Macroeconomic Dynamics, **21**, 2017, 1790–1810. Printed in the United States of America.
doi:10.1017/S1365100515001066

MD Interview

AN INTERVIEW WITH NEIL WALLACE

David Altig
Federal Reserve Bank of Atlanta

Ed Nosal
Federal Reserve Bank of Chicago

A few years ago we sat down with Neil Wallace and had two lengthy, free-ranging conversations about his career and, generally speaking, his views on economics. What follows is a distillation of these conversations.

Keywords: Monetary Economics, Research, Teaching, Policy

1. INTRODUCTION

The lore of the Cleveland Fed research department famously includes (and maybe even starts with) an episode in the late 1980s when Neil Wallace participated in a conference on money demand, organized and hosted by the Bank. At the time, like many other Federal Reserve research departments, the Cleveland group was in full-trot transition from a team devoted largely to business conditions analysis to one focused on policy-related research that could stand up to the highest professional standards and scrutiny. As a member of the pioneering research outfit at the Minneapolis Fed, and a founding father of the rational expectations revolution (which at the time still felt quite young), Neil Wallace was the honored guest at the conference.

Befitting his status, Neil was seated at the head table for the conference dinner, along with the president, Lee Hoskins, and the research director, John Davis. In the course of the evening's conversation, Neil was asked what policies he would be pursuing if he was in charge of the Fed. His first response (of course) was that he would never take such a job. But his hosts pressed on, insisting that he suspend disbelief and deliver a policy conclusion.

"Well," Neil eventually replied, " I guess I would set the money supply as a constant fraction of the government debt." After a few seconds, his table companions pressed him further on why that would be his preferred course.

The views expressed here do not necessarily represent those of the Federal Reserve Banks of Atlanta and Chicago or the Board of Governors of the Federal Reserve System. Address correspondence to: Ed Nosal, 230 S. LaSalle St., Chicago, IL 60604, USA; e-mail: ed.nosal@chi.frb.org.

 1365-1005/16

" Well," Neil even more eventually replied, " it turns out some models are easier to solve with that assumption."

When we reminded Neil of this story during the course of our interviews, he responded first with a silent bemused look, and then entered his silent tilted-head/raised-eyebrows/I'm-thinking mode. After quite a few moments of this—Neil likes to note that people have told him even his pauses have pauses—he began to describe to us the class of models and problems he was working on at the time, and why a constant money–debt ratio seemed like a plausible policy.

You will read a lot of that sort of thought process in this interview. What stands out to us in this conversation, as with every one we have ever had with Neil, is how clearly each idea emerges from the intellectual platform on which it is built. As Neil describes his career you will recognize the tale of a serious scholar, someone not just doing the work but continuously thinking through how the work ought to be done.

An old joke asks how many economists it takes to change a light bulb. The punchline is that economists don't change light bulbs. They screw one in during graduate school, and when it burns out they just sit there in the dark. You will find none of that in what follows, which is a distillation of two lengthy, free-ranging conversations that we had with Neil at the Federal Reserve Bank of Chicago several years back. The time devoted to these talks reflect both Neil's incredible generosity and the unwillingness of the interviewers to cut short any opportunity to learn from a man that we both, with some amount of self-flattery, consider a mentor.

Very few people have a truly coherent view of their own intellectual history, and the capacity to articulate their mind's journey. Fewer still have lived that history in such a remarkably consequential period for their chosen fields. Neil Wallace is one of those few.

Neil's vita appears at the end of this article.

2. GRADUATE SCHOOL

Neil Wallace: I went to Columbia as an undergraduate and then the University of Chicago as a graduate student. I began my first job in 1963, when I was 24, at the University of Minnesota. A lot has happened since then. For the things that I think about, there have been two main developments over this time. One is the development of rational expectations equilibrium and the other, more generally, is the notion that we ought to build models in macroeconomics and monetary theory in a coherent way.

As a graduate student, did you have a feeling that something along these lines was about to happen?

In my first year at Chicago, Harry Johnson taught us macro, and it was pretty strange. I remember Harry coming into each class with these voluminous handwritten notes, and reading to us. The course met twice a week in two-hour sessions, and I don't recall a break during the two hours. So we were sort of sitting there,

https://doi.org/10.1017/S1365100515001066 Published online by Cambridge University Press

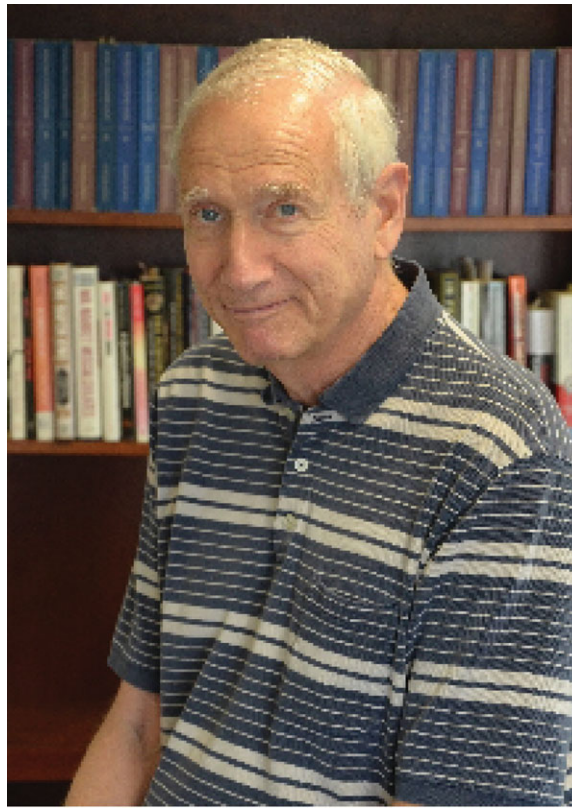

FIGURE 1. Neil Wallace in his office, 2013.

listening. I remember that Harry almost never used the blackboard. His approach was the history of thought of macro. I don't know how far back we went, but I can certainly remember trying to read some of Irving Fisher. Toward the end of the course, we read Franco Modigliani's 1944 *Econometrica* paper. I can still remember my feeling that this paper was the Promised Land. I got to know a few graduate students when I was an undergraduate at Columbia, and I remember going back when I was on vacation from Chicago, and running into some of them. They were still trying to do macro using the equation of exchange as their framework, while I had Modigliani's framework. It was an eye opener for me at the time.

Can you remember exactly what it was? What conception of macro did you have before Modigliani?

Well, I really had no conception. You know what the history of macro is like. Prominent economists, like John Stuart Mill, wrote a micro book and a macro book. If you pick up the micro book, you sort of recognize what is going on, even though it's very primitive. You pick up the macro book, and you don't know

https://doi.org/10.1017/S1365100515001066 Published online by Cambridge University Press

what's going on. Alfred Marshall was the big exception. And it wasn't an accident. According to what I heard—maybe from George Stigler in a graduate course in the history of thought or from Milton Friedman—Marshall had the view that macro was not well enough developed to call for an exposition like the principles of macro. So when people want Marshall's views on money, they read his testimony about the Indian monetary system before various commissions or rely on reports from those who attended his lectures. After looking at this early unrecognizable macro stuff, in Modigliani you see equations whose number is equal to that of the unknowns, and the equations seem to allow you to talk about things the way people still do today. So it was a real eye opener.

One of the best things I learned at the University of Chicago was some econometrics. I can't remember who taught us this course, but the book was by Theil, and it wasn't an econometrics text. The book had a huge appendix, with the first exposition of two-stage least squares. So, somehow, IS–LM, macroeconomics, and econometrics were fitting together. That's where I was in graduate school.

3. MOVING FROM MODIGLIANI TO MODERN MACROECONOMICS

Can you remember when dissatisfaction with the Modigliani program started to bubble up for you?

It wasn't until I saw a working paper version of Lucas's " Expectations and the Neutrality of Money." Before that, Tom Sargent and I were trying to build models which were elaborations of IS–LM. I won't speak for Tom, but I was stuck in a static mode. We had the view that things depend on people's expectations of inflation, and we wanted to endogenize the expectations. Our approach was to be clear about stocks and flows in IS–LM. But when you start thinking about that, you're naturally led to think about some kind of dynamics. At the time, I was also working with John Kareken and Tom Muench. Muench was a good operations researcher, so to him the idea that an equilibrium condition is a difference or differential equation didn't seem strange. But to me, that was just way out there. So although we were thinking about dynamics, it was in a very static way. Then I saw this paper by Lucas; I don't know why I read it, or tried to read it. In part, I did because Bob had been a classmate at Chicago and I had a high opinion of him. I picked up the paper, and it's talking about people, two-period-lived people. What's that? There are no people in macroeconomics! I pretty much saw that the Phillip's curve ideas that Sargent and I had been working on were a dead end. That paper [Lucas (1972)] raised the standard, and there was no turning back.

The Lucas '72 paper had a huge influence on me in all sorts of respects. First of all, in terms of style and a way of theorizing, that pretty much marked a break for me. And the paper made me think about what money was doing in that model. That's probably my first substantive, what I would call serious, thinking about money. So it was hugely influential. Recently, I was at a little conference in Phoenix, and Lucas gave a keynote address. In talking about the recent financial crisis he, in part, used the equation of exchange as a framework. He said, "The

https://doi.org/10.1017/S1365100515001066 Published online by Cambridge University Press

profession is struggling with trying to figure out how a change in nominal income is split between prices and real income," and " I wasted ten years on that problem." I don't know who was sitting next to me in the audience, but I said " Well, maybe, but he got a Nobel for it." But anyway, that really is the way he feels. He wasn't joking when he said, " I wasted ten years on this."

Do you still think it's an interesting problem?

Yes. I'm not ready to dismiss outright the ideas that are in Lucas's '72 paper. The idea that it is unreasonable to have lags in seeing the money supply because you can look up the St Louis Fed's estimate of it is, I think, a misguided criticism. Lucas's paper is a model of aggregate demand shocks and how they impinge on the world. The St. Louis Fed numbers are only part of the story about last quarter's aggregate demand shock. So I think some of the ideas in the Lucas paper shouldn't be dismissed.

So the bell went off after you read the Lucas paper. You obviously had a stockpile of things that you were working on. Were you in conflict for three or four years trying to finish up these (pre-Lucas '72) papers?

I don't think I was in much of a conflict. I sort of abandoned that stuff. In 1975, Tom and I published a well-known paper. That was Tom's effort to get a little something out of a failed research effort. But I was done. Lucas's paper got me thinking about money and, for the most part, abandoning directly thinking about business cycles. Since money was the only asset in Lucas '72, there was no issue about why it was held.

What was your thinking at the time about what would be a good way to think about money?

For a while there, I thought the overlapping-generations model was going to be the model of money. My attitude was to try to put that model to work in a number of applications. A classic problem in monetary economics, identified by Hicks back in 1935, is why money exists when there are higher-return assets around. I had this idea that if we legally inhibited intermediation, then rate-of-return dominance would emerge. I wrote a couple of papers about that using an overlapping-generations model. In retrospect, I think the profession was right to reject that view of money.

Because it didn't deliver on Hicks's central question?

I don't think it dealt with Hicks's question very well. But the overlapping-generations model really failed miserably in explaining why economies have money in the following sense. People have had in mind for, I think, a couple of thousand years that something like money is a useful thing. It helps an economy achieve outcomes that it couldn't achieve in its absence. So it's natural to have as a goal of monetary theory to invent settings where that is the case. How are you going to do that? There are two versions of answering this question. One version has money as one of a number of devices for achieving allocations, and it's not the unique one. I have as a goal the second and more ambitious version. I want

money to be the unique way to achieve allocations. People looked at Samuelson's overlapping-generations model, and gave the first answer. Money was one way to achieve the allocation. But it was hard to make the claim that it was a unique way to do it. In the early '90's, Kandori wrote a paper about implementing allocations in overlapping-generations models. In his paper, he said that if we can remember what people did when they were young and reward them when they are old contingent on what they did as young people, then we don't need money. We can achieve everything that you could achieve with money. This record-keeping idea is much older. I attribute it, mainly, to Joe Ostroy in his thesis and in some papers that he published in the mid-'70's. Even though I had discussed Ostroy's papers in classes, I didn't see their significance. Although Ostroy, a paper by Townsend in the '80's, and Kandori's work predate Narayana's paper entitled "Money is Memory," that paper brought home the idea that money is a way to record past actions.

This seems to me to be a great idea, and something we ought to be working on. It fits within economics in general. When I teach undergraduates money and banking, I say, suppose you're in class and you don't have a pencil. So you turn to your neighbor, and what are you going to say? You might say, " Do you have a spare pencil?" Suppose the person has a spare pencil. Do you offer to buy it? Or do you say, " lend me a pencil." I think you would say, " lend me a pencil," because you're in a continuing relationship with this person. The idea is that we use money with strangers, and we don't with people we know.

I guess some of us have tried to hire our kids and to give them allowances to get them to behave. But by and large we don't use money in the family. You don't use money in a small Amish community or in a kibbutz. We know what people have done. Maybe we don't use money in the same way in an organization either. So I think it's a big idea, and a good idea. It just wasn't in the overlapping-generations model.

4. DEVIATIONS FROM THE ARROW–DEBREU MODEL

Obviously, the notion of hidden trading histories seems very natural. But, given technological advances, it seems to be becoming increasingly less natural to think in those terms. Money is becoming less important. So do you have thoughts about this?

Sure. I think that idea should push us in the direction of thinking about payments instruments in terms of the informational requirements needed to support them. We use credit and debit cards most of the time. There's an information structure that supports that. And that idea is related to what we should take to be the base for the inflation tax. It ought to be currency, not some broad notion of money. It may be true that your checking account has a low interest rate, but there are lots of reasons why that might be. An obvious one is that the payment of explicit interest would be part of your income for tax purposes, and giving you payment services for free is something you don't have to claim as income.

One of the best models we have of demand deposits, the Diamond–Dybvig model, says that you're sacrificing some rate of return for the right to spend early if you want. This model predicts that demand deposits will have a low rate of return in exactly the circumstances under which the equilibrium looks like an illiquid banking system. So, when we think about inflation taxes, we're thinking about currency. And once we think about currency, where does that push us? Well, obviously, it pushes us to thinking about the underground-economy role of currency.

All of this goes along with the idea about trading histories. Certainly we can imagine the world going cashless. Is a cashless world going to be the Arrow–Debreu model? Well, for lots of reasons, maybe not. In particular, these ideas about circumstances under which money is essential always assume that people are not committing to future actions. That's always there in the background, as it probably should be in everything we think about. But that's not the Arrow–Debreu model.

In a world without cash, what would monetary theory be about?

A few years ago, I started out a graduate course in monetary theory by handing out a table of GDP by sector of origin. I asked the students " which of these sectors fit comfortably as an activity or an industry or a good in the Arrow–Debreu model?" You can do the Arrow–Debreu model very crudely, and maybe shove many things into it, but not very easily. One sector which does not fit into it is FIRE: finance, insurance, and real estate. Retailing is another such sector. We need new models for those sectors and, perhaps, when we have them, we will see a relationship to monetary theory. By the way, I wonder whether Greenspan, who probably was a big believer in some version of the first welfare theorem, recognized that he was presiding over a financial system which doesn't exist in the model to which the theorem applies.

You just argued a little while ago that incomplete trading histories is the best story we've got to describe the frictions that give rise to money. But maybe it is only a piece of this big non-Arrow–Debreu world?

I don't know whether this imperfect monitoring idea is the whole idea, but I think it's a big idea in thinking about how we transact. But your question brings to mind another question: what is the role of pairwise meetings in monetary economics and elsewhere in economics? I like pairwise meetings. In some ways, they certainly fit well with imperfect monitoring—you don't know about things occurring across the street right now because you're not there. There are a lot of things in the past that you don't know about because you weren't there and haven't been told about. The idea that we're not all costlessly and immediately connected has a lot to do with those big non-Arrow–Debreu sectors of the economy. I've done some applications of pairwise meetings models where this lack of connection is a big deal. One of my favorite papers is something called " Float on a Note," written with Tao Zhu, which takes up a seeming paradox related to the profitability of bank note issuing in private banking systems. In a very general way, float in the financial system is about people not being connected.

5. TEACHING

How do these ideas about money, informational frictions, and Arrow–Debreu influence your views on teaching economics?

If we were to construct an economics curriculum, independent of where we've come from, then what would it look like? The first physics I ever saw was in high school. I went to the Bronx High School of Science, so you might think they would have had some good physics there. The year I took it somebody was on sabbatical, and I was taught physics by someone who was a Latin instructor. He was struggling along with us. I can vaguely remember something about frictionless inclined planes, and stuff like that. So that's what a first physics course is; it's Newtonian mechanics. So what do we have in economics that's the analogue of Newtonian mechanics? I would say it's the Arrow–Debreu general competitive model. So that might be a starting point. At the undergraduate level, do we ever actually teach that model?

That means that you would not talk about money in your first course.

That's right. Suppose we taught the Arrow–Debreu model. Then at the end we'd have to say that this model has certain shortcomings. First of all, the equilibrium concept is a little hokey. It's not a game, which is to say there are no outcomes associated with other than equilibrium choices. And second, where do the prices come from? You'd want to point out that the prices in the Arrow–Debreu model are not the prices you see in the supermarket because there's no one in the model writing down the prices. That might take you to strategic models of trade. You would also want to point out that there are a lot of serious things in the world that we think we see that aren't in the model: unemployment, money, and (an interesting notion of) firms aren't in the Arrow–Debreu model. What else? Investing in innovation, which is critical to growth, isn't in that model. Neither is asymmetric information. The curriculum, after this grounding in the analogue of Newtonian mechanics—which is the Arrow–Debreu model—would go into these other things. It would talk about departures from that theory to deal with such things; and it would describe unsolved problems.

So that's a vision of a curriculum. Where would macro be? One way to think about macro is in terms of substantive issues. From that point of view, most of us would say macro is about business cycles and growth. Viewed in terms of the curriculum I outlined, business cycles and growth would be among the areas that are not in the Arrow–Debreu model. You can talk about attempts to shove them into the model, and why they fall short, and what else you can do.

Another aspect of macro is some special tools used to analyze intertemporal models. At Penn State, the beginning part of the graduate macroeconomics curriculum is essentially only that. In some programs, there may be a mixture of tools and substantive issues.

My vision of a curriculum differs a bit from the parallel treatment of micro and macro that we see at all levels of the economics curriculum. That parallel treatment is a bad idea. The substantive issues in macro—business cycles and

growth—should be what are called field courses, parallel to courses in industrial organization and international trade.

I can remember a little debate we had in the Minnesota economics department in the 1970s. We had principles of economics courses, and one of the courses had the number 1, and the other had the number 2. There was no prerequisite ordering over these courses, but one had the number 1 and the other had the number 2. At the time I got there, macro was number 1 and micro was number 2. I was among those who said, " even though we're not willing to put a prerequisite ordering on these courses, why don't we switch the numbers?" to at least give some hint that a little background in micro might be helpful background for macro. Not everyone liked this idea of switching the numbers. It was noted that the then current labeling was adopted to encourage interest in economics by having them take macro first.

As part of the discussion, we talked about how you don't need micro to do the quantity theory of money, IS–LM, and so on. That's the argument for teaching micro and macro as parallel offerings. I don't think that's the impression that we want to impart to students. Just because business cycles and growth are important doesn't mean you start out with discussions of them. Cancer is important, but it's not the first biology course.

It sounds as if you're going to tell the students, "This is medicine, you have to take it, and then you'll get to appreciate the economy through economics."

This goes to issues in the philosophy of science. It goes to issues like, are there facts that are distinct from theories, or are there just theories? I don't think we're doing students a service by, say, teaching beginning students supply and demand and not pointing out the sense in which this is a theory. It's not truth, it's a theory. There are these old debates in economics about whether supply/demand is empty, but it's not empty. Supply/demand is the general competitive equilibrium model, with its first welfare theorem, and so on. It's not empty, but to teach it as if it's the truth is in every sense a bad idea. Yes, students want to know about the economy, but somehow, at some point, you have to dispel the notion that you're up there dispensing truth.

6. THEORY AND POLICY

You seem to believe that theory informs us about some practical things. For example, Diamond–Dybvig appeals to you as more than just a nice theoretical construct. It appeals to you because it addresses something that is pertinent. So what is it?

Adam Smith wrote a chapter on banking. In it, he said that banks should try to match the maturities of their liabilities to the maturities of their assets. In modern language, he would have been saying banks shouldn't take on term-structure risk. Friedman advocated 100% reserve banking. Today, there are advocates of narrow banking, which is much the same thing. Is there a theory underlying those claims?

I think not and I think that Diamond–Dybvig throws light on that policy issue. It comes out on the side of those who say that it's the function of banks to borrow short and lend long. In a dramatic way, the model provides new insights into that issue.

This gets back to your earlier argument that maybe we should just be thinking of macro as a set of substantive questions.

Right. I think policy decisions both at the individual level and government level are, in a general way, a good entry into motivating what should be taught. So, for example, you can motivate a finance course, or at least some of it, by saying, "Well, you guys out there and your parents are making some portfolio decisions. Let's think about the elements that go into that."

Do you think economists as a group have made a prominent or useful contribution to public policy?

I don't know. Some time ago, I read a book of essays by Lewis Thomas, a very well-known biological medical researcher. In one of his essays, he says that he rarely picks up an issue of the journal *Nature* in which he doesn't come across some article that completely changes his mind about some natural phenomenon. If we pick up an issue of a leading economics journal, it's a little hard to say something analogous. But I think we have made some progress. I remember reading some articles from the 60's by some well-known people discussing Ricardian equivalence issues. I think the profession is much clearer about that today than it was 40 or 50 years ago. Also, you would be hard pressed to find an economist that believed in a permanent inflation–unemployment trade-off today.

It would be nice to be able to point out the big discoveries in economics which you might put side by side with a list of big discoveries in biology, medicine, physics, or chemistry. It's not so easy to make that prominent list for economics.

Is this because the problems in economics are just a lot less significant than in other areas? Or are answers just a lot less certain in economics?

So here's an issue. Michele Boldrin and David Levine have been working on a big endeavor for a number of years. They advocate that we eliminate patents. I really applaud them for taking up this issue. We've had studies going way back which suggest that most of growth is attributable to innovation as opposed to changes in inputs of labor and capital. So you'd think that finding would lead people to turn to the question: How does a society organize to achieve innovation? Most economists have not addressed the question, but Boldrin and Levine have.

They're two very smart guys. They started out trying to argue that they discovered a flaw in the theory that defended patents, and that unfettered competition might lead to innovation. I think they've backed off that claim. Now they seem to have adopted the view that whether patents are desirable is an empirical issue. They have a book that largely looks at case studies and says that there's enough advantage to being the first mover that you don't need patents.

How society should organize to achieve the right amount of innovation or the best bang for the buck is a hard question. It's an instance of an important question that is an economics question, and one that we're not very far along in answering. Nailing that question would be analogous to discovering penicillin. But can we imagine nailing it? Maybe, but I'm not sure. In any case, there are questions in economics that if answered in a convincing way would rank with major discoveries in other fields.

If you were a policy maker how you would think about policy? We've observed that you have been kind of silent about specific recommendations. Why is that?

This is sort of related to things we've been talking about. I have no desire to be a policy maker. To have a desire to be a policy maker would be to say something like, " Well, if I had been in Bernanke's situation I would have done things very differently for the following reasons, and things would be much better had I been in that position." I have no feelings in that direction. It's very hard to make policy decisions given our current state of knowledge, and I have no desire to try.

Do you find any irony in that fact you've been so closely attached to many Federal Reserve banks throughout your career, prominently in Minneapolis of course, but even afterward?

Not really. This is delicate on a number of grounds. I've been working in monetary theory for all this time, and I think it's a fun and exciting area to work in. I look at received theory, and see a hole in it regarding money. If you can help fill it in a way that provides some insights on policy issues, then that's a big contribution.

To what extent should the Federal Reserve System be engaged in such research?

In my years in association with the Federal Reserve Bank in Minneapolis, there have been a few times when my research was directly inspired by some policy issue that the Bank wanted to address. One was some work in the 60's on so-called intermediate policy targets; the other was related to bank failures and deposit insurance issues that first came up in the mid-70's.

You wouldn't pick unpleasant monetarist arithmetic?

Right, I wouldn't. We were thinking a little about current events, but it's not that the Bank president posed the question.

Wasn't your work on deposit insurance directly motivated by a real-time policy question?

It was. At the time, John Kareken was an economic advisor to the Bank president and we became interested in the way the Fed and the FDIC handled the failure of Franklin National. But in most other respects, the research questions that have inspired me have just been there and have been there for a long time.

7. KAREKEN AND WALLACE INDETERMINACY

One of things that the Minneapolis Fed was prominent in advocating for a while was fixed exchange rate regimes, which we'd always gathered was highly influenced by your work.

I can't recall what exactly inspired it, but I can remember some things about it. This was about the time when I was going from model building by just slapping down some demand function for money to wanting to do something more fundamental—maybe, using overlapping generations to do it. But it was in a quite early stage of doing that. I'm not sure what our thinking was, but we decided we would write down an overlapping-generations model with two countries and two monies, and we'd think about the possibility of currency substitution in that world. As I remember, we were surprised by the result that if we didn't have some extraneous rules—residents of one country are not allowed to hold the other countries' money or something related—then we couldn't get well-defined demands for individual monies.

At the time when we first got this result, it was pretty disturbing to me. I felt like we were heading into some completely unknown area. Luckily for me, Kareken was willing to be adventurous. He wanted to take this wherever it went: if it's a little disturbing, fine. He had in mind that it was a good thing to look for serious micro foundations. So we wrote this little paper. I'm not sure what we called it, but it came be labeled " indeterminacy of exchange rates." This is the only *Quarterly Journal of Economics* publication I've ever had.[1]

At the time, the Minneapolis Fed had a quarterly review, and it was always a struggle to fill it. I can remember thinking, maybe I should write a quarterly review article about indeterminacy of exchange rates. I'm Jewish by background, and knew a bit about Passover. Part of it is a meal at which one of the children asks what are called the four questions. And one of these questions, or maybe the general overriding question, is " why is this night different from other nights?" I was driving home from work one day and I thought of a title for a potential *Quarterly Review* piece; namely, why markets in foreign exchange are different from other markets. Because I liked the title, I decided I would write the paper. And I think it's the only one of my *Quarterly Review* articles that doesn't have a symbol in it. The article was picked up in a couple of books of readings because Federal Reserve papers can be freely reprinted and because it was all words.

But I did like the question—why markets in foreign exchange are different from other markets—because it relates back to some fundamental issues. I can find in writing Milton Friedman saying that if markets can price cars, then they can price German marks in terms of dollars. So what does it mean that markets can price cars? That's an allusion to the first welfare theorem. There's something good about markets pricing cars, but dollars and other currencies are not in the model. So this analogy is not valid in several senses. That's partly the theme that I pursued. And, yes, I've been skeptical of floating exchange rates on those grounds.

Floating exchange rates is not just an application of free markets. It has to rest on other grounds.

In fairness to Friedman, his main argument for flexible exchange rates was not the first welfare theorem. It's actually Phillips curve stuff: it's easy to get price adjustments through an exchange rate rather than from price levels. It was a story about some prices being more flexible than others. But at the time, these sorts of inflexible nominal price models were not the things we were pushing in Minneapolis, so where was the case for floating exchange rates?

If I was taking up this issue today, my analysis might be somewhat different. However, I'd still have a tough time defending the view that the Canadian dollar should float against the U.S. dollar.

8. REFLECTIONS ON OWN RESEARCH

Staying with the topic of your research, you are known to be one of your own toughest critics. But over your career there's got to be some stuff that you really like. What papers do you still like and why?

In one sense I've been fortunate. There are people who do their best work as their first work. Consider Kenneth Arrow and his work on social choice theory. I think most people would put that work up there among the best things that he's ever done. I've talked about how I came from doing old style macro, where you build models one equation at a time, and you don't think about the underlying behavior very much. So I've sort of made a painful conversion from being that kind of economist to learning a little economic theory. And along with learning a little economic theory, I learned a little mathematics. I still have holes, substantial holes, in my mathematical knowledge. But I'm in this somewhat happy position of thinking that I'm writing my best papers now—certainly relative to what I've done in the past, and even relative to what other economists are doing now.

I think I've mentioned that I like the paper that Tao Zhu and I wrote, called "Float on a Note." It took this issue, not the biggest deal in the world, and provided a nice insight about it. And I liked the little paper we and Manjong Lee did on denomination structures. It's about how we think about what are good denomination structures, in the following sense: should you have $5's, $7's, and $20's or something else. It's a small question and it's easy to master the small literature on it. Our paper couldn't have been written without the background work on models in which people meet in pairs to trade. The same is true of the paper on float. Both papers are on small questions, but represent substantial advances on the existing literature on those questions. Most important, they take an existing group of models and apply them in new areas.

I'm not just picking those things out because they're recent. If I put them up against things I'm better known for—unpleasant monetarist arithmetic or the work on deposit insurance—the recent things are more innovative. That's my judgment.

I think I've become a better economist, mainly because I couldn't help but get better.

You don't have anything that you would say, " I got it about right."

I don't think so. I think what I got right, since about the mid-'70's, is telling graduate students that monetary theory is quite undeveloped. There are some real voids to fill there, so we should try to fill them. I haven't been the only one saying that by any means. But I've probably been saying it longer than a lot of people. But in terms of actually making contributions that might stand some test of time, I don't think that earlier stuff will meet the test.

Don't you think new ideas help even if they don't stand the test of time? You've already discussed how the OLG framework was really the first attempt to get at the deeper foundations of monetary economics. Ok, so it didn't quite pan out, but the question got asked. . . .

Yes, perhaps that's so. By the way, I like the work I've done with Ricardo Cavalcanti on inside and outside money. (We would do it better if we were writing that paper now, but that paper with Ricardo is one that I like a lot.) When I compare that work with a cash-in-advance model, or a model in which money is in the utility function, our work is much better and takes up a real issue. Should we allow people to issue private monies? It's a long-standing question. What do you do with that question if your theory of money is cash-in-advance? It's a dead end. It's a conversation stopper. You're either going to say, well, the private money is the cash in the cash-in-advance model or it's not. That's the end.

9. IMPORTANCE OF HETEROGENEITY

Could you expand on that? What is the right approach to building monetary models?

The right way to integrate money into economic theory is to try to build models from the ground up; try to build models in which money has a role. That pushes you in a certain direction. If you like models in which people meet in pairs, then any kind of idiosyncratic shocks that occur—such as health shocks, tastes shocks, or some people running into trading opportunities and others not—will lead to a situation of diverse outcomes. This gives rise to heterogeneous-agent models.

The difficulty is having a state variable that is a distribution. Except in very special cases, you do not get closed-form solutions. By the way, outside of monetary economics, people are not really shying away from this. In any case, the issue is whether you can you innocently simplify the model and get away from this heterogeneity.

Lagos–Wright thought of a way of doing this. They had the vision that the economy has some decentralized trade in the economy and some trade that looks more like competitive markets. This, perhaps, seems reasonable. Then they assume quasi-linear preferences in the part of the model where there's competitive trade. That assumption has far-reaching consequences. When you think about

quasi-linear preferences in microeconomics, it's used to do partial equilibrium analysis in a somewhat rigorous way; that is, it allows you to talk about one market at a time. It's widely used in microeconomics and is sometimes called transferable utility. It simplifies things by getting rid of income effects. It was clever for Lagos and Wright to see that this hugely simplifies matching models.

So then the question is, What are you getting through this assumption (of quasi-linear preferences), and what are you giving up? Well, you're getting a huge amount of tractability, the ability to get closed-form solutions. But you have to ask yourself, what's the price of this? I do not think that is being addressed. Related to this, when matching models began to be used, it was standard to assume that money holdings were either zero or one indivisible unit. And there, too, it is important to ask whether the answers are sensitive to that assumption, an assumption which is made only for the sake of tractability.

So what about quasi-linearity? By now, we know some qualitative things about this. We know if you could support the taxation required to have the Friedman rule in that model, it would give us the first-best allocation. But we also know that if we had a departure from quasi-linearity, in terms of preferences in this competitive part of the model, then we wouldn't want to have the Friedman rule. We'd want to have less deflation, or a lower rate of return on money. So this particular assumption of quasi-linearity gives a particular answer which is not general and is not indicative of the general policy prescription.

So I think we ought to be careful about treating this quasi-linearity as if it's some normal case. When you look at the way these models work, what they say is that if you think about them in terms of the propensity to save, at some wealth level below some critical value, the marginal propensity to save is unity and above some critical value, it's zero. So this is a pretty special thing. You have to decide for what issues quasi-linearity is somewhat innocent and for what issues it's not. It should not be the default model.

Beyond tractability, one argument for the Lagos–Wright approach is that it eliminates heterogeneity that is not an essential characteristic of a model that would be useful for answering many questions about monetary economies. Do you think this argument is wrong?

Well, what are those questions? They seem to be questions about the Friedman rule and the welfare costs of inflation. As I said before, I believe that currency is the base for the inflation tax. Once you look at currency, you have to really shut your eyes to treat inflation as a representative-agent tax. We may or may not want to tax currency, but to imagine that the holdings are spread approximately evenly and are unrelated to other things in the economy seems silly.

10. WRAPPING UP

The real business cycle model was a representative-agent model and this was a big revolution in macro. It seems like you didn't really participate in that research

https://doi.org/10.1017/S1365100515001066 Published online by Cambridge University Press

program. Is that because you think the representative-agent framework cannot answer important questions?

When Prescott started real business cycle theory, I viewed it as having a qualitative message. The idea was to make the point that innocent settings, settings in which there are obviously no departures from optimality, can give rise to qualitative observations which had been viewed as puzzling—mainly, that investment fluctuates a lot relative to consumption. That was a worthwhile message.

The message that optimal outcomes could generate fluctuations and, therefore, don't call for interventions, is not intrinsically connected with a representative agent. Nor is the idea that exogenous technology shocks account for most fluctuations. However, if real business cycle theory turns into a theory in which TFP is an empty box into which you can shove anything, a view which Prescott seemed to espouse in the Ely lecture he gave a number of years ago, then it becomes a much less interesting theory.

Given the very strong sentiment you expressed about heterogeneity being at the center of some of the questions we really want to answer, do you think that the place we really ought to be moving on the frontier is on methods for handling heterogeneity and distributional issues? Is the big piece of this now a math problem?

Maybe it is, or maybe it is just a programming problem. If I had a really good model whose implications were really going to tell me something important, I do not think it would be hard to get a big grant to do a lot of intensive computing. People don't do that because they cannot make the case that the answer is important enough. Either the question being posed isn't interesting enough, or the way it's being addressed does not merit that kind of effort.

You've spent a huge amount of your time working in monetary economics. Some people out there would say this is useful so we can help central banks get a handle on the welfare costs of inflation. Is this the big question or would you propose other big questions in monetary theory?

One nice thing about doing research is that you build models which do some things, and then you see that you can use them to address other questions. I don't start out wanting to improve monetary theory in order to get better estimates of the welfare costs of inflation. I start out to do monetary theory to try to put in place some building blocks that we're happy with. Better estimates of the welfare costs of inflation may be part of the output, but I don't think it's the only one by any means.

For example, quite a few years ago at the Federal Reserve Bank of Minneapolis, an economist there and I had a few brief discussions about Federal Reserve float. The Federal Reserve has rules about how it credits depositing banks and debits banks on which checks are written. Other countries have different rules. The question was, Can we appraise those different rules? At the time, I decided we weren't ready to do it. We didn't have a model of transactions or of float. Now, we have such models. Although I haven't returned to that question, if I did I would have at least some inkling about how to begin.

11. NEIL WALLACE VITA

Distinguished Fellow, American Economic Association (2012–present).

Distinction in the Social Sciences, College of the Liberal Arts, Penn State (2005–present).

Fellow, American Academy of Arts and Sciences (2005–present).

Honorary Degree, Universidad del Pacifico, Lima, Peru (1995–present).

Fellow, Econometric Society (1981–present).

NOTE

1. Well, not quite. Neil has a very recent paper in the *Quarterly Journal of Economics*.

ARTICLES PUBLISHED IN REFEREED JOURNALS

Hu, Tai-Wei and Neil Wallace (2016) Information aggregation in a large multi-stage market game. *Journal of Economic Theory* 161, 103–144.

Wallace, Neil (2014) Optimal money-creation in "pure-currency" economies: A conjecture. *Quarterly Journal of Economics* 129, 259–274.

Wallace, Neil and Alexei Deviatov (2014) Optimal inflation in a model of inside money. *Review of Economic Dynamics* 17, 287–293.

Wallace, Neil (2013) Comment on "Moneyspots: Extraneous Attributes and the Coexistence of Money and Interest-Bearing Nominal Bonds" by Ricardo Lagos. *Journal of Political Economy* 121, 793–795.

Deviatov, Alexei and Neil Wallace (2009) A model in which monetary policy is about money. *Journal of Monetary Economics* 56, 5.

Hu, Tai-Wei, John Kennan, and Neil Wallace (2009) Coalition-proof trade and the Friedman rule in the Lagos–Wright model. *Journal of Political Economy* 117, 116–137.

Nosal, Ed and Neil Wallace (2007) A model of the threat of counterfeiting. *Journal of Monetary Economics* 54, 994–1001.

Wallace, Neil and Tao Zhu (2007) Float on a note. *Journal of Monetary Economics* 54, 229–246.

Zhu, Tao and Neil Wallace (2007) Pairwise trade and coexistence of money and higher return assets. *Journal of Economic Theory* 133, 524–535.

Andolfatto, David, Ed Nosal, and Neil Wallace (2007) The role of independence in the Green–Lin Diamond–Dybvig model. *Journal of Economic Theory* 137, 709–715.

Lee, Manjong, and Neil Wallace (2006) Optimal divisibility of money when money is costly to produce. *Review of Economic Dynamics* 9, 541–556.

Lee, Manjong, Neil Wallace, and Tao Zhu (2005) Modeling Denomination Structures. *Econometrica* 73, 949–960.

Wallace, Neil and Tao Zhu (2004) A commodity money refinement in matching models. *Journal of Economic Theory* 117, 246–258.

Wallace, Neil (2003) Modeling small change: A review article. *Journal of Monetary Economics* 50, 1391–1401.

Katzman, Brett, John Kennan, and Neil Wallace (2003) Output and price level effects of monetary uncertainty in a matching model. *Journal of Economic Theory* 108, 217–255.

Deviatov, Alexei and Neil Wallace (2001) Another example in which lump-sum money creation is beneficial. *Advances in Macroeconomics* 1, Article 1.

Wallace, Neil (2000) A model of the liquidity yield structure based on asset indivisibility. *Journal of Monetary Economics* 45, 55–68.

Braun, R.A., Richard Todd, and Neil Wallace (1999) A general equilibrium interpretation of damage contingent securities. *Journal of Risk and Insurance* 66, 583–596.
Taber, Alexander and Neil Wallace (1999) A matching model with bounded holdings of indivisible money. *International Economic Review* 40, 961–984.
Cavalcanti, Ricardo and Neil Wallace (1999) A model of private bank-note issue. *Review of Economic Dynamics* 2, 104–136.
Cavalcanti, Ricardo and Neil Wallace (1999) Inside and outside money as alternative media of exchange. *Journal of Money, Credit and Banking* 31, 443–457.
Kocherlakota, Narayana and Neil Wallace (1998) Optimal allocations with incomplete record-keeping and no-commitment. *Journal of Economic Theory* 81, 272–289. Invited.
Wallace, Neil and Ruilin Zhou (1997) A model of a currency shortage. *Journal of Monetary Economics* 40, 555–572.
Aiyagari, S.R. and Neil Wallace (1997) Government transaction policy, the medium of exchange, and welfare. *Journal of Economic Theory* 74, 1–18.
Wallace, Neil (1997) Short-run and long-run effects of changes in money in a random matching model. *Journal of Political Economy* 105, 1293–1307.
Aiyagari, S.R., Neil Wallace, and Randall Wright (1996) Coexistence of money and interest-bearing securities. *Journal of Monetary Economics* 37, 397–419.
Champ, Bruce, Neil Wallace, and Warren Weber (1994) "Interest rates under the U.S. national banking system. *Journal of Monetary Economics* 34(3), 343–358.
Aiyagari, S.R. and Neil Wallace (1992) Fiat money in the Kiyotaki–Wright model. *Econometric Theory* 2, 447–464.
Wallace, Neil (1992) Lucas' signal extraction model: A finite state exposition with aggregate real shocks. *Journal of Monetary Economics* 30, 433–447.
King, Robert, Neil Wallace, and Warren Weber (1992) Non-fundamental uncertainty and exchange rates. *Journal of International Economics* 32, 83–108.
Aiyagari, S.R. and Neil Wallace (1991) Existence of steady states with positive consumption in the Kiyotaki–Wright model *Review of Economic Studies* 58, 901–916.
Marimon, Ramon and Neil Wallace (1987) Trade using assets divisible at a cost. *Journal of Economic Theory* 43, 223–251.
Sargent, Thomas and Neil Wallace (1985) Interest on reserves. *Journal of Monetary Economics* 15, 279–290.
Bryant, John and Neil Wallace (1984) A price discrimination analysis of monetary policy. *Review of Economic Studies* 51, 279–288.
Sargent, Thomas and Neil Wallace (1983) A model of commodity money. *Journal of Monetary Economics* 12, 163–167.
Sargent, Thomas and Neil Wallace (1982) The real bills doctrine vs. the quantity theory: A reconsideration. *Journal of Political Economy* 90, 1212–1236.
Wallace, Neil (1981) A hybrid fiat–commodity monetary system. *Journal of Economic Theory* 25, 421–430.
Wallace, Neil (1981) A Modigliani–Miller theorem for open-market operations. *American Economic Review* 71, 267–274.
Kareken, John and Neil Wallace (1981) On the indeterminacy of equilibrium exchange rates. *Quarterly Journal of Economics* 96, 207–222.
Bryant, John and Neil Wallace (1980) Open-market operations in a model of regulated, insured intermediaries. *Journal of Political Economy* 88, 146–173.
Bryant, John and Neil Wallace (1979) The inefficiency of interest-bearing national debt. *Journal of Political Economy* 87, 365–381.
Kareken, John and Neil Wallace (1978) Deposit insurance and bank regulation: A partial equilibrium exposition. *Journal of Business* 51, 413–438.
Kareken, John and Neil Wallace (1977) Portfolio autarky: A welfare analysis. *Journal of International Economics* 7 19–43.

Sargent, Thomas and Neil Wallace (1976) Rational expectations and the theory of economic policy, *Journal of Monetary Economics* 2, 169–183.
Sargent, Thomas and Neil Wallace (1975) Rational expectations, the optimal monetary instrument, and the optimal money supply rule. *Journal of Political Economy* 2, 241–254.
Muench, Thomas, Arthur Rolnick, Neil Wallace, and William Weiler (1974) Tests of structural change and prediction intervals for the reduced forms of two structural models of the U.S.: The FRB–MIT and Michigan quarterly models. *Annals of Economic and Social Measurement* 3, 491–519.
Kareken, John, Thomas Muench, and Neil Wallace (1973) Optimal open market strategy: The use of information variables, *American Economic Review* 63, 156–172.
Sargent, Thomas and Neil Wallace (1973) Rational expectations and the dynamics of hyperinflation. *International Economic Review* 14, 328–350.
Sargent, Thomas and Neil Wallace (1973) The stability of models of money and growth with perfect foresight. *Econometrica* 41, 1043–1048.
Wallace, Neil (1972) An approach to the study of money and non-money exchange structures. *Journal of Money, Credit and Banking* 4, 838–847.
Kareken, John and Neil Wallace (1972) The monetary instrument variable choice: How important? Comment. *Journal of Money, Credit and Banking* 4, 723–729.
Sargent, Thomas and Neil Wallace (1971) Market transaction costs, asset demand functions, and the relative potency of monetary and fiscal policy. *Journal of Money, Credit and Banking* 3, 469–505.
Wallace, Neil (1970) The determination of the stock of reserves and balance of payments in a Neo-Keynesian model. *Journal of Money, Credit and Banking* 2, 269–290.
McCall, John and Neil Wallace (1969) A supply function for first-term re-enlistees to the Air Force. *Journal of Human Resources* 4, 293–310.
Wallace, Neil (1969) Buse on Meiselman—A comment. *Journal of Political Economy* 77, 524–527.
Wallace, Neil (1967) Comment on "Debt management and the term structure of interest rates: An empirical analysis." *Journal of Political Economy* Supplement, 590–592.
Wallace, Neil (1967) The term structure of interest rates and the maturity composition of the federal debt. *Journal of Finance* 22, 301–312.
Grilliches, Zvi and Neil Wallace (1965) The determinants of investment revisited. *International Economic Review* 6, 311–329.
Grilliches, Zvi, Robert Lucas, G.S.G.S. Maddala, and Neil Wallace (co-author, 25%) (1962) Notes on estimated aggregate quarterly consumption functions. *Econometrica* 30, 491–500.

BOOKS

McCandless, George, and Neil Wallace (1991) *Introduction to Dynamic Economic Theory, An Overlapping Generations Approach*. Cambridge, MA: Harvard University Press.
Prescott, Edward and Neil Wallace (1987) *Contractual Arrangements for Intertemporal Trade, Edward Prescott and Neil Wallace*. Minneapolis: University of Minnesota Press.
Kareken, John and Neil Wallace (1980) *Models of Monetary Economies*. Minneapolis: Federal Reserve Bank of Minneapolis.

CHAPTERS IN BOOKS

Wallace, Neil (2011) The mechanism-design approach to monetary theory. In Benjamin Friedman and Michael Woodford (eds.), *Handbook of Monetary Economics*, Vol. 3a pp. 1–20. Amsterdam: Elsevier.
Cavalcanti, Ricardo, and Neil Wallace (2008) New models of "old" payment questions. In *The Future of Payment Systems*, pp. 75–86. Oxford, UK: Routledge.

Wallace, Neil (1996) A dictum for monetary theory. In Steven G. Medema and Warren J. Samuels (eds.), *Foundations of Research in Economics: How Do Economists Do Economics?*, pp. 248–259. Cheltenham, UK: Edward Elgar Publishing.

Wallace, Neil (1989) Some alternative monetary models and their implications for open market policy. In Robert Barro (ed.) *Modern Business Cycle Theory*, pp. 306–328. Cambridge, MA: Harvard University Press.

Townsend, Robert, and Neil Wallace (1987) Circulating private debt: An example with a coordination problem. In Edward Prescott and Neil Wallace (eds.), *Contractual Arrangements for Intertemporal Trade*, pp. 105–120. Minneapolis: University of Minnesota Press.

Wallace, Neil (1987) Some unsolved problems for monetary theory. In William Barnett and Kenneth Singleton (eds.), *New Approaches to Monetary Economics, Proceedings of the Second International Symposium in Economic Theory and Econometrics*, pp. 340–349. Cambridge, UK: Cambridge University Press.

Wallace, Neil (1984) Do we need a special theory for "money"? In *Money: A Search for Common Ground*, Proceedings of a conference, pp. 179–198. Oxford, UK: Blackwell Publishers.

Sargent, Thomas and Neil Wallace 1984 Some unpleasant monetarist arithmetic. In B. Griffiths and G.E. Wood (eds.) *Monetarism in the United Kingdom*, pp. 1541. London: Macmillan.

Wallace, Neil (1980) The overlapping generations model of fiat money. In John Kareken and Neil Wallace (eds.), *Models of Monetary Economies*, pp. 49–82. Minneapolis: Federal Reserve Bank of Minneapolis.

Haitovsky, Yoel and Neil Wallace, (1972) A study of discretionary and nondiscretionary monetary and fiscal policies on the context of stochastic macro-econometric models. In V. Zarnowitz (ed.), *The Business Cycle Today*, pp. 261–309. New York: Columbia University Press.

OTHER WORKS

Wallace, Neil (2008) Fiat money. *The New Palgrave Dictionary of Economics*, 2nd ed., pp. 4–8.

JOURNAL ARTICLE, ACADEMIC JOURNAL, NONREFEREED

Wallace, Neil (2005) From private banking to central banking: Ingredients of a welfare analysis. *International Economic Review* 46, 619–631. Invited.

Wallace, Neil (2001) Whither monetary economics? *International Economic Review* 42, 847–869.

Wallace, Neil (2000) Knowledge of individual histories and optimal payment arrangements. *Federal Reserve Bank of Minneapolis Quarterly Review* (Summer), 11–21.

Wallace, Neil (1998) Introduction to modeling money and studying monetary policy. *Journal of Economic Theory* 11 223–231.

Wallace, Neil (1997) Absence-of-double-coincidence models of money: A progress report. *Federal Reserve Bank of Minneapolis Quarterly Review* (Winter), 2–20.

Wallace, Neil (1996) Narrow banking meets the Diamond–Dybvig model. *Federal Reserve Bank of Minneapolis Quarterly Review* (Winter), 3–13.

Todd, Richard and Neil Wallace (1992) SPDAs and GICs: Like money in the bank? *Federal Reserve Bank of Minneapolis Quarterly Review* (Summer), 2–17.

Wallace, Neil (1990) A banking model in which partial suspension is best. *Federal Reserve Bank of Minneapolis Quarterly Review* (Fall), 11–23.

Wallace, Neil (1988) A suggestion for oversimplifying the theory of money. *Economic Journal* 98, 25–36.

Wallace, Neil (1988) Another attempt to explain an illiquid banking system: The Diamond and Dybvig model with sequential service taken seriously. *Federal Reserve Bank of Minneapolis Quarterly Review* (Fall), 3–16.

Muench, Thomas and Neil Wallace (1974) On finding a good stabilization policy: Models and methods. *American Economic Review* 63, 773–781.

Sargent, Thomas, and Neil Wallace (1974) The elasticity of substitution and the cyclical behavior of productivity, average hourly earnings and labor's share. *American Economic Review* 64, 257–263.

REFERENCES

Arrow, K. (1951) *Social Choice and Individual Values*. New Haven, CT: Yale University Press.
Boldrin, M. and D. Levine (2008) *Against Intellectual Monopoly*. Cambridge, UK: Cambridge University Press.
Cavalcanti, R. and N. Wallace (1999) A model of private bank-note issue. *Review of Economic Dynamics* 2, 104–136.
Cavalcanti, R. and N. Wallace (1999) Inside and outside money as alternative media of exchange. *Journal of Money, Credit and Banking* 31, 443–457.
Diamond, W. and P. Dybvig (1983) Bank runs, deposit insurance, and liquidity. *Journal of Political Economy* 91, 401–419.
Friedman, M. (1951) *A Program for Monetary Stability*. New York: Fordham University Press.
Hicks, J. (1935) A suggestion for simplifying the theory of money. *Economica* 2, 1–19.
Hu, T., J. Kennan, and N. Wallace (2009) Coalition-proof trade and the Friedman rule in the Lagos–Wright model. *Journal of Political Economy* 117, 116–137.
Kandori, M. (1992) Repeated games played by overlapping generations of players. *Review of Economic Studies* 59, 81–92.
Kareken, J. (1983) Deposit insurance reform; or, deregulation is the cart, not the horse. *Federal Reserve Bank of Minneapolis Quarterly Review* 7(2), 1–9.
Kareken, J. and N. Wallace (1981) On the indeterminacy of equilibrium exchange rates. *Quarterly Journal of Economics* 96, 207–222.
Kockerlakota, N. (1998) Money is memory. *Journal of Economic Theory* 81, 232–251.
Lagos, R. and R. Wright (2005) A unified framework for monetary theory and policy analysis. *Journal of Political Economy* 113, 463–484.
Lucas, R. (1972) Expectations and neutrality of money. *Journal of Economic Theory* 4, 103–124.
Modigiliani, F. (1994) Liquidity preference and the theory of interest and money. *Econometrica* 12, 45–88.
Ostroy, J. (1973) The informational efficiency of monetary exchange. *American Economic Review* 63, 597–610.
Prescott, E. (2002) Richard T. Ely lecture: Prosperity and depression. *American Economic Review* 92, 1–15.
Samuelson. P. (1958) An exact consumption-loan model of interest with or without the social contrivance of money. *Journal of Political Economy* 66, 467–482.
Sargent, T. and N. Wallace (1975) " Rational" expectations, the optimal monetary instrument, and the optimal money supply rule. *Journal of Political Economy* 83, 241–254.
Sargent, T. and N. Wallace (1981) Some unpleasant monetarist arithmetic. *Federal Reserve Bank of Minneapolis Quarterly Review* 5(3), 1–7.
Smith, A. (1977) *An Inquiry into the Nature and Causes of the Wealth of Nations*. Chicago: Chicago University Press.
Theil, H. (1958) *Economic Forecasts and Policy*. Amsterdam: North-Holland.
Thomas, L. (1978) *Lives of a Cell*. London, UK: Penguin.
Townsend, R. (1987) Economic organization with limited communication. *American Economic Review* 77, 954–971.
Wallace, N. (1979) Why markets in foreign exchange are different from other markets. *Federal Reserve Bank of Minneapolis Quarterly Review* 3(4), 1–7.
Wallace, N. and T. Zhu (2007) Float on a note. *Journal of Monetary Economics* 54, 229–246.