very conservative institution. It was run by trustees, there was concern with the shaping of the minds of young leaders, and in various ways, it was not open to and not flexible enough to respond to the demands of the day.

Final myth: 'The New School started in 1919 collapsed and was then rescued by the University in Exile in 1933.' This is a narrative you'll find in the two books about the New School – the sense that the founders wanted the school to be a place basically giving graduate degrees in the social sciences. They ran out of money in 1921, and somehow kept the dream alive secretly for the next dozen years. In the meantime they provided a setting for anybody to teach anything they wanted just to keep the thing going. Then happily. By some strange unintended or perhaps intended consequence, the money that was brought in for the University in Exile allowed The New School to connect once again to the original dream of being a research institution. This is a little bit unfair to the University in Exile. It's strange for us as an institution to feel *ourselves* saved by it, but it's also unfair to the history of The New School and actually even to us here, as even the study of economics goes back beyond 1933. One of the things I hope to give you is some information about why it is that you should tell people the longer story about yourselves.

So, those are my five myths. Now let me give you some material for trying to move beyond them. Not for each of the five; I'll do the first, second and third together, and then the fourth and the fifth.

Who was The New School really founded by? Historians. But what was a historian in 1918 or 1919? What was a social scientist at that point? At that time, if you wanted to get a PhD in the social sciences, you would probably have to go to a land grant university. If you were a woman or a Jew and wanted to get a PhD in these fields, you'd go to one of those places, and you'd never find a job anywhere. So, starting a 'new school of social science for men and women,' the name of the original proposal, was quite a radical thing to be doing.

In fact, the school was founded by people who were in history departments but were essentially economists. The two main historians who resigned from Columbia were looking for a pretext to resign. They were sick and tired of being at Columbia. They were big people, they were too big for Columbia. They thought they were, at least. One of them was Charles Beard who in 1913 wrote a very influential book called *An Economic Interpretation of the American Constitution*. As Beard talked about what history was, it was clearly economic history, or bringing the economic dimensions into an understanding of the history of political institutions. Beard and James Harvey Robinson, the historians who founded The New School, were champions of something called the 'new history.' The 'new history,' as it understood itself, was concerned with the concerns of the moment and in particular was very interested in nascent political, economic, and social scientific fields. Beard was a very important historian and economist. Thorstein Veblen obviously was an economist too. Alvin Johnson, an agricultural economist from Nebraska, found his way here through a circuitous route, but the school, which didn't want to have a president, and then found that it couldn't live without a president, was maintained by this president who was an economist. Alvin Johnson's name comes up over and over again, and justly so (although he didn't do all the things he got credit for, a woman names Clara Mayer did a lot of those things), he helped the institution survive. Nobody actually thought The New School was going to live beyond ten years. You might know that it started out in two adjacent brownstones in what is now London Terrace in Chelsea, between Ninth and Tenth Avenue between 23rd and 24th street. They had a lease for ten years, and nobody thought the New School could last beyond that. The fact that it did is a credit to Alvin Johnson and it probably didn't hurt they he actually knew how to think as an economist. So, who were we founded by? Social scientists, but as part of a landscape that is quite different from the understanding of what social science is today.

Let me say a few more words about not wanting to be a university. Veblen and Robinson were only some of the people that came here who had written books in the first years of the 20th century critiquing the institution of the university. We tend to tell a story in which there was a very local problem having to do with WWI and Columbia, but in fact there was a larger problem that Veblen and others had identified before that about universities as actually unhelpful institutions. As repositories for old knowledge, that actually slowed the progress of society and prevented people from recognizing new constellations of social, intellectual, and cultural life. So they did not want to form a better university; they wanted a non-university. That is why it was called a *school*. What Julia and I have found to be the best way to explain how different what they intended was is to say that The New School was not a new and better university; it was what would happen if you took a magazine like *The New Republic* and turned it into a school. We don't mention *The New Republic* by accident. All of the founders of The New School were involved with *The New Republic*.

The idea of The New School, which was already being bruited long before the crisis in Columbia around the pacifist professors, was to set up a kind of institution of wider learning for adult learners. It would not be giving degrees, and would not be beholden to academic disciplines (which are beholden to universities, which are very conservative institutions), but would have the agility to be innovative, to respond to the needs of the day, to talk about things that the other universities were neither equipped nor interested in dealing with, like the new economic and political world after the end of WWI. What was the League of Nations? What were the economic consequences of reparations and things after that? These were some of the things that The New School was originally going to be about. Others included labor, about women entering the workforce, and other sorts of issues like that. So: thinking about *The New Republic* as creating a community, and then thinking about what it would be to put that community on the map as a place where people could learn more and teach each other: this has turned out for us to be a much more helpful way of explaining the initial ideal of what The New School might be. We'd be very interested in your thoughts about it.

There were a lot of other institutions than universities conducting research. If you look at the social sciences in particular there were places with names like the Bureau of Municipal Research (of which Beard was the director), the Bureau of Industrial Research, the Eugenics Research Office of the Carnegie Institute, the Bureau of Vocational Information. A lot of these ended up consolidating with universities but that was not their purpose. Partly that's because universities didn't want to have them. But at that point, the universities were very theoretical; they were very focused on theoretical academic backward-looking things and not concerned especially with questions of policy, social welfare, social research, finding out what the rates of poverty were for example. Lots of research was done by these institutions and all these other people, most of them PhD's from land grant universities. This is a history that many of you probably know better than me. The one thing we are *not* like that we'd perhaps like to have been like was the Rand Institute for Social Research. They were the real leftists, that wasn't us. But they were part of a similar moment when people were trying to set up a different kind of institution because the university didn't seem like it was worth working with. The university was too unwieldy. The universities at that stage were very much old boys clubs and closed to some of the most important and interesting thinkers.

You may or may not know that the wonderful noble idea of The New School practically collapsed within about three years. I call this the 'seven semester itch.' The various people who got together in the office of *The New Republic* and dreamed of what a better institution would look like had very, very different ideas of what it might be. They fought with each other tooth-and-nail about it. Beard wanted a particularly strong connection to labor unions. He really wanted to develop not only courses specifically for members of labor unions, but also to become home to a publishing house that would produce introductions to social scientific knowledge specifically for workers. Kind of a nice idea, but it never happened. For various different reasons it didn’t happen. Beard left, and by about 1922 all of the original founders had left The New School. Alvin Johnson stayed, though. Horace Kallen was right there from the beginning, a very interesting philosopher, was there for about 70 years in his career, an amazing person, but the original founders left. Then we entered – what in the official history becomes– a dark, murky, out-to-sea period until 1933 when suddenly the lights come back on again.

What really happened in those next twelve years is actually very, very interesting. Thank you, Isabella, for giving us the updated university vision according to which we actually intended to teach philosophy, jazz and all these other cutting-edge subjects. Not quite. We intended to teach whatever needed to be taught that was not being taught elsewhere. The number of initiatives that have been started and failed at The New School is so great that it’s actually wrong to say that they failed. It’s really more a kind of entrepreneurial thing; it’s really very capitalist in some way. We’ll provide a setting where people can set up anything they think might fly. How did the New Museum come out of the New School? The New School provided a place to work when nobody else did. That’s the kind of thing the New School did. It also provided refuge to a whole bunch of other people we may not have heard of because they failed. Part of what made the New School a really interesting place was its openness to experimentation of many different kinds. Allowing for such experimentation also paid the bills. The original concept, as you may know, was that it should have no trustees, no endowment; it should be driven by tuition revenue, which meant that it would be forced to remain relevant by teaching courses that people would be willing to pay for. The idea was, at that point, that there were various kinds of things that people needed to know that weren’t available in other places, so we would offer them.

By the end of the 1920’s, the New School for Social Research had become a major center of the arts. It was also the major center for teaching lay people about psychoanalysis. Urban studies sort of starts here in some interesting ways, too. Yes, Berenice Abbott was teaching photography here before anybody else did, but this was part of a broader efflorescence of the modern arts at the New School, something that wasn’t happening at universities. They weren’t interested in modern arts, and there weren’t other institutions that supported this. When people wanted to learn about modern dance, or music, or literature, or art, or architecture, or theater, they came to The New School. And many of the books that taught Americans about these arts – like John Martin's *The Modern Dance*, Aaron Copland's *How to Listen to Music*, Lewis Mumford's *Sticks and Stones* and Abbott's *Guide to Better Photography* – came straight out of courses taught at The New School.

Updated university vision aside, you will often hear a rather derogatory way of talking about this period. It's almost as if the New School really should have been doing social science. And then for a bunch of years, because there wasn't any money, they rented out their space to whoever wanted whatever. They did that until there was enough money to get back to the mission of doing research in social science, which, conveniently, the University in Exile provided. I find that story offensive but it's also intellectually regrettable because it makes us not take seriously what it was that made the modern arts come to The New School. It's not just that there was nowhere else for those places to go.

Let me go back to the question about what 'social research' means. The original proposal for a New School in 1918 didn't use the phrase 'social research' but 'social science.' Somehow between 1918 and 1919 the name of the school changed from New School of Social Science to the New School for Social Research. The Holy Grail for us would be to find out how that decision was made and what it meant. There were a few other places that used the phrase 'social research' at that stage, and they tended to be research-oriented and policy-focused institutions. They tended not to be teaching, though some of them did. The Frankfurt Institute for Social Research hadn't been founded yet but it was about this period when things like this were happening. You might know that the great inspiration for the founders of the New School were the London School of Economics and the Ecole Pratique in Paris. There are lots of ways in which the history of economics as a discipline, and the institutions that economists in particular helped to create, is one in which The New School is a link and an important one.

My own theory is that the concept of 'social research' was deliberately open – open enough that it helped you appreciate the modern arts as forms of research. There is something about the modernist movements in the arts that made them the kind of thing that the New School for Social Research was supposed to do. There might be aspects of modern urban life that you would understand better through dance: the experience of dance that might actually itself be an important form of social research.

I'd love for us to actually embrace the concept of social research in this very broad sense and suggest that, in fact, there was a continuity with the original founding and the kind of work that happened in the arts after that. It's a question to you as well, if you can tell us more about what meanings the phrase 'social research' might have had in the first few decades of the 20th century based on your work, it would be really interesting to know what the New School was doing, how it was positioning or identifying itself. It doesn't work if people don't hear something when you choose a new phrase like that. What they meant by it is something we absolutely would love to know. Economics is important to it but we do think that it's a much broader thing than economics. Thinking back to Will Milberg's opening remarks, it's that much broader and capacious sense of the social dimension of all things.

One final point – the keystone of the arch I've been building here from 1919 to 1933. The New School was home to something called the *Encyclopedia of the Social Sciences,* and it's one of the great crimes against our own history that we don't remember it and celebrate it. The *Encyclopedia of the Social Sciences*' editor in chief was Edwin Seligman, a psychologist at Columbia, but he was just the figurehead. The person who actually ran it was none other than Alvin Johnson – at the same time that he was keeping the New School going enough to actually get it a building. (If they were able to get that first modernist building in New York City built at 66 West 12th Street before the University in Exile, the 1920s must not have just been a dark age in which nothing was happening. Somebody must have thought it was worth paying for.)

The *Encyclopedia of the Social Sciences'* fifteen volumes brought together work in the social sciences, understood in just that broad and generous way Will described – the New School way. Alvin Johnson did all the commissioning of the entries and a great deal of the editing, The original idea for it came from an anthropologist from Columbia, who like many Columbia people, came to The New School because they were eager to teach the things they thought were more interesting than what Columbia would let him teach. It was his idea originally to have an encyclopedia for the social sciences, perhaps for workers, which would be a very German-like; a set of manuals, 250 page chapters that would tell you everything you would need to know about Sociology, Demographics, and others. Alvin Johnson nixed this idea and replaced it with an alphabetical structure with as many authors as possible. Johnson's *Encyclopedia of Social Sciences* aimed to make visible the large international community of people who were working in this broad field of Social Sciences. ("Social science" was defined broadly enough to encompass the arts – including dance!) So rather than finding the six experts who could write the book-length chapters, he put together something with thousands of entries and hundreds of contributors – and a little biography of each of the contributors in the back. In more ways than one The New School's *Encyclopedia of the Social Sciences* put the social sciences on the map. For 35 years it was the main reference work for this subject throughout North America and beyond, in institutions from high school and public libraries to research and policy centers to universities. It was connected to the New School and people knew that it was. And it was through that project that Alvin Johnson met almost all the people that were invited to the University in Exile. The missing link between the earlier years and the University in Exile, which is so important to us, is the *Encyclopedia of the Social Sciences*.

Ours is a 94-year story of experimentation with social research understood in broad and experimental ways – often, as I've suggested, led by worldly philosophers. Just as Hannah Arendt may be said to have come to The New School in 1967 because it was already her kind of place, we can say that the University in Exile was conceived and implemented by The New School in 1933 because it was already a vibrant and visionary center for social research.

Ed Nell: What Is The Mission Of The Economics Department: Transforming Economics Or Teaching Undergraduates?

Transcription*:* Abid Raza Khan

Mark's excellent introduction calls attention to the views of the German exiles who came in the 1930s. They were very much attracted to The New School by the fact that it was a pay-as-you-go institution. Thorstein Veblen, one of the founders, had been influential in setting it up that way, because he wished to avoid the influence of what he called the 'Captains of Erudition' – an ironic reference to the self-anointed 'Captains of Industry', who, like the 'job-creators' of today, thought that they were the creators of the modern world. The 'Captains of Erudition' , Veblen argued, wanted to impose a capitalist vision on education, celebrating the benefits of capital and free markets. In his view, they were uniquely unqualified to explain the working of the system in which they functioned, for part of their position was to act as propagandists and ideologues. .The German scholars who came to the New School, however, were less worried about the influence of capital on higher education, and more worried, very worried, about the potential influence of the state, worries that bubbled to the surface as the McCarthy Era unfolded. Indeed the New School had come into existence as a reaction to Nicholas Murray Butler's policies at Columbia University, where an Assistant Professor of History had been arrested for sedition because he gave a lecture arguing that German diplomacy did not solely or even chiefly cause the outbreak of World War I; responsibility was widely shared. That arrest, organized and supported by the Columbia Administration, was the origin of the rebellion which brought together the group of distinguished and progressive faculty that started The New School. The German exiles were very much concerned with the possible influence of the state, and aware that in the United States local and state Governments were very likely at times to interfere in the management of institutions of higher learning. So the New School avoided reliance on both private capital and state funding, and this was, I think, one of the attractions.

They were also uninterested in administration, and glad to see it minimlzed. One of the many good things Alvin Johnson, the first President, did that we should remember, was that he kept the bureaucracy small. When I came to the New School, the Graduate Faculty had many more students than today, but there were only a few full time administrators. I remember three, the Dean, the Assistant Dean and the Director Of Admissions (who also ran Recofds). No doubt there were others, certainly later on. There were also a number of very important secretaries; traditional secretaries, who often ended up managing everything. They played a very important role, and so did graduate students. Graduate students were hired as administrators, and they largely managed Student Affairs and did all the work in Records. So it was very unlike a normal University. In that respect, as you all know, it has changed pretty dramatically.

In the 1970s, the early 1970s, and really from the end of WWII, the city of NY offered a substantial raise to high school teachers and to elementary school teachers who earned an MA. Social studies teachers all over the city found the New School programs in Economics and Political Economy very attractive. I think my first class at the New School in Macroeconomics and Income Distribution had over 200 students, in 1969. Sometime in the late 1970s, during the crisis where the city nearly went bankrupt, giving teachers a raise for getting an MA stopped. This was a loss for the Economics Dept; our MA program had been substantial and had had students who were pretty good. The high school teachers knew what they wanted, they worked hard, and they were highly critical. They did not need spoon-feeding. They were adults; the New School was a place for adult education. So teaching them was quite different from teaching undergraduates. MAs for teachers was the foundation that supported, not only our department, but also sociology, anthropology, and politics. Psychology was different but also very large. All this meant that the graduate faculty was not such a big burden on the rest of the New School. I haven't looked at the figures, but my understanding is that, although it drew funds off the rest of the New School, it was after the city crisis that the Graduate Faculty became a serious burden.

The department, which was founded during the university in exile, and which perpetually debated and fought, discussed, argued, and fought again, over its curriculum was dominated by, well, it's not really clear who or what approach, if any, dominated. We had Keynesians and post Keynesians, competing varieties of Marxists, progressive neo-Classicals, and institutionalists. And a few who were more or less conventional. Right from the start there was diversity – and an ability to work together, which, of course, became frayed at times. Adolph Lowe was a particularly important figure, in his own right, and because he was Robert Heilbroner's mentor, (and, indeed, mine.) And he was a very significant figure in Germany. But there were others. Hans Neisser was an acutely intelligent marginalist economist, very critical of Keyne's liquidity preference approach to money, and critical of the Keynesian system in general. He didn't like the IS-LM model and he didn't like other variants of effective demand. He tried to develop an approach to macroeconomics which was built on the quantity theory of money. But it was not the quantity theory of money at all in the sense of Friedman; he considered that money was engendered in the process of the economy, in production, in distribution of income, in expenditure, money was, in a modern sense, endogenous. But the quantity theory gave a flow of funds approach to classify what was going on in the economy. And he tried to develop that. We held a conference called re-inventing functional finance. 'Functional finance' was a phrase of another person, not really a refugee although he was from London, Abba Lerner, who wrote a famous essay on functional finance; published in Social research, and then reprinted everywhere.

At the conference we had on re-inventing functional finance Professor Hans Michael Trautwein gave a paper on the work of Neisser, and others, showing that at the New School there was a lot of hesitation in the 30s 40s and 50s about the Keynesian system. It was not seen as adequate, for two interlocking reasons: it was not historical and it did not build on or around Marx. It did not take positions that were 'for and against Marx' - the title of the very influential book of Heilbroner. Prior to the big changes in the Department that came in the early 70s, there was a great deal of tension over the Marxian tradition: How do we handle Marx, how do we handle Keynes? Both were seen as fundamental to the development of a new economics.

Understand that there was no question that we needed to develop a new economics. The conventional economics of the post-War days were clearly inadequate because they provided no way of understanding fluctuations and growth, no way of foreseeing crises, no way of dealing with them adequately – a situation which only got worse as the century wore on, and the influence of conservative economics grew. Ordinary economics was something in which you could dig around and you might find some interesting ideas. But the question is how you build something new and more realistic. And it was understood that it had to be historical. These were Germans. This was the German Historical School.

This was the background to an understanding of what the Department set out to do. It was a small department, it was supposed to give PhDs, and it was in America, which means that the students had to know something about the ‘Anglo-Saxon’ economics. Which many people in the Department didn’t think was much help in understanding the economics of the real world. So we had to teach a critique, but we needed an approach that would help us understand the actual economy. Looking at the work of our predecessors in 30s and 40s, they had a made a great start, but it was time to develop these beginnings into a new way of thinking. There was plenty to build on in Lowe’s work, and in that of others – Abba Lerner, for example. ‘Functional finance’ is quite a radical approach, even today: It dismisses concerns over the deficit, it assumes that money is state=backed credit money, and it basically says that Keynes has not gone nearly far enough in bringing the state into the question of controlling the cycle.

In the 1940s a series of events took place which are truly remarkable, although the department of economics is not given credit for this and the administration of the school is perhaps not even aware of it. This was the development by Jacob Marschak of a seminar, which created the foundation of statistics and econometrics. The question he and his colleagues posed: how are we going to make use of the new insights of statistics in order to build an empirical model, which expresses our theoretical understanding of macroeconomics? But at that time It was not really clear what macroeconomics is, and it became responsibility of one of the new school students, Franco Modigliani, to work that out in his dissertation, which he did, resulting in the famous article in Econometrica that co-created the IS-LM system with Hicks in England.

This seminar would not be permitted today; first of all, it moved around a lot, meeting at other universities, or in private quarters, such as offices of the NBER. It also met irregularly. It had no definite syllabus, or rather, the syllabus and plan changed frequently. Students taking it had no way of knowing what to expect or what the requirements were. Secondly, a lot of people regularly attending and presenting papers were neither teachers nor students at The New School. Nor did they pay any fees. An especially notable case was Trygve Haavelmo, who later won a Nobel Memorial Prize for the work that he began in this seminar (the probabilistic approach to econometrics.) War had begun in Europe, the Nazis had moved into Norway, and he had come to New York as a clerk in the Norwegian

Shipping lines. He was an especially important and regular participant, often presiding over the discussion. Admittedly at the end of the seminar, at the end of three years of taking part in and jointly running it, he was offered a job at the New School. Imagine how today's administration would react to having somebody regularly acting as faculty over a three year period, teaching, and teaching very importantly; he was one of the supervisors of Modigliani. But he wasn't actually on the faculty, nor was he paid. He left for Harvard almost immediately after taking up his appointment, so his official connection to the New School was almost non-existent. Marschak ran this seminar as an intellectual project, an adventure; he brought in people he thought could help,- Abraham Wald from Columbia -, and many others. All of them worked on developing something wholly new - the foundations of econometrics

Marschak was then hired away to Colorado where he helped to found the Cowles Foundation; they somehow persuaded Alfred Cowles to pay for a research institute. It later moved to the University of Chicago and then to Yale, where it is now. But the origin of what came to be known as the Cowles Commission approach to econometrics is the seminar at New School that Jacob Marschak ran.

I say that because what was clearly most important in the minds of the people that participated in that seminar was basic research. It wasn't just research; it was creation. They aimed to create a new way of thinking about economics, one which would provide for exact measurement, so that you could say exactly what a government should do. How much would be needed to reach a goal or produce a result. Think of a recent example, the stimulus package that the Obama administration produced: Willi Semmler and I actually took only about 15 minutes to work out that if was half the size of what it should be, and we wrote a note on that. (Few agreed then, many agree today.) We were able to do this, and do it quickly, because there were plenty of statistics available, and plenty of ways to working it out based calculations of the values of the relevant multipliers. We could find multipliers for taxes, for various categories of expenditures, and so on. So we were able to quickly put them all together and come out with an estimate that the stimulus was about half the size of what it should be to accomplish the objective. Such a quick calculation was inconceivable in 1940s. There was no way to do anything like that; that it can be done now very easily is one of the legacies of that Seminar, and the Cowles Commission. (Of course, it can be argued that the Cowles approach to econometrics did ultimately fall short, because the models did mostly fail in 1970s. They broke down when faced with stagflation; they were being asked to do things they were never designed to do, as Lawrence Klein argued very strongly.)

But that kind of original, creative research, carried out in a movable seminar, is not consistent with a well administered university. The seminar is not presenting a teachable subject. It is not presenting material that can be learned – and paid for. But it is something the Economic Department nevertheless did, from time to time. Over the years, some 'irresponsible' members of the Department's faculty have permitted students to come in and take part in their classes for a long periods of time, in that same spirit. These were students or faculty from elsewhere who wanted to participate in developing new approaches, and who were not interested in credits or degrees. They did not want to *learn*, they wanted to take part in, help to create, a dialogue, one that might develop into a new approach. What I want to stress in this is that the mission of the department was understood to be developing a wholly new and different approach economics; a new economics. And it was the responsibility of the department in producing an MA and PHD that the students also understood why that was necessary, what the standard and orthodox view was and what was wrong about it.

But this also meant that the Department had to teach twice as much as any other department. Because we had to teach the orthodox approach, we had to criticize the orthodox approach, and then we had to develop those criticisms into an alternative. Apart from survey courses, we did not teach what we learned in graduate schools. We taught what we created, invented, as we went along. David Gordon taught social structures of accumulation; but there was no idea or theory of social structures of accumulation before David, Sam Bowles and Tom Weisskopf got together and worked it out. Many of us taught the capital theory critique, but we were among the ones who worked it out. We taught the articles we published. Willi Semmler taught the dynamics he was developing with his German colleagues; Lance Taylor the new macroeconomics of Development. A reinterpretation of Marx and the labor theory of value - when that came up in Anwar Shaikh's courses it was not because he had learned it in graduate school at Columbia. Nor did Duncan Foley teach what he had learned in graduate school; he taught why he rejected much of what he had faced in graduate school. There was no theory of Transformational Growth before I developed it; I taught what I was working on. All of us did. Our research and our teaching went together. David Schwartzman was our residential neo-classical and David was an immense repository on the working and mis-working of the great corporations of America. He was very critical. However, he held the view that simple textbook supply and demand gives you enough insight to see what was going on, and that competition was very important to understanding how the corporation works – we all learned from David, even though this framework was what the rest of us were refuting. But David knew the institutional facts, and was a great resource.

The course curriculum was a perennial issue. We had to work out how we could responsibly teach what we were developing and simultaneously meet the standards. Two sets of standards had to be met - the intellectual standards which we felt committed to, together with the standards imposed by the state and the administration. The point was to make sure that we covered, critically, intelligently and historically the orthodox positions in economics, while at the same time presenting the new materials we were developing.

It was a monumental task for a small department to take on this enormous project and we were unique. The department struggled with this again and again, almost every year. How could a department of somewhere around 10-12 full-time faculty, with a few part-timers, teaching, at that time 6 courses, and then 5, come up with a program that was intellectually responsible and at the same time, allowed for the faculty to focus on the research project of creating a new economics? Not to mention the task of responsibly supervising students writing dissertations for the PhD. Now that is an immense task.

I would conclude with a question. How are you going to do all that *and* develop an undergraduate major, for Lang College? How are you possibly going to do *both* a responsible and innovative, alternative graduate program *and* a coherent, comprehensive undergraduate major? You - the graduate students - are now going to get only *half the time* of the faculty. And the faculty will now have an additional burden, namely preparing and teaching courses for undergraduates – courses that cannot reasonably be based on their own new work. Undergraduate courses have to introduce students to the established basics of the discipline – *the basic ideas that failed to foresee, and have failed to explain the Great Recession, basic ideas which over its history, the Department has largely rejected*. But a good liberal arts college usually wants to teach the disciplines as they are. No doubt this can be done critically, but to go beyond that, and teach new and different approaches will be very difficult. Undergrads are paying customers, and expect to be served from a normal menu, one that will help them to get into a good grad school or business school. And, of course, the views of Trustees and donors, as well as the parents of undergrads, must be respected. The faculty will now have to spend time getting to know and cultivate and nurture a set of undergraduates as well as graduate students, and by the way …, in their spare time, not in the classroom, invent a new economics. Can this be done? What will have to give way? That is the question.

Or perhaps we should put it more provocatively: which does the world need more: another mid-level, moderately progressive liberal arts college, catering to undergraduates from well-to-do families, here and in China, or a truly new and more realistic economics, one that will fulfill the ambition of the early members of the Department, to show how the system actually works in ways that can be measured, so as to enable us to formulate policies that will make it possible to control the economy? (Note that both require raising money.)

Of course, a new approach to the economy might turn out to be flawed, or too complex to be usable. But then, a new liberal arts college might run into unmanageable competition from the Internet… the education business needs to heed what happened to the music business.

Postscript

The obvious retort to the preceding is that, for us and for many universities, a research/PHD program has to be supported by the profit from a high-tuition undergraduate college. The equally obvious reply to this has already been given: a small Department can't responsibly or adequately do both. (And the profits won't be enough to establish a large Department.)

No doubt compromises could be made: some faculty could be hired exclusively for research and PHD supervision, faculty with a serious research project under way could be exempted from under grad teaching and other duties, grad students could teach all the Introductory courses, so faculty could try to fold advanced undergrad courses into graduate courses ,,, and so on. But these all shortchange the undergrad program. Not to make them, however, shows a lack of commitment to research.

Think of it as an economist might: Adam Smith taught us that productivity arises from specialization and division of labor. We have three projects

- servicing an undergraduate major
- servicing a critical, alternative graduate program
- developing the foundations –theoretical and empirical – of a new economics

We can't do them all. As a Department we have had some success doing the second and third together. To try and do the first two together, instead, would amount to giving up the Department's heritage.

The only way I can see to save ourselves is to set up a campaign to raise money *specifically to develop a new approach to economics*. Some of us have been successful at doing this on a small scale. Now it should be attempted on a large scale. In the past the Administration has been reluctant to try this. However, after the miserable showing of conventional economics in dealing with the Crash and the weak recovery, this might be a good time to make the effort.

Anwar Shaikh: A History of the Structure and Evolution of New School Economics Program since the 1970s

Transcription: Samar Issa and Michael Isaacson

I wanted to talk in the little time I have about five issues: how the department was formed and what motivated people like us to come to this place; what we were trying and are still trying to do; some core structure of the path to give you some sense of difference between what we were once doing and what we now do. I will skip the details of a planned discussion about some past faculty and will just mention some eminent visitors. The names are available in the school's catalog and the list is quite extraordinary. And then I will perhaps end with some thoughts about the issues in the department now, some of the issues that Ed just raised, and some others.

Let me preface this by saying how I personally came to NSSR. I was a graduate student at Columbia University starting in 1967. I was living and working in Harlem. I was teaching at a school called Harlem Prep, which was founded to teach young people who were kicked out of New York school city public school and were considered unteachable. My job among other thing was to help teach them social science and math. We were pretty good at the math part.

At that time in 1968 Columbia was riven by a strike, which was started by SDS and spread. I joined the occupations in Fayerweather Hall, which was the building where the social sciences were located. And in that process I can say that I was substantially radicalized by subsequent events. I had already been moving to the Left. I was already living and working in Harlem, but this event led me to think of economics in a broader context. I had already become involved with the Cambridge Capital Controversy on which I later wrote in my PhD dissertation. But I got into Political Economy because I took a course in the Business School at Columbia in which there was a book by a man named Robert Heilbroner called the Worldly Philosophers. It's then when I thought this is it; this is what I want to do. This was so exciting and nothing like the courses I was taking at Columbia, although I had wonderful teachers. Gary Becker was a great teacher, and Bill Vickery was a great teacher, Kevin Lancaster and Ronald Finlay, but Heilbroner just motivated me in a way that nothing else had. Now, at some point later Bob Heilbroner had come to Columbia to give a lecture and I could not go because I was teaching. So Bob came by my office (graduate students had offices in those days if they were teaching!) and he stopped by and said “the Department Chair told me that you are a good person to talk to so if you are ever looking for a job give me a call.” I said I would be very happy to do that, I mean this was Bob Heilbroner, wow!

There were four of us who were radicals in the department but I was the youngest. Three out of the four were not rehired. Neither was the Business School Professor who built his course around Heilbroner's book. I was the fourth and in the spirit of solidarity I resigned from my teaching at Columbia – and then was without a job. I can tell you if you are going to do this, it’s much better to line up another job first because when I went to Heilbroner, I said "Professor Heilbroner do you remember me? You talked to me," and he said, "Yes," I said, "Well now I am ready for a job," and he said, "Well I don’t have one!" He said, “This is not how it works, we have to advertise a job and you have to apply.” But out of kindness he gave me a job as an adjunct, and that’s how I happened to come to The New School.

I was interested in The New School because of its activist roots and because of its interest in theory. Those were my roots at the time, and they were the roots of many people there. David Gordon (who came later) was involved in the Civil Rights Movement and also was interested in theory. Ed Nell can tell you about the occupations he himself had been in when we have more time to talk We understood that this world is not something we have to accept passively, and economics is not something we have to accept passively. Remember this is in the context of the civil rights movement, the context of the feminist movement, the context of the anti-war movement. All of us were involved in these activities. The Union for Radical Political Economics (URPE) was founded in 1968 after the strike in Columbia, and we were involved in that also from the start. Those are the activist roots.

The second part was the theoretical roots of the department. We were focused on critiques of the capitalist structure, racism, sexism, and especially for those of us who came from the third world, of imperialism. But there was much less interest at that point, especially from side of American radicals, in the issue of class. That came to the fore after we discovered Marx. Sexism was a big issue, right from the start.

We were also rediscovering things that had been suppressed and lost. And this is an important point. One of the things that motivated us to do what we do was the history of thought. Why? I always argue because the earlier economists were better. And we lose something if we don’t stay in touch with the Greats because they are the founts of ideas, many of which had been suppressed, especially the rediscovery of Marx in economics, through the work of the *Monthly Review* school, Sweezy's *Theory of Capitalist Development*, Baran and Sweezy's book *Monopoly Capital*, Braverman's extraordinary book called *Labor and Monopoly Capital* which everybody should read, Magdoff's work on imperialism.

Then there was the Cambridge Capital controversy which is how I got involved in the theory side because I read this article by Geoffrey Harcourt, and I became a fanatic. I used to carry around that article everywhere, confronting my professors, saying, “What about this? What about reswitching?”

It’s through this that I met John Robinson. I have told the story before. I actually invited Joan Robinson to come to lecture in Columbia, not realizing that students are not supposed to invite faculty to lecture, and she accepted. I went to the faculty, and I said John Robinson is coming, I invited her! And they said: well, we have to discuss this and they discussed it, and they said no because she’s not really an economist. And it’s only through threats that we were able to get her to come including the threat to raise money for her by having donations from students, and in the end she was invited and came. But the Economics Department then held a meeting at the same time for which attendance was mandatory so that no faculty could attend John Robinson’s talk -- except for two people Donald Dewey and Edmund Phelps because they had taken courses at Cambridge with her. You can see where my preferences were in this discussion. I got to meet people like Joan Robinson, later Pasinetti who was my teacher at Columbia, Garegnani, Kaldor against the Samuelson/ Solow wing which was so heavily represented in Columbia's department.

There was also the revitalization of Keynesian economics through journals and writings. Ed Nell is absolutely a major US figure in that, and Joan Robinson obviously on the other side. In Post Keynesian economics there, as Malcom Sawyer, Philip Arestis, Alfred Eichner who was my teacher at Columbia before he was not given tenure and had to move to Rutgers. There was the rise of Sraffian economics, coming out of Sraffa’s elliptical and mysterious work, which I still puzzle through every couple of years when I teach a seminar on it. Pasinetti, Garegnani, Steedman, Kurz – all these major figures who came here, with whom were in constant touch. Then there was the rise of global concerns about imperialism and development and under development –Samir Amin, Arghiri Emmanuel, Ernest Mandel. All of these we were struggling and trying to incorporate, and trying to understand because we saw it as our goal, which was to develop a different and more grounded understanding of actual capitalism.

And then the third element was the wonderfully active and passionate student body. There is one thing that kept all of us on the faculty here – the students – because many times we wanted to leave because of the difficulty of being in this place, but the students! The

Americans, the Greeks, the Turks, the Iranians, the Mexicans, the Brazilians – the list just goes on. I still have a huge number of friends from that time period, because they were so passionate, and they were passionate because they were changing the world where they lived. They are not doing it for theory alone. They wanted to understand, then go back and try and change things.

In 1973 the tuition was $95 a credit – that’s about $2500 a year, and that's counting 4 courses per semester. The net cost of tuition after financial aid was $275 in the late 1970s. I know because I was Chair, and I got the list of the financial aid. We had incoming classes of 80 to 90 people at the peak.

Now the net cost is about $25,000 if you are lucky, maybe more. We have tried as economists for years and years to persuade the administration that there is something called the elasticity of demand. We’re down to 15 incoming students. But the tuition continues to go up every year.

There is one thing we can say that as radical economists in the NSSR, we were a Department in Exile in the University in Exile. Being an economist of this sort they exiled us from our profession, and by the 1980s the exile was permanent. All of us came from universities where we could no longer go back and give a talk because what we had to say was not even considered possible. Remember how Lucas talks about people giggling and snickering when Keynes was mentioned? Now imagine what people in Chicago would have done if you talked about Marx, the Cambridge Capital Controversy, or Joan Robinson who “wasn’t really an economist.”

What were we trying to do? We are trying to integrate our practice and experience with the past and with new theories. That was important because discovering the past and rediscovering the past theories was very important. There were always differences and perspectives that Ed alluded to. There was a differences between those who believed in monopoly and those who believed in competition; those who focused on the US and those who focused on international and global capitalism; those who believed that patriarchy was a dominant relationship versus those who believe that class was the dominant relationship; those who believe that social structure drove accumulation versus those who believe that profit drove accumulation and social structures merely modulated them.

When I say these were differences it doesn’t give you any sense of the blood in the water there. We had faculty members driven out of classrooms in tears because of the conflict in the class that arose between them and students, not to mention what would happen in department meetings. Yet, we were concerned with the common cause which is the critique of the ideology of the orthodox economics. This was something that was common to all of us, and that's one uniting feature. We were engaged deeply in a critique of methodology. Many of us studied anthropology, psychology, and philosophy, including me. I was part of Althusser-Colletti study group which lasted for two years, and this we did in our spare time. We had gender and race incorporated in the program from the start - Heidi Hartmann, Gita Sen, Nancy Folbre, Rhonda Williams, and Howard Stanback. Rhonda did all three because she was black, lesbian, and feminist, and she did all three extraordinarily well, but she did not like The New School and she left. I strongly recommend that you look at this catalog on display to give you a sense of what we were trying to do and what we taught.

In 1978, we offered 13 fields of concentration in Economics in which there were at least two or usually three courses in each field – so that's 39 courses in 13 fields. Economics and political economy was one field, history of thought, money and banking, public finance, industrial organization, international trade (That was me – I taught international trade for ten years), economic history, labor economics, quantitative methods, development economics, urban and regional economics, economic planning, and class and gender. That was our structure and was filled by full time faculty members.

Now you have to remember that we each taught six courses, there were about between 10 and 13 of us – usually 11 to 13 of us – so six courses times 13 is how we kept this program alive. We did not have any undergraduate courses, and we didn't get sabbaticals unless you taught the sixth course, so sabbaticals were not given to you unless you over-taught. So every six years you taught a year's worth of courses in advance, and then if they were in a good mood they allowed you to take a sabbatical. We tried – David Gordon particularly – to organize a union, but it didn't work because most of the older faculty didn't like the idea of a union, and it was not much prospect of a sufficient number of the younger faculty joining it.

The names of most of the faculty have already been mentioned, but I want to mention add Thomas Vietorisz (the "Mad Hungarian" as he was known) an MIT economist taught every kind of thing from planning to a microeconomics course which began with a discussion of modes of production -- much to the consternation of people who thought they were going to be studying demand and supply curves.

David Gordon, Harry Cleaver, Heidi Hartmann, Alfredo Medio, Heiner Ganssmann, Ross Thomson, Jan Kregel , Vivian Walsh, John Eatwell, Heinz Kurtz, Nilüfer Çagatay, Gerald Epstein, Nancy Folbre, Howard Stanback who went to work for Mayor Harold Washington in Chicago, Pierro Garegnani, Rhonda Williams, Gunseli Berik, David Howell, Thomas Palley, Salih Neftçi, and this is just where the catalog ends in 1997.

Lectures were given here by Paul Sweezy, who actually taught a course for a while before it turned out he couldn't handle the stress of the conflict in the class. Samir Amin, Gunder Frank, Ernest Mandel came here for the first time he was allowed in the United States. He was not allowed previously, but when I was Chair I invited him. The hall was so full that we had to run the feed into the student cafeteria in order to accommodate the numbers of people who came to hear him. That was the first time that Americans in his movement has seen him in person because he was banned from the United States. Abba Lerner came here. I remember he was in sandals. He came from California. He was wearing socks because it was winter, and I thought, "Wow, that is definitely a different climate in California. That would be great." It was a concession to us that he wore socks.

The NSSR was born in 1919 after the convulsions of World War I. The University of Exile was created in the 1930s in response to the rise of Nazism. The current Economics department encompasses all those strands, which Mark so brilliantly laid out, and that Ed laid out in terms of the German faculty of the NSSR. It encompasses the ferment and change of the 1960s and 1970s, and now again hopefully a revival after the long sleep of the 1980s and 1990s.

One of the things that distinguished the Economics department beginning from the 1970s was that our orientation expanded beyond the traditional focus on Europe, which it was so common in other departments. From the start, we were international because our student body was international, and our faculty at least at that time was more international. What always distinguished us was a commitment to social change and theoretical rigor in a critical perspective around the world. I believe, and I believe most people in this faculty believe, that technical tools come and go. I don't know how many people know what a Cusp Catastrophe is, but it was a very hot topic at one point.

What remains fundamental is Heilbrober's question of the vision that structures different Economic traditions. Whatever our own differences were on that score, we had generally been united in our criticism of neoclassical orthodoxy and of the policy it espouses. Many of us were on panels in URPE in which we were presented a united face against orthodoxy even though we were disagreeing with each other, because we saw this as a political commitment. We had to have stand together because we were easy to knock down if we were standing separately.

The present world is once again convulsing in crisis and political upheaval as it was in the 70's, as it was in the 30's. And even if the US is still calm and the US campuses are sleepy there is no question that the demand for alternate perspective has grown sharply and that we in this department are proud to remain part of this supply.

I am not a Keynesian, so I don't think supply comes from demand, but I do believe that in this particular instance the supply of alternative economics should actively expand to meet demand. So I think this is something that we should think about. I think that the key as we go forward and especially as we consider new faculty is to consider people who have the ability to discuss and analyze competing visions, who therefore have been grounded in and that are capable of dealing with people who differ with them sharply, because if they can't do that the students will suffer. I think that has been one of the strengths of our department. Ed and I have spent countless hours, as David and I did, talking about our differences, often outside the department because you don't have time when you are here.

And secondly, my personal opinion is that new faculty we hire should have the courage to take on the orthodoxy. It is very easy to be against the orthodoxy by focusing on one small area, but we tend to ask people for something more: what is your vision? And I think that is a good thing to ask everybody, us included, when you as students are questioning existing and potential new faculty.

Breakout Panel:

The New School Economics' Role in Academia

Report by Michael Isaacson

Panelists

Ramaa Vasudevan*:* Assistant Professor, Economics Department, Colorado State University.

Rajiv Sethi: Professor of Economics, Barnard College, Columbia University.

Matias Vernengo: Associate Professor of Economics, Bucknell University

The afternoon session of The State of Worldly Philosophy colloquium broke into two side by side panels. The purpose of both was to investigate the prospects that New School for Social Research (NSSR) students have in the professional world - one panel for academia, the other for policy. The academia panel was chaired by NSSR Economics PhD student Rishabh Kumar.

The panelists are all successful professors of economics at a variety of institutions. All three hold PhD's from The New School. Rajiv Sethi is a professor at Barnard College. Ramaa Vasudevan is a professor at Colorado State University. Matias Vernengo is a professor at Bucknell University. Each charted a unique path to The New School and through academia therefrom.

The graduate students present had several pressing concerns. The global economy was still in a recovery that often elicited scare quotes in heterodox circles. Academic jobs were scarce and tenure track jobs were becoming the exception and not the rule. Neoliberalism is the conventional wisdom, and mainstream policy discussions are torn between "a little austerity" and "sell all government assets to the most politically-connected bidder."

Given the political climate, the heterodox education offered at The New School is becoming an increasingly harder sell. The conventional economic demagoguery, widely debunked by every other social science, was finding its way into pop science books with catchy titles such as Freakonomics and The Undercover Economist. In mainstream circles, alternative methodology connoted merely altering the dominant model of analysis to arrive at slightly different results from previous studies. How could a minority of economists be expected to challenge the air of the orthodoxy, present a meaningful critique, and spread a viable alternative?

Ultimately, the students were concerned about negotiating a place relative to economic orthodoxy; remaining faithful to realistic theoretical commitments; and finding gainful employment within academia in the process.

Rajiv Sethi

Rajiv Sethi began his remarks by reminiscing about his time as a PhD student at the NSSR. In particular, he recalled discovering Hyman Minsky and John Maynard Smith. The former, rather well known in heterodox circles, was a pioneer of the theory of the economic cycle. Minsky posited that economic cycles were the result of the financial sector vacillating between the paradigms of hedging, speculative, and ponzi borrowing. The latter, John Maynard Smith is not to be confused with the better well known John Maynard of economics - Keynes. Smith's book *Evolution and the Theory of Games* presented an extension of Nash game theory called the "evolutionarily stable strategy." Such a strategy, when adopted as convention by a given population could not be invaded by any alternative strategy.

At The New School, Sethi was inspired by his professors, but his most rewarding interactions at NSSR were with other students. He was encouraged to fuse the ideas of Minsky and Smith which yielded a flurry of papers in journals such as *Metroeconomica* and the *Journal of Economic Theory* throughout the 1990's and early 2000's. In addition to being a professor of economics at Barnard College, he is an external professor at the Santa Fe Institute and on the board of editors of the *American Economic Review.*

Sethi is skeptical of approaches which self-consciously placed themselves outside of the economic mainstream. He insists that, despite the apparent hegemony of Neoclassical economics, crises like the present one present an opportunity for alternative approaches to penetrate into mainstream discourse. He marks the frenzy to add frictions into the dominant dynamic stochastic general equilibrium (DSGE) models a signal of the desperation of the Neoclassical synthesis to stay relevant.

To Sethi, students at The New School are as capable as students in any other program, but far more interesting. As a result, he quipped, they find themselves without jobs. There is an apparent hole in the discipline, and NSSR students are in a unique place to ll it. The reactionary elements stoked by the financial crisis are trying to push alternatives out of the discipline, and New School graduates must push back against them.

He believes that, as professionals, alumni of The New School must be cognizant of what academic institutions want, and that's coauthorships. Thus, New School graduates must demonstrate their capability to handle a diversity of modeling techniques. Additionally, ability to acquire external funding also plays a large role. Sethi says New School alumni are uniquely positioned to pursue interdisplinary research and grants. Success for NSSR alumni comes from embracing a pluralistic approach with a heterodox intuition.

Sethi actually doesn't find the distinction between heterodoxy and orthodoxy to be particularly useful. He is not guided by a strictly heterodox orientation so much as an intense curiosity and dissatisfaction. To him, there ought to be different ways to engage with the profession. He urges us as amateur economists to engage the mainstream on all fronts. He suggested commenting on the blogs and replying to the working papers of professional economists. He also offered his office hours to anyone who wanted to stop by.

Ramaa Vasudevan

Ramaa Vasudevan came to The New School having studied the labor markets of colonial India. By the time she got her M. Phil at the Centre for Development Studies, she was a self-confessed Marxist. At The New School, she felt she was prepared for a rigorous investigation of Marxist thought and analysis. She graduated from NSSR having studied uneven development in international trade and finance.

That preparation has shown through her work. As a professor at Colorado State University – an institution well-known for its heterodox bent – she has published in the Review of Radical Political Economy, the Economic and Political Weekly and the Monthly Review primarily with publications discussing financialization through a dialectical-materialist framework. Additionally, she is a member of the Eastern Economic Association, the Union of Radical Political Economists, and the International Development Economists Association.

At The New School, she recalled that she was forced to rigorously engage with a breadth of material, learn their period in history, and assess them relative to each other. However, she warned that theoretical methods are not enough. Well framed questions must be validated by solid empirical answers. She observes that progressive economists tend to overlook empirical techniques. This, she says, is a mistake.

Vasudevan cautions that this does not mean that the use of empirical methods means fighting with the mainstream on its own terms. In addition to learning empirical methods, she has hope for an interdisciplinary path, particularly as a research strategy. However, one's research path must ultimately depend on the question.

She asserts that the project for heterodoxy cannot be fought from within because it blunts the conversation. She points to the result of the Cambridge capital controversy. Joan Robinson had pointed out the circuitous logic required to explain the measurement of capital. Passinetti had proven the technical unlikelihood of monotonically decreasing demand curves for factors of production. Anwar Shaikh demonstrated that deriving Cobb-Douglas type translog functions proved not microeconomic laws of production but rather macroeconomic laws of accounting algebra. Heterodox economists have a tendency to celebrate it as a thorough debunking of neoclassical economics, but few think about the result: Exposing the logical flaws of orthodoxy is not enough to dislodge it from its perch. Thus, no one need waste their time with directly engaging the mainstream. One is engaging the mainstream by virtue of being in the economics profession. Simply because mainstream is mainstream, anyone arguing outside the mainstream is necessarily challenging the mainstream. And in a male dominated profession, she says, women outside the mainstream face an even greater challenge.

But it's a challenge that she doesn't hesitate to take on.

Matias Vernengo

Matias Vernengo came to The New School from Universidade Federal do Rio de Janeiro, Brazil. His master's thesis was on Keynes' role in the history of economic thought. When he got to NSSR, he says that he was shocked by what passes for heterodox economics in the United States. He wrote his dissertation on the relationship between foreign exchange, interest and prices.

While studying in the US, Vernengo recalls the degree of anti-intellectualism his colleagues at more mainstream institutions faced. He recalled one colleague who was told by a professor, “You seem to like to think; you should go to Europe.”

He remembers the degree to which students around him at other schools were cautioned against learning too much. To him, he lessons the mainstream seemed to teach were, Don't read. Don't think. Just solve the exercises.

Vernengo recalled that, unlike the other panelists present, he was not worried about finding employment in economics because jobs were available in Latin America. Throughout his career, he has held various academic and policy positions in the US and Latin America including the International Labor Organization, the Central Bank of Argentina, the University of Utah, and presently, Bucknell University. Additionally, he can be found on Twitter and maintaining the blog Naked Keynesianism.

For Vernengo, heterodoxy includes rethinking the profession and reconsidering classical (as opposed to neoclassical) traditions in economics. However, like both Marx and Keynes, we must get rid of Say's Law. To do this while critically engaging the mainstream is an almost intractable task. He told the students present, “If you came to The New School to be at the top of the profession, you're really crazy. I mean, love you, let's go have a beer, but you're really crazy.”

He acknowledged that we, as heterodox economists, don't have much theoretical engagement with the mainstream, but that we don't need it. He dismissed out of hand the notion that because heterodox economists cannot get published in certain journals they are being excluded. He sees the project of heterodoxy requiring an identity in-and-of-itself, rather than relative to the mainstream. He remarked that, “We are in a profession that is supposed to tell policy makers, 'Screw them; markets are good. '”

Like Sethi, Vernengo believes that students from The New School are uniquely positioned to contribute to academia. Given their critical training, New School graduates are in a better place to teach neoclassical economics. The secret, however, is getting one's foot in the door. He advised students to accomplish this by having a fantastic job talk paper and to network -- by which he meant get internships.

Rather than focus on toppling the mainstream, Vernengo advises that we ought to be spreading the gospel -- a project that begins in the classroom. In a nod to Piero Sraffa, he quipped we need to be engaged in the “production of heterodox economists by means of heterodox economists.”

Moving Forward

Students came away from the panel with a renewed sense of purpose. Rather than fight an uphill battle of trying to demolish the mainstream, students were encouraged by the panelists' advice to create realistic alternatives. Students knew that finding good academic jobs with a degree from The New School wasn't going to be easy, but they were reassured that the task is not intractable. Equipped with a multiplicity of paths, students felt more confident that they could follow the trails blazed before them and eventually chart their own.

Breakout Panel

Where The New School meets Policy

Report by Ingrid Harvold Kvangraven

Panelists

Heather Boushey
PhD from the New School, 1998
Current Position: Executive Director of Center for Equitable Growth and Senior Fellow at Center for American Progress
Research focus: Employment and Earnings, Economic Policy, Inequality and Growth
PhD Advisors: David Gordon, Anwar Shaikh, Bennett Harrison, Hector Cordero-Guzman

Massimiliano La Marca
PhD from The New School, 2007
Current Position: Economic policy specialist at the International Labor Organization (ILO)
Research focus: Trade and development
PhD Advisors: Duncan Foley, Lance Taylor, William Millberg

Rick McGahey
PhD from The New School, 1982
Current Position: Director of the Environmental Policy and Sustainability Management Program, and Professor of Professional Practice in Public Policy and Economics at the Milano School of International Affairs, Management, and Urban Policy at The New School
Research focus: Urban and regional economic development, triple bottom line investing, program evaluation, retirement policy, and workforce development
PhD Advisors: David Gordon, Robert Heilbroner

While many New School graduates go straight into academia, many also seek policy jobs, be it in the US, in their home countries or in international organizations. In fact, according to New School alumni working in policy, this university prepares graduates particularly well for a variety of policy jobs.

This was the general sentiment at the policy panel of the student-organized colloquium in February. PhD student Lauren Schmitz chaired a panel of three New School alumni from the economics department that have all had or still have successful careers in policy, namely Heather Boushey (PhD 1998), Massimiliano La Marca (PhD 2007) and Rick McGahey (PhD 1982). Their paths to The New School and after are quite different, but it turns out that they agree on many of the strengths that New School graduates possess and the unique perspectives they bring to the policy sphere.

From The New School to policy

The three panelists' journeys from The New School to the policy world were quite different, but they have similar elements. For example, all three point to the fact that they did not land directly into their dream job upon graduating, but eventually got there through alternative routes. Rick McGahey, for instance, was a part of a federal funded project on crime and unemployment while at school, and did data work that he could use in his dissertation. Doing this prepared him for a job at a research center at NYU, where he started writing opinion and scholarly pieces, which were eventually picked up by the New York State economic development agencies that offered him a job as an advisor. This became his gateway to the political scene and he was later to work as a policy advisor for Senator Ted Kennedy, a job McGahey describes as the "best I ever had". Before returning to The New School and academia, McGahey had a number of policy positions, such as Assistant Secretary for Policy and later for Pension and Welfare Benefits at the US Department of Labor in the Clinton administration, Executive Director of the Congressional Joint Economic Committee, as well as Deputy Commissioner for Policy and Research for the New York State Department of Economic Development.

Similarly, Massimiliano La Marca took a job as an adjunct professor at The University of Palermo in his home country Italy upon graduating, although he had his mind set on the United Nations. Nonetheless, La Marca had done UN internships and shorter contracts for various UN agencies before, and from Palermo he started getting in touch with UN agencies in Geneva. One year later he got his first job at the UN Conference on Trade and Development (UNCTAD). At UNCTAD he got lots of relevant experience working on the Trade and Development Reports and the Least Developed Countries Reports, and he was eventually able to transition to his current position as economic policy specialist at the International Labor Organization (ILO). La Marca's work at the UN has been mostly research focused and it has allowed him to draw on knowledge about development and macroeconomics that he learned at The New School. He describes ILO as an interesting and challenging workplace, where he gets to conduct economic analysis with social and even normative aspects.

Heather Boushey took a policy job immediately upon graduating, where she was able to gain experience that led to a think tank position in DC. Her first job was a post-doctoral position at the New York City Housing Authority. This was a great opportunity for her to develop her analytic skills and build a network. While a post-doc, she did a lot of work with advocacy and community groups and think tanks in DC on the side. With this wealth of networks she knew a lot of people when she finally moved to DC and it also provided an opening to a job at the Economic Policy Institute (EPI). She spent three years at EPI and then five years at the Center for Economic and Policy Research (CEPR) and one year at the US Senate, before taking a job at the Center for American Progress in late 2008. Most recently, she co-founded the Washington Center for Equitable Growth with John Podesta.

The role of New School graduates in Policy

"Is there a unique perspective or vision that New School students bring to policy work?" Lauren Schmitz asked the panelists at the colloquium. Although McGahey, La Marca and Boushey have quite different policy jobs, their answer to this question was a solid yes. McGahey points to the fact that The New School really teaches you to take a critical stance and this prepares you well for policy work. Additionally, he finds that a lot of New School students choose to take rigorous econometrics courses, which allow you to analyze and understand policy papers well. This is important in the policy world, even if you are not going to be running econometric tests yourself.

La Marca adds to McGahey's point that The New School teaches you to see economic phenomena in a broader perspective; that these phenomena have historical and social dimensions. Furthermore, La Marca finds that there is certainly a demand for New School graduates and for alternative perspectives in international organizations. In the ILO, for example, there are three generations of New School graduates. One of the most important elements that these organizations look for, however, is the ability to provide answers to important questions. The New School, with its inter-disciplinary nature, prepares you well for this type of job.

Boushey echoes some of La Marca and McGahey's arguments, as she highlights how valuable it is to obtain this deep understanding of how the economy works, which the New School provides its students with. Having focused on labor, macro, and political economy at The New School, Boushey feels like she really gets what makes our economy tick, she understands the role of policy in that context, and both the limits to what policy can achieve and its potential. She found that when the crisis struck, she was prepared to understand what was going on and provide policy advice because her training had provided a strong foundation.

Furthermore, the panelists all pointed to the critical inquiry taught at the New School as a great advantage, as it gives students a solid base for thinking critically. McGahey pointed to the fact that students at The New School do not take a lot of things for granted, and this helps them in policy debates. Further, if you have learned that there are varieties of perspectives within economics, this can help you understand a particular policy problem, as you a have a large toolbox. La Marca adds that it is an advantage that New School graduates know both heterodox and mainstream economics. The school helps you aquire a breadth of knowledge, which is necessary when you search for useful answers. But, you also need to make sure that you have some specialization.

What's more, Boushey points to the University's interdisciplinary nature as an important strength. The real world is not split into different academic fields. You can be really good at economics, but in a policy setting that may not always be very helpful as that is only one piece of a very large puzzle. Economists from other schools have normally not studied sociology, political science, or history. At the New School, she was able to study economic issues from a variety of disciplines, which she has found helpful for understanding how the economic system works. McGahey echoes this sentiment and adds that he can count on one hand the number of policy makers he has met that have heard of Hyman Minsky or other heterodox economists who could help explain issues such as the economic crisis, inequality, financialization, or labor market discrimination.

However, after decades in the public sector, McGahey finds that New School graduates are still fighting an uphill battle and that heterodox economists in general aren't widely regarded in the public space. Nonetheless, he finds that there's a very important place for them and that there are good, interesting and important careers to be had, especially in institutions affiliated with the left, including labor unions, progressive advocacy organizations, and progressive elected officials.

Is it possible to combine academic and policy jobs?

The policy market definitely has more entry points than academia, Rick McGahey points out. Although you do rigorous work in the policy world, it is not the same kind of work that you can feed into academic journals, so it can be hard to maintain a foot in both worlds. McGahey himself is an example of a successful crossover from policy to academia, however, as most of his career has been spent outside of full time academic work, but he is now a Professor of Professional Practice in Public Policy and Economics at The New School. Boushey finds that people she knows who have wanted to go back to academia have been able to get academic jobs, many in policy schools, which is often their preference.

According to Massimiliano La Marca, there are a lot of experienced people going from academia to international organizations as well, but they do meet some challenges, as the way of working is completely different. He points to an important way to bridge the gap between these two worlds, and that is to make use of exchanges. The IMF and World Bank, for example, have visiting scholars and this can be an advantage to both academia and policy spheres. What's more, in some international organizations you will actually have time to publish academically, whereas in others there is more focus on flashy policy reports.

Advice to current students

Finally, the question many of the students in the audience had been waiting for - What advice can the panelists provide for current students looking for a career in policy? All three panelists point to the marketability of technical skills. Boushey spent the first five years of her career running regressions and doing data analysis and this was clearly helpful for the rest of her career. In addition to data skills, La Marca finds that modeling can also be very useful for international organizations, as they are increasingly focusing on making accurate forecasting models.

Furthermore, all the panelists point to networking, internships and focusing your dissertation on something policy-related as important ways to get into the policy sphere. Boushey emphasizes that it is important to write a good job market paper and direct it towards the policy sector. She personally did many interviews when writing her dissertation, and although she did not directly use them in her thesis in the end, it provided her with a network that greatly benefited her career. She also suggests that if you want to work in policy in the US it will be useful to work in politics for some time, as her time in the Senate was a valuable way to gain an understanding of how to be helpful to policy makers. New School professor Teresa Ghilarducci, who was in the audience, added that one of the aims of the Economics Department's think tank, The *Schwartz Center for Economic Policy Analysis* (*SCEPA*) is to get New School PhDs involved in policy and to give them writing and policy training.

In sum, although it is not the easiest job market out there, New School graduates possess important skills and an understanding of the world that makes them exceptionally suitable to work with policy.

Future Vision for New School Economics

Transcription: Alexandria Eisenbarth

Mike Isaacson: The purpose of this panel is to answer the question that Vladimir Lenin asked, "What is to be done?" As Gregor [Semieniuk] mentioned in his remarks this morning, the Economics department currently finds itself in a sort of crisis. As our faculty ages (and they do so as fine wine), we find ourselves scrambling for new scholars and new scholarship. As with any such crisis, we are presented with an opportunity. I see this as a gift. Rather than patching up a sinking ship, I see this crisis as an opportunity to build a fleet of new ships, both in faculty research and, more importantly for me, in student scholarship. We find ourselves with the opportunity to chart an infinity of new paths forward. So first things first, let's go ahead and introduce ourselves. I'd like our panelists to give their names, their affiliations with The New School, and two or three pieces of research that they are particularly proud of.

Duncan Foley: I'm Duncan Foley. I'm the Leo Model Professor of Economics in the department and I am very much interested in its current and future welfare. Some of the things that I have been working on recently are a book called Adam's Fallacy about the history of political economy, some work on global warming, climate change, and political economy, and I'm also working on bringing ideas from information theory and statistical physics into Economic methodology.

Ellen Mutari: Hi, I am Ellen Mutari. I bring somewhat more of an outside perspective since I had a very brief one-year position here at The New School during the 1995-1996 academic year, which was right after getting my Ph.D. in political economy from American University in D.C. I like to say I jumped into the deep-end of the pool teaching graduate students here immediately after.

I assume that I am probably here to represent some of that impressive sequence of feminist political economists that people talked about this morning who passed through this department in the 1980s and 1990s Gita Sen, Lourdes Benería, Heidi Hartmann, Rhonda Williams, Nilüfer Çagatay, Nancy Folbre, and many others who were mentioned this morning.

While I was here, I taught a year-long sequence in feminist political economy and another in US economic history, both of which are very important to me. During that year, Heather

Boushey and I, along with another graduate student, Will Fraher, put on a conference that resulted in our edited book, *Gender and Political Economy*, published by M. E. Sharpe and, I have to say, after hearing the stories told this morning, that I never left the classroom in tears. Since then, I have been at the Richard Stockton College starting in 1999, which has a strong legacy in interdisciplinary teaching and learning. It is a public sector institution.

I'm particularly proud of my work on a 2002 book from Routledge, *Living Wages Equal Wages: Gender and Labor Market Policies in the United States*. This book was written with my long-time collaborator and partner Deb Figart and our friend Marilyn Power who also teaches from time to time at The New School. The book's theoretical contribution was our pluralist approach to wage determination that wove together multiple strands of political economy with feminist social practice theory and it is that integration of feminist social practice theory that I am particularly proud of.

My forthcoming book also with Deb Figart is on the study of managerial strategies and declining job quality in the casino gambling industry in Atlantic City, New Jersey. We did something that we are allowed to do at this stage in our careers, which is that we talked to people; we did qualitative interviews with casino workers about their perceptions of their jobs. So look for that coming out next year.

Ramaa Vasudevan: I am an alum of The New School; I did my Ph.D. here under Duncan [Foley]. Anwar [Shaikh] and Lance [Taylor] were also on my committee. My research then was on the international monetary system and, since then, my work has been on the political economy of money and finance, trying to extend Marx's monetary theory into a framework to analyze a world monetary system based on monetary liabilities, the credit money of a country in order to deepen a radical analysis of money and finance as a way of understanding imperialism. I have been working on questions about the economic crisis and, in particular, applications of international financial system hinged on the dollar on the unfolding of the crisis. And right now I am interested in the role of the managerial class.

After my Ph.D., I taught for a year at Barnard and since then I have been at Colorado State University. My fellow New School-er, Daniele Tavani, is also at Colorado State, so we have a little bit of The New School at the foothills of the Rockies and we are trying really hard to build a wider space for rigorous vibrant heterodox research. So wish us luck!

It is kind of embarrassing to be asked for papers you are proud of in such company but one paper I am proud of came out in the *Review of Radical Political Economy*, "From the Gold Standard to the Floating Dollar Standard: An Appraisal in the light of Marx's Theory of

Money." The other piece of work I am proud of was work done in collaboration with Deepankar Basu on the technology distribution and the rate of profit in the US economy.

Finally, something that does not count as an academic publication but something that I think is read more than any of my other papers is a *Monthly Review* piece called "'Libor'ing Under the Market Illusion" on the libor-crisis.

Ali Khan: My name is Ali Khan. Other than 4 years at Urbana-Champaign, John Hopkins was my first appointment. I have been there all the time. I just spoke to Daniele and he said he took a course in 2006 from me. This was an invitation which Duncan [Foley] gave to me to come and teach the course so it must be 2004. I try not to remember dates because I am superstitious and I feel I might be jinxed.

My association with The New School goes deeper than I have full understanding of, myself. In some sense, and I should not be saying this here because the chair person is sitting on my right, teaching here has always been therapeutic for me. I still have not got over the classes and the cohorts that I have taught here. And it has been a fascinating comparative study for me between my students at Hopkins and my students at The New School. Duncan very kindly came down for a small conference in May at John Hopkins. Duncan and Markus Schneider spoke and they said that they represented my double life and that is precisely the way I should like to characterize it.

In terms of my research, I don't mean to be churlish and in any sense undercut what has been said before but I really think there is nothing I have written I have been proud of, really.

[laughter]

And this has no implication on what has been said. I try to puzzle out things and I write with coauthors and the coauthors have always contributed more. It is really difficult for me but I hope, as things go forward, we will talk about this. It is related to why teaching here has been therapeutic for me.

Teresa Ghilarducci: Hi, I am Teresa Ghilarducci and I am the Bernard and Irene Schwartz Chair of Economic Policy Analysis. I want to give due to my benefactors of the endowed chair but I am also chair of the department, which is probably one of the reasons I am here on the panel. My research engages labor economics and it revives what was called institutional labor economics or what they taught at Columbia [University], the labor problem. It is to look at power relationships, history, class relationships, in trying to explain labor market outcomes. But what I have to do is overturn and criticize neoclassical economics, which has mistaken the observation of the individual worker. We have some of the best data sets in the world in terms of asking workers what do you get paid, how much do you work, and all sorts of other demographic characteristics. The unit of analysis becomes an excuse mistaken for the model so that much of neoclassical and liberal economics is explaining things from worker's choices. And my aim here—and I have been very explicit about it in my labor economics courses here that I love to teach—is that the employer has to be brought into labor economics and labor market regulation has to be brought in. I am actually highlighting the role of the employer and the power relationship.

My research then looks to you all, maybe, as retirement, but it really is about labor and labor force. I see retirement, which was the most significant working class victory in the last 50 years, as this ability to have a dignified retirement, whereas the rich had only had that entitlement. That is the most significant and enduring working class victory. The attack on that is just another way to proletarianize everybody who is not proletarianized. There is a world-wide agenda to proletarianize people over the age of 65 or 70 and delegitimatization that people can retire before they drop dead. So, as many of us defend the weekend, I defend the period of time after a career of work. I am isolated and I do stand alone among many of the economists that I have to engage with.

So why am I here? Much of what I do here in the department may be different than what Ali Khan has done for our department or what Anwar [Shaikh] has done. I have learned everything I need to really formulate myself from Heilbroner from Marx from Keynes and Milton Friedman—and I take this very important political economy framework to be brought to bear with the kind of generative work that I do. I like almost all my research papers because I always start with a question I want to answer. Every paper starts with a question that needs to be answered. Now, do I answer it well? I would be more proud of the ones I can answer better than others, but I always like the question. My books are always about the political economy of aging but the last book was explicitly about the attack on retirement called *The Plot Against Pensions and the Plan to Save Them*. Most recently with Katherine Moos, here, and Tony Bonen, we are writing about the unequal distribution of mortality and morbidity and we are finding out that class has a big effect on one of the most important human achievements, which is to live a normal human lifespan.

MI: As I said before, we have a whole range of paths that we can take forward given our faculty hiring process that is currently underway. So, a general opening question for the panelists is, what might those paths look like? There are a range of disciplines within both heterodoxy and orthodoxy that both faculty and students are interested in and I would like to hear from the panelists, where you think we can improve, where we need to strengthen our reserves.

AK: As I said through the morning and so on, there are a lot of very interesting questions. I was amazed listening. There is this one question: Why is this place unique and singular in this profession? What is it about this place that the profession should pay to preserve? In some sense, why is this place the profession's conscience—*in some sense*? And it is a historical conscience, so I'm coming from there.

Then I hear very clearly two colleagues I admire and respect, so there is this thing between: to what extent do you engage the profession and to what extent do you not engage. Or to put it another way, the way Ed [Nell] talked about, you tear it down. You want to tear it down and you want to rebuild. You do not want piecemeal engineering. You want to wipe the slate clean and start again. That's the one that comes in clear.

At the same time (it was fascinating) you began with the Jacob Marshak and Modigliani and the founding of the Econometric Society and then there's this engagement. So where do you go, do you tear it down or do you engage? This is fascinating.

[laughter]

And you do… both but the word does not come out. I stutter because it is not easy to build and tear it down at the same time. Piecemeal engineering and wiping the slate clean. Both? No. The word doesn't come out, you stutter. So that is one side. Tearing it down is a big proposition; you have to be a bigger man than I am.

So let me go—without answering this question—there are these two dichotomies they are things you build them up to climb and afterwards all dichotomies become irrelevant but you use them to climb. So you climb, I climb the other side. I say engage. What do you engage? You engage the community. Which community? Well, there is this difference. I'm still trying to puzzle out: what do you include and what do you exclude? What is the difference between a society and a community and an economy and a polity? And there you have the Social Sciences.

So how do you extract? How do you represent? How do you take a photograph? How do you write a novel? How do you make a model? How do you model how you make a model?

And so in a sense, for me study of economists is more interesting than the study of the economy. Because for the economy, society has been excluded, polity has been excluded, certain decisions were made in 1950 that you will study this part and Soros will study this part.

Today, somebody talked about *a priori* theorizing. This is again a problem. Will [Milberg] talked of "we don't need theorists *a priori*." So the question is what is theory? I would like to argue you cannot do interesting theory without doing history. So history of thought is a prerequisite; it is essential to doing theory. How long is history? When is it history? Is it the baseball commentator's history? As soon as the error is made, that's history! Or is it the last 20 years, the last 40 years, what is history? So you pick a group of thinkers and you study them historically. You study them in the psychological register. You study registers. I think for this place, [studying historically] is in your blood. And in that sense you are a conscience of the profession.

How you communicate this fact to your administration and explain to them what is unique, I have no idea. I have not had much success at Hopkins. In a sense there are certain forces. In some sense, to tear it down is a reclusive activity. I don't know. Sometimes it is essential! You are racist, I will not break bread with you! Finished! No piecemeal engineering! So you draw a line. So where are these lines? I think a vision will have to be built on that.

EM: I have some thoughts on this, partly because I find the study of real economies more interesting than the study of economic traditions. So I guess we may have a little yin and yang thing going on here.

You segued from the importance of studying history and the importance of studying history of thought and I think it is important to remember that they are distinct. History is important to me, and it is very sad that there is not the same emphasis on economic history that there was when I had a brief history here.

I have a process suggestion perhaps. In thinking about where you need to go, you really need to think about this historical moment. Where are we in the world economy right now? Listening to people talk about the history of The New School this morning, I was struck by the ways in which different changes in The New School were grounded in real historical events. How do we rethink about this moment where we have re-commodified labor and a lot of the de-commodification has become undone. In that historical moment, where do we need economics to go? Is labor still at the core of our economy in the same way that it has been? I have been reading Guy Standing's work—is there so much precariousness that we are at a very different historical moment with respect to labor?

Partly, I am obligated to raise this because I think that is why I am here. Gender and race issues are becoming much salient, not just as a subject of study, but gender is also a tool that we use—a lens not just in understanding women's lives, but the structure of the economy and economic social practices in general.

I'm giving you a couple of things: I think history is important, gender analysis could be important but I think that, whether you accept those concrete suggestions or not, it is really about where we are in this historical moment and building the department on the issues that are important now, not the issues that were important at other historical moments.

RV: While I use the term "heterodoxy" quite liberally myself, I should also state that I prefer to talk about progressive economic ideas and progressive research agendas. Duncan has often expressed misgivings about the word heterodoxy as branding that is both misleading and bad.

And talking about bad branding, the economics department at CSU, where I am, has been plagued by this history where what we could call the "mainstream" associates the heterodoxy with rabid anti-quantitativism, so we definitely need a clear definition of what we are and what we do.

The term heterodoxy is a bit of a grab bag. Again, in the piece that appears in [the 6th edition of] NSER, Anwar Shaikh talks about defining heterodoxy in terms of departures from the core assumptions and, specifically, the ease with which these departures get absorbed as special case extensions of the core model and the absence of general theory departures. So a critique of neoclassical model has to pose coherent and rigorous alternative models. Classical/Sraffian, Marxian, and Post-Keynesian approaches provide such alternative models.

But the formulation of a coherent alternative theoretical framework is not all that is needed. There have been shifts in the terrain of mainstream economics, as it moves towards issues that have been traditionally excluded—whether it be race, crime, norms and social behavior, or institutions. Ben Fine has written about how "economics imperialism" is colonizing the subject matter of other social sciences. And the problem is not simply that rational agent models are being used to study a wider variety of social and political questions. In growth and development literature, institutions now have an important, explanatory role whether it is explaining diverse growth trajectories or the Lucas paradox (of capital flows from 'poor' to 'rich' countries). But good institutions are always defined as enforcing private property rights. To engage with this, one has to go beyond the critique of the neoclassical method to a critique of capitalism itself.

What a lot of us mean when we talk about the future of heterodox economics, boils down to a question of the future for a progressive agenda that is in touch with the real pressing issues of the people. A progressive agenda requires not just a critique of the neoclassical model and an elaboration of an alternative model, not just a push at the boundaries of the discipline to engage with broader questions, but also a critique of the capitalist system. And this raises the challenge of investigating the scope and possibility of other alternatives; whether in terms of policies that may contain the pernicious effects for instance on inequality and on the environment, or in terms of alternative institutional forms of organizations—like the functioning of worker cooperatives.

Turning to the question of what would disseminate progressive ideas in economics, a critical area of reform is definitely undergraduate education. And liberal arts colleges remain a space where progressive economists find a home. But the effective teaching of undergraduate principles and theory courses is a challenge. We need better textbooks and to develop a pedagogy that allows exposure and appreciation of a range of coherent approaches to the same problem. Instead of saying, this is the textbook model and this is how the real world works, it might be useful to begin with the real world problem and then see how this is attacked in different ways. Because I think for progressive economists, teaching and influencing future generations of economists is a really important aspect of the way in which we can make a difference.

Another area has to do with publishing. A lot of our work is excluded from top journals. At the same time, while there are a small number of heterodox journals, they are not always easy to get entry to. It is difficult to find places where our work is going to be published and there is a depressing sense of going down the line to journals that are less and less prestigious. With journal ranking for non-mainstream journals not being particularly favorable, this stacks up to a very mediocre publishing report. In new journals like *Review of Keynesian Economics*, there is a definitely a hope. But you also need a strategy for elevating the presence and the standing of existing non-mainstream journals. I know that the *Review of Radical Political Economy* has been very consciously trying to improve its track record—in manuscript turnover and in establishing a more critical and tight referee process which raises the quality of scholarship in the journal.

What are the areas we can make the most impact and widen the space for heterodoxy? Right after the collapse in 2008 there was a sense, for a brief moment, that a space was opening up for approaches outside the mainstream. By now a lot of us feel that the moment has passed in the sense that orthodoxy is still very entrenched.

I don’t think the ground has been lost completely. We have movements like the post-crash economics at Manchester University and Rethinking Economics right here. You can see a tremendous demand for alternative paradigms and an economics approach that is more in touch with the lives of people. And even beyond students of economics, there is a hunger for analysis that is accessible and relevant to people's everyday concerns. So, while the mainstream is definitely not dislodged and remains as complacent as before, there is a wider space for a progressive research agenda. Progressive economists might still find themselves locked out of top mainstream journals but there is a push from publishers for books on such themes. The problem, of course, is that for jobs and tenure you still need articles more than books but that's an area I won't even begin to address!

So there is a space but to make the most of the space, I think it is necessary to engage with the mainstream at points where it is pushing at the boundaries. Which is not to say that we fight from within but that we are to take serious note of developments even if they fall short of a critique of neoclassical model or of capitalism. Because these boundaries are areas where it might be easiest to open a conversation and establish a presence for alternative approaches. For example, if experimental economics focuses on how the experience of poverty results in changes in behavior, that opens up a space to ask what are the overlying structures, the social and economic constraints and norms under which this different behavior is happening.

At The New School, we focus a lot on theory. Without that, the uniqueness of The New School tradition is lost. But there is also a paucity of good empirical work that buttresses the progressive agenda. Piketty and Saez's work is quite seminal in advancing progressive research and it is widely cited by economists of all views. Such empirical research has the potential for a far reaching impact and would be a crucial place where a progressive research agenda could advance. It does not necessarily have to be big picture issues, though we all love the big picture issues. It could be more focused empirical investigations, on the impact of healthcare, or on the cessation of unemployment benefits which takes you to the policy arena, too. Empirical work and accessible empirical work is really important.

I also think there is a lot of traction for progressive economic ideas in areas of interdisciplinary research; for instance, the intersection of health, epidemiology and poverty, or the investigation of approaches towards mitigation and adaptation to climate change. There are new and emerging areas where progressive economists could play a significant role in shaping the debates.

Finally, a progressive agenda is best pursued where pluralism prevails. Pluralism is fostered where there is a demand for alternative approaches. And the impetus for these alternative approaches does not come necessarily from inside the profession but from outside the profession, too. There is a demand for ideas and resources. Engagement in the public space, popular writing, blogs, policy work, consulting with unions, will definitely be part of what we need to do.

I know this is a moment of crisis, and anyone who is a New Schooler and is associated with The New School will always have a stake in this tradition, in keeping it alive and in widening its scope. But I have to say this. This conference has been wonderful and is a testament to one of the great strengths of The New School, which is its student body. The fact that the students can put on a conference like this and engage with the history is a tremendous resource and hope for the future.

DF: Let me summarize briefly the outline of the approach that I thought through before I came down here today and then make a couple remarks about process as we go forward. I was trying to think, what is the point of The New School department? Why should you have it at all, why should it be here? I sometimes make the point in executive faculty meetings that it is really rather unlikely that a very small, poorly funded, poorly administered university without any particular intellectual leadership or imagination should foster an institution that has a world reputation for critical thinking and education in a field like economics. I was thinking about it and it seems that the dimensions that one wants to think about that are as follows.

One is methodology, which is perhaps especially an area the students are positioned to keep a careful eye on. Some of the lines that get drawn between mainstream and non-mainstream or between orthodox and heterodox economics are methodological. Certain methodologies get very entrenched. They become very much associated with certain conclusions and certain ways of understanding the world. Having space to make any real, critical evaluation and, if necessary, attack on those things is one of the functions of a place like The New School. And I think we have seen that historically. Part of the theme of The New School department in the '30s and the '40s is it had people who, at that time, were methodologically on the cutting edge.

The second dimension that seems to be important to me is ideology. And this is a very tricky thing to talk about but I think otherwise it becomes the elephant in the living room. I think the issue here is really about what Marx called "bourgeois economics." There is a very strong structural tendency in capitalist society to create an economics which explains capitalism to people in an ideologically acceptable way. It is in this dimension of ideology that questions of theory and some of the questions that Ali Khan brought up about wiping the slate clean are important. Obviously, we're not going to wipe the slate clean as far as the sociology of economics goes, the leadership of the big journals, the control of major departments. On the other hand, it is important for people to wipe the slate clean theoretically every once in a while and try to rethink problems from a new, fresh point of view. This problem of ideology is difficult because it gets blurry in the middle since, in fact, mainstream economics is far from ideologically monolithic. There are fragments of the sociological mainstream that are quite uncomfortable with the ideology of other parts. The fresh-water and salt-water macroeconomists can get just as annoyed with each other as the Marxists with the post-Keynesians, or the post-Keynesians with the neoclassical economists. So there gets to be a grey area. What is exactly the difference between the post-Keyenesian economist and New Keynesian economist, where does one draw the line? But I think it is still worth keeping the ideological dimension in mind as you go forward. That does not necessarily mean taking one particular side of the ideological dimension but it just means trying to make choices, personnel choices, with the understanding that ideology is part of the context.

The third dimension that I was going to talk about was quality. This is perhaps the most difficult thing to talk about. That is, some people's work has a kind of resonance, a kind of staying power that is very important towards the shaping of the discipline's history and so forth. The reason for The New School's reputation, fundamentally lies in the fact that it has had the gift of a series of very, very remarkable scholars who have made their mark and in making their mark have also put The New School on the intellectual map.

Let me finish up these scattered remarks by raising a couple of things from my experience. I have now been through quite a few years, quite a few hires, watched several generations of graduate students go on to careers and start to publish, which is a wonderful thing, but it has left me with the feeling that it is very, very difficult to know where people are going to go, to take a person at one time in their career and understand what they are likely to be working on and the level they're going to be working at in the next 5 years. That's why personnel decisions are so very difficult. You always make them on the basis of limited information about what is going to happen in the future rather than what happened in the past. You try to make an investment and you hope that it is going to work out.

Having said that it is very difficult, I still think, that it is better to try to choose excellent people than to bring fields or problems or methods or approaches. In the 1960's and early 1970's there was a boom of interest in resource and environment economics around a Club of Rome report called *The Limits to Growth*. This intersects with the quality issue because that work had some severe methodological and other flaws that made it vulnerable to criticism. The mainstream of that time, people like Bob Solow, for example, used that report to discourage younger scholars from getting interested in resource and environmental economics, often the people that they thought were some of their best. Some students didn't take that advice and continued to do work on problems of resources and environment. But many people were discouraged by the criticism of the Club of Rome report. Now, at that same time, I noticed that Bob Solow, while he was discouraging his graduate students from it, actually was spending quite a bit of time himself on resource and environmental economics. After he wrote papers saying the Club of Rome report was full of hot air, he was going back to see exactly how much hot air and how much substance was in there. Today environment and resource economics is a great big deal and many people have retooled from whatever they had originally specialized in to get into it. So I think it is very hard to pick either fields or people but its better trying to pick people. The future of a small department like this depends a whole lot on the flowering out and the career of individuals.

So that may sound like the personnel is hopeless and practically impossible to do anything about. However, at The New School, even though we have very weak administration and very weak funding, we do have one huge strength, which is our students. We should not underestimate in terms of an individual scholar's career development how much students contribute. Maybe not in a dramatic Road to Damascus moment but through day-to-day conversation and contact and questions that cumulate to a great deal of influence on the way people think about the world and the way they think about their research.

AK: If I may just say a phrase, a lack of servility, a lack of intellectual servility. This to me has always come through with the students here.

TG: At the Center for Economic Policy Analysis that you heard about this morning—which was always imbedded in the department as a way to translate our critique of neoclassical economics and our hopeful proposing of alternative models—imbedded in this department has always been a public intellectual agenda which is that we outreach to policy makers and practitioners. That also includes advocacy groups, labor unions, and just the general public and voters. So we are animated by our real desire to change not just economics but to change the world and make it a better place for humanity. We all share this vision and that always comes through and as a leader (and we rotate leadership) it is very important that we pay attention to the diversity of our faculty and the diversity of ways we undertake our scholarship. Some folks are really good at synthesizing seeing the big pictures, other folks are good at engineering, others are always critiquing. I remember my colleague Phil Mirowski, he wasn't out there doing policy recommendations—he was wiping the slate clean. But what we have done at SCEPA is have this conversation with our heterodox economics to the media and what we found in the past five years is that alternative economic thinking, those key words, to the mainstream, a think tank within a university has huge amounts of power and this is how you get power in institutions; you are in the media and you get grants, you get outside validation. All the time and it turns out, modern economics Ph.D. production requires a material base that is different than it was 80 years ago. And the material base of Ph.D. production just on that scope are undergraduates. Now that's a worthy goal anyway, if you want to make a difference you do it at high school level and at the undergraduate level. And we need better alternative economics textbooks so it is very important part of our mission, maybe we have to do a little more undergraduate teaching but boy is it good for all of us and it provides jobs for our graduate students. So that is a mode of production of Ph.D.s

70% of Ph.D. students are funded by outside grants. There are a lot of grants out there that want interdisciplinary research funded. Actually, these always have economists in them whether the grants are for medical research, anthropology, etc. Our department—we should be a little self-critical here—has to do much more in terms of getting outside validation and getting our students to have outside funding and ourselves outside funding.

The other place that we really have to be self-critical is in our curriculum. You look at our course catalog and we have had the same curriculum, the same names of our classes, for the past 25 years and it is not because we have to teach undergraduates. There is something going on or not going on with us as faculty. We also teach in the same pedagogy. We sit here, the students sit there, and we discuss the truth and the analysis from on high. A lot of things have happened in pedagogical thinking that we have not incorporated.

This is a really great start for what we are due to do in the economics department and that is not only just hiring but we have a 10-year review that we have not done in 15 years. We have to do strategic planning, we have to do external review, but the most important part of that is internal review and you students have started that for our department today. I am very hopeful but it has to be organic and borne of conflict to have a new history.

MI: The second question I'd like to pose, first to Ellen Mutari and then we can open the floor. The best tweet of the day came this morning from Kate Bahn, "If gender and race were incorporated in #NSSRecon from the start, then where is it now?"

EM: I said I am here to represent an illustrious line of feminist economists who passed through here. There are probably many reasons why they passed through and left voluntarily or involuntarily and this has to do with issues like other people wooing them away and so forth. I will simply say that, when I first came here, I was told that this place was a snake pit. And the phallic imagery there was not coincidental. This was a place that used women and spit them out. I had a wonderful experience because I had no expectations of being here longer than one year, so I had the experience of having a wonderful post-doc. I had a great experience because I was not an authoritarian figure in my classes, having just finished my doctorate. They were seminars, they were collaborative, they were wonderful, they were a great experience for me and my students seemed to appreciate them. So I had a great experience, so I am not here to diss anybody. So there is this reputation out there, a particular attitude.

I almost got up and left because I felt that Will [Milberg] said the three things I think are important: pluralism, history, and interdisciplinarity. Pluralism is the one I want to talk about. Again, I think it is important that we scrutinize what we do as a social practice and that means scrutinize the historical and material base of our own practices. There has been some work by Rob Garnett and historian Michael Bernstein about the ways in which the ways we do economics and the way we think about science and social science as being a product of a cold war mentality in which we are warring paradigms and only one can win. I think for many years we thought about economics as: I have the right theory, I will tolerate some other people in the face of a common enemy (which is neoclassical economics) but in my heart of hearts, I know that I have the paradigm that I want to win over all others. I think that is a legacy of a particular historical moment and it is a legacy that we have to move beyond. I do not think that we are the only place [this cold war mentality] exists but my sense is it exists here so we need to be serious about pluralism.

On my office door, there is a *New York Times* article profile from 2008. It was an interview with a geneticist, Eric Lander from the Broad Institute. The call out from the interview with him said, “You can never capture something like an economy, a genome, or an ecosystem with one model or with one taxonomy. It all depends on the questions you want to ask.” I think that that is critical.

We have different theories for different questions and the deep questions you might have about the structure of the capitalist system are different than the theoretical tool kits that you need to figure out what kind of stimulus package you might want to get. And we need all of those tools. Just like a carpenter can’t do everything with just a hammer, we need a variety of tools and we need to respect that we need a variety of tools.

I am somebody who has been active in feminist economics. I’m the president elect of the Association for Social Economists. I’m involved with the institutional economists. I live this belief that there is something to be learned from a variety of different theoretical approaches and I think that is where we need to be. Not trying to figure out what is the one economics, the one theory that we should all be spouting off, but a place where we can respect a variety of different theoretical approaches.

AK: I would like to connect this last comment about pluralism with pedagogy and with Duncan's second point about ideology. So all three in some sense go together and you cannot tackle ideology or good pedagogy or understand what pluralism is all about if you are going to rule out certain "languages" for want of a better expression. And the languages could be the language of measure theory, could be the language of topology, could be the language of statistical theory, and could be the language of the scandal of the speaking body! But you must engage different registers, different languages to, if you like, puncture the ideological thrust and get its cash value out, to emphasize pedagogy and then to inculcate, in one's self before telling others what pluralistic thinking is all about, how do you learn to tolerate the intolerable? We all tolerate the tolerable, what about the intolerable?

Debate on Afternoon Session

Interventions by Mattias Vernengo, Anwar Shaikh, Deb Figart, and others.

Question: I was expecting more of the debate to have opened the question of the future of the department which impacts all of us. I like what you said, but I think it's wishful thinking, and thank you for flattering us, that we are the conscience of the profession and I think there is something important in what you said and it's that we taught history and history of thought, that we put this in a historical context within a profession and that forces us to say something about how to go about hiring: hiring people rather than hiring topics. It might be true that it is a better way of finding talent, you may get more hens from a person than from a field. I think Anwar said something about catastrophe theory, which went nowhere so I see your point. But if the conscience of the profession is somehow satiated to history of thought and history and those things are not taught here, then we are missing something really important.

Question: I'd like to know if people think the history of economic thought and the great philosophers and gender and gender identity and race issues, folks should think about infusing it throughout the curriculum or that segmenting it so that, with a very limited number of faculty, you want to try to cover 20 or 25 fields and field exams. So I want to ask about mainstreaming… a different word than mainstream. Embedding.

Question: It would have been good to invite a couple of New School administrators to this panel. I want to add to the comment that everything we say—good ideas, good suggestions—there is always a money constraint it seems. I wanted to ask if you wanted to try to tie a vision to a way to make this project also feasible money-wise. Does the vision have to be there to get the money? Apparently there is so much demand, but it still seems very tight. And if there is so much demand, are we doing something wrong in marketing ourselves or does the money have to be there in order to have a vision?

TG: I think about it every day. The vision is there and the marketing flows from the vision but the vision has to be flexible, has to be filled with respect, has to be pluralistic, and has to pay attention to what is popular. The courses do have to be refreshed, the titles have to be refreshed, and how they fit together has to be part of what faculty talk about all the time. Anwar gave us a couple of visions. One is a vision of a faculty that did a lot more teaching. It was not that we did the same amount of teaching and then we got diverted from Ph.D. classes to our masters teaching and then undergraduate, it was that we went from 5 to 4 classes and then we did a 6th in order to get our leave.

At Notre Dame, a place I was before [coming to The New School], we changed the department and the school overnight when, to get a leave, we had to submit a proposal to get outside money. We did not have to get outside money but we had to conceive of a project, put it on paper, and engage the scholarly community in order to get our reward: our time off. And all the sudden all these young women faculty members who had been invisible in the patriarchal institution rose up with their NEAs and their NEHs. All of the sudden, those who we hired for affirmative action became the stars. Student enrollment and graduate student applications went up in value. Why? Because somebody was a referee of a proposal that came from Notre Dame and that referee said, “Hey there’s something interesting happening at that place.”

The New School’s faculty is better than the one at Notre Dame and the Notre Dame students were fully funded. If only we engaged in our scholarly community and wrote many more grants, even if we didn’t get them, the reputation and the marketing would happen. The vision is there, everyone is hungry for alternative economics that is rooted in very good methodology—we heard that from the policy panel. So we have to do a lot of work internally. The administration is very supportive of anything that brings in high-quality students. We happen to be at this moment better off than most of the other programs at NSSR. Much of it is because we are going back to our original mission of having a relevant master’s program that helps adults learn and looking at our demographics, it is going back to more people over age 40 that are going to want to come back and learn economics, whether its non-credit, master's, or even a Ph.D. Our vision has to be in tune to our historical moment but so does our business plan.

DF: I hate to say this but there is no budget constraint in this department. That argument is largely smoke and mirrors. There is a fiction inside this university that the department swallows that there is some kind of rational resource allocation mechanism. There is not. The New School operates purely by redistribution of surplus. The scale of the surplus is so large in comparison to what we want to do in this department, that there is effectively no budget constraint. The budgetary policies and procedures are so opaque and mystifying that it is a mistake to take them very seriously. One great thing about the University in Exhile was that it was very cheap. Once you got people across the Atlantic Ocean, it was the depression and prices were really low; keeping them was very, very cheap. Now it is not so cheap to run a first-class research and scholarly institution but where the money comes from is going to be from the trustees or from presidential or provostial leadership that decides that they want to put some money into department building. I would also say from my observation that the New School administrators and Trustees have no interest in the economics department's self-defined mission of training Ph.D.s. The economics department at the NSSR, there is no constituency for it. What they are interested in is a certain amount of glamour and excitement that can be generated from having certain people on the masthead. So I am more than a little skeptical about the claim that the budget constraint is the problem.

AK: There is a lot of parallelism with John Hopkins: the glamour, the charisma, you want a superstar. As an administration you have your own objectives. Your objectives are: you can go to a university at a higher level and say, "Look, I got these stars in this place!" now you are on to the next one. Again, I may be being jaundiced here but we know game theory and these guys [in the administration] are all pursuing their own objectives.

RV: The budget constraint is, a large part, this administrative bloat. That is where the money is going rather than having a lack of money. Having a structure where there is transparency and some say for the people who actually have a stake in how that money gets allocated. There is, in the tradition of The New School, a unique legacy which is not just restricted to the U.S. but a legacy which draws people from around the world, the developing world, to here. This tradition is this theoretical contribution, which is not monolithic, which is diverse, but a tradition that you can draw on if you want to pursue a progressive research agenda. This is one of the few places that you get that theoretical tradition and that has to do with the vision and that also has to do with this notion of pluralism. Without having different ways you can tackle issues, without having links to the history of economic ideas, the history of our discipline, that is what keeps the discipline alive. We are never going to take over the mainstream and that is not even the objective. Progressive agendas are best served when there is a variety of approaches and tools out there. You don’t have to use all of them; you use the approach you think is relevant to the problem you want to pursue. But in the absence of that environment, none of the currents in heterodoxy would survive.

Question: I want to remind people that one of the virtues of being in this peculiar place is that you can walk across the hall and talk to first class philosophers, historians, political scientists, and we encourage people to do that. We have always encouraged that from the very beginning. So, we do not necessarily have to internalize all of that. I have attended lectures by Eric Hobsbawm, Kerry Anderson, Robin Blackburn and talked to them. Not many institutions allow you to do that in this kind of proximity and encourage students to do that. That is something to keep in mind; that is a positive thing.

I agree with a lot of what Ellen said but I have to say that I do not agree with the toolbox thing but perhaps that is because I used to be an engineer and it seems to me that there are no tools that contradict the laws of physics or the laws of gravity. Tools have to have a certain consistency and coherence and that is what I have understood to be the function of vision.

Let me give you an example. All of us today are struggling with the idea of what kind of reaction should we have to the crisis and what should we say about austerity. What we say about austerity is not just a matter of measuring the impact, because that impact depends on how you look at it but also trying to understand the theoretical foundation. I am not a Keynesian but I happen to think that [austerity] is a bad way to proceed though I would not argue that on a Keynesian basis. I think the pluralism that we need also has to be at that level, not just at the level of concrete practices and policies and connections. Some people do that, other people like myself do not because I choose not to in order to do something else, which brings me to the last point that Teresa raised.

The outside funding idea is a great idea but it is a little bit cruel. I have been here 40 years and I have gotten one outside grant and it is because INET was created in response to the economic crisis. Yes, I could have worked for the UN. Yes, I could have worked on labor markets. I could have done stuff on inequality but then I could not have done what I thought was important. I forwent that precisely because I did not think that and I have worked with students who did not want to do it either. So it would be wonderful if we as heterodox economists could get grants from the National Science Foundation, grants for basic research but they do not do it for our kind of research and if we are to get that kind of money we are to become them. I think that is a mistake. I think it's better to be poor and do what you think is right than to be funded and do what they think is right.

Question: Ramaa said in her talk that there was this moment after the crisis and it seemed like an opening for alternative theories but that the moment has passed. But to me that seems a tad pessimistic. I would like to present the more optimistic side of that story because I think things have fundamentally changed since the crisis and the possibilities for doing alternative economics have opened up maybe not dramatically like real economy came down but slow tectonic movements. You mentioned the post-crash Manchester society. That society is not unique, that is one of fifty societies that have sprung up all over the world. Maybe we do not notice it here because we have always been criticizing neoclassical theory and maybe the faculties of other universities are slow but among the student bodies at the mainstream universities and maybe in the general public there are some very different expectations. I am engaged with rethinking economics and I talked to this postgraduate society part of rethinking economics almost every week. We have so much media attention, we have so much student demand. They write us letters from India, Brazil, Germany, France. Keith is involved in writing a manifesto, I think there is a lot of stuff moving. There is no reason to be pessimistic at this moment. The challenge now is not to let all of what we have been advocating for many years happen without us taking part of it. [More information on Rethinking Economics at RethinkEconomics.org]

Question: I am an alumn of The New School and the panelists got me thinking about my time and why I came here. I had a fully funded Ph.D. program in D.C., but midway through the first semester, I realized this was not what I wanted to do. So I gave up my fully funded position and came to The New School because there were questions I was asking myself and I saw there are people here asking these questions. I was then fortunate enough to get some form of financial support at The New School but I think I would have come also even if I had not been financially supported. When I came, I did not know what the vision was. I do remember there was vision that was advertised to prospective students and something like that I am sure is very helpful, but I am not sure the department needs to have a vision as long as there are a couple interesting thinkers who you think you can learn something from.

Question: The reason that I came to The New School is that The New School is science-making, not the ready-made science off the shelf. I totally agree that you can create science with or without vision. Where do you see The New School within the science-making framework, where do you see The New School within this framework advancing something new?

Question: I have one comment and two brief questions. My comment is, maybe the curriculum of the department looks much more conventional to what it looked in the ‘70s but nonetheless, before I came to The New School, I took standard neoclassical micro and macro and everything. Then I came here and took the Ph.D. micro and we started with Smith, Ricardo, and then VonNeumann, so we spent half the class doing classical political economy—the same thing with macro with Lance [Taylor]. We did a lot of history of economic thought and this is one of the things I enjoyed the most. Even if the curriculum looks more conventional, it is still a very enjoyable experience to take a class at The New School.

My first question is: If there is no budget constraint, why do we have 5 members of the faculty and not 15 or 20?

The second question is related to what Teresa said, I don’t know of you refer to Notre Dame before or after what happened—it is a very sad story of what happened to Notre Dame. To what extent is there the danger at The New School of what happened to Notre Dame, of turning the department into a neoclassical department? I think this is a question.

Question: Picking up on what Duncan said about choosing people and quality people over topics. If we do not have the topics we need being taught here, no budget constraints, and we could hire, how can we at this point hire quality people to teach what we need? Do we have to choose between the things that we need to be taught here and someone that is really good and consistent? How do we make this choice?

EM: One of the interesting questions is how you define quality—do you just know it? I guess I am not seeing that dichotomy. How do you know who is being innovative and cutting-edge if you do not know, and I am going to repeat myself, where we are at this historical moment, where the department is going, and where the economy is going. I am not sure how we separate quality from those questions.

The other thing I wanted to throw in, as somebody who teaches at a place that could not be more different, in many ways, from The New School, all of these debates you are having sound so familiar. Who have we been? Where are we going? Are there really budget constraints or is it simply misallocation of resources? In our case, of course, we absolutely can look at how much less money we are getting from the state and it is pretty obvious that there are diminishing resources however they may be misallocated. So just remember that it is not that you are alone in struggling with these issues. They are very difficult and as an outsider there is a lot for which I do not have enough perspective.

DF: The constraint is two-fold—in part it's a supply constraint. Not just who is out there—but who might be interested to moving to New York and teaching at The New School. That is probably a more important constraint than the budget constraint on The New School side. The other thing we ought to be very realistic about, is that the department has had a great deal of disunity a great deal of trouble making up its mind whom it wants when we have had the opportunity to make choices. Disunity also militates against our getting increases in the budget, and authorization for searches. I think that touches on what Ellen asked, who is doing exciting, good work? And in a way, this is where we earn our keep as professionals. What we can do—that presidents and trustees and provosts and deans and review committees can't—is make judgments about the quality of work: what makes sense, what has depth, what has consistency, and therefore what has staying power.

And as a final thing, I don't want to throw cold water on the enthusiasm behind Rethinking Economics but I do want to point out that this New Economics movement does face a kind of crisis of substance. There is certainly immense demand for new economics. Everyone hates the old economics, that's not the problem. The question is what is the substance of new economics? Is it neuroeconomics? Is it nonlinear dynamical economics? Is it Minskian financial fragility economics? All of these ideas, and many others, are floating around.

I think during the crisis there was a reserve of quite good and interesting work which had not been very visible but was able to surge into visibility because of the circumstances. But I'm worried that impulse is a bit spent and there may not be too much left in the cupboard. Maybe I am wrong about that but somebody has to make sure there is something in the cupboard that has real substance.

RV: I want to speak a little more on the moment because something definitely has shifted, one does not deny that. But it has not shifted within the profession. What shifted has shifted outside which opened up a space. But we have our work cut out in trying to occupy that space and trying to fulfill that demand. While there was some soul searching within the mainstream about their models and how they failed, that disappeared really, really fast. That self-reflection in the mainstream went away. So the hope that now there is a different discipline opening up, that will take much more time. Which does not mean that you should be pessimistic. Academia is a part of that social structure so we are part of that, too.

AK: I want to go at it again. You see, I want to put before you two things: one, my conflict and, second, the comparison between this place and Hopkins. Anwar said, "Look, I did the humbug production function. I could have done a career like that. I chose not do that." So I think, "ahh, there's a brave man. I just went on extending." This is my genuine thought; I am sharing with you sincerely! So when I say I am not proud of my work, it is not to make a small point; I am talking about the internal struggle. I always knew [Anwar] as a brave man. Ed [Nell] also.

And now Teresa says, "Look but we must get grants and have to talk within the thing." There is this tussle between to what extent I am working within the system and engaging within the system and to what extent I am cutting myself off from the system. To the extent that I am dependent from the market, whatever that market is—whether it is foundations, grants, World Bank, IMF, whatever. To the extent that I am not independent from the market, my independence is lost. When Duncan talks about ideology, this conflict is there, there is no easy answer here. You have to make this struggle, you have to get this balance.

I say I am alone at [John Hopkins]. It is a mainstream department. It has aspirations. It wants to move from top 18 to top 17, top 16. There is a motivating engine. I do not want to talk against those who are putting bread in my mouth but, nevertheless, I see the servility. The graduate students are terrified. They are checking google citations. How can they get a job? How can they work within the system? I can't teach anything! Why? Because, all the students want to know is to what extent will it help them get a job? How will it translate in the job market? I am now working within the system.

Here [at NSSR], I am not alone. So when I say therapeutic it is because here are crazy guys! There is a certain bravery, there is a certain disregard. I work on the continuum, if you like. Within the continuum between Anwar and myself to some extent there is much more bravery here; we are going to disregard. So, in front of the president at Hopkins I said the problem is to keep the market at bay and to allow ourselves to grow as scholars, as independent scholars. After the committee, he put his arm around me and said I never met an economist who wanted to keep the market at bay. And to the extent you are trying to work with the grants, Anwar said he got one grant in 40 years. That's one more than I got! But I am working with the mainstream. So it is not monolithic.

TG: This is not the first time that I have been misunderstood, but I have been misunderstood. I do not care that we get grants, I really care that we apply for them and put our crazy ideas in front of the scholarly community all of the time. So I just want us to put our ideas out there all the time. That's marketing.

AK: Oh, indeed. Yes, you should, you should.

MI: I'd like to thank all of the panelists for being here and all the distinguished guests.

Watch the panel here: www.youtube.com/watch?v=aufQPgR0nEQ&feature=youtu.be

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