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Introduction to  
“Models of Monetary Economies II:  
The Next Generation”

Randall Wright

Optimal Monetary Policy:  
What We Know  
and What We Don't Know

Narayana R. Kocherlakota

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## Introduction to “Models of Monetary Economies II: The Next Generation”

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In 1978, John Kareken and Neil Wallace of the University of Minnesota organized a conference on monetary economics at the Federal Reserve Bank of Minneapolis. The conference proceedings were published as a book by the Bank under the title *Models of Monetary Economies*. Many of the articles, as well as the formal discussions of these articles, have become classics. “Models of Monetary Economies” set the agenda and the terms of discussion for monetary economics over the next quarter century. In May 2004, the Federal Reserve Bank of Minneapolis and the University of Minnesota hosted a follow-up conference, “Models of Monetary Economies II: The Next Generation,” that I had the privilege of organizing along with Narayana Kocherlakota of Stanford University. The idea for the conference was to take stock of what has been happening in theoretical monetary economics since the last conference and discuss some recent papers in the area.

Of course, there is no way to replicate the impact of the first “Models of Monetary Economies,” which had some of the best scholars of our time doing some of the best work of their careers. Still, it seemed worthwhile to have another conference, to revisit some of the issues, and to encourage work in the area. The [published] papers . . . help communicate recent developments to a larger audience than could have been in attendance. In writing this introduction, it is not my intention to attempt

to provide anything like the introduction to the first volume by Kareken and Wallace (1980), which seems far too ambitious and daunting a task at present. I will mainly let the papers and the formal comments on the papers . . . speak for themselves. I will simply summarize aspects of the papers that I think are particularly relevant, and try to give an overview of how the contributions fit together. At the end, I will offer some brief comments on monetary theory more generally—on how it has evolved over the years since the first conference and on where it may be headed.

The first article . . . is by Kiyotaki and Moore (2005).<sup>1</sup> They start with the following observations. First, define monetary, or liquid, assets as those that can be readily sold on the market and held by different people prior to

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<sup>1</sup>Note that the order of the articles [mentioned] here is not exactly the same as the order in which they were presented at the conference. Also note that the Kiyotaki–Moore article is based on a Klein lecture sponsored by the IER and presented by the first author in 2002 at Osaka. Also note that a formal discussion of this paper is not included in the [*International Economic Review*] issue.

maturity. If such assets circulate as media of exchange, then they will be held not only for their maturity value but also for their exchange value. Kiyotaki and Moore develop a model based on these observations and use it to discuss some puzzles in the asset-pricing literature and business cycle theory. They first ask, in what kind of environment is the circulation of liquid assets essential for the smooth running of the economy? In such economies, there will be a liquidity premium on certain assets; say, if capital is less liquid than land, it must bear a higher rate of return. They revisit some issues in asset pricing and business cycle theory from this perspective. As I see it, this research is important not necessarily as a contribution to the microfoundations of monetary theory *per se*—this does not seem what the authors are after in this article—but as a message to people who study assets prices and business cycles while ignoring the fact that different assets have different liquidity properties. Although it is not as if no one thought of this before, obviously, there is much value in being explicit about what liquidity is, and about what types of assumptions give rise to a role for liquidity.

The next three articles provide different attempts to delve deeper into the microfoundations of monetary theory. Green and Zhou (2005) adopt a mechanism design approach in a classic model that appeared in an article by Bewley (1980) from the original conference, and was extended in Bewley (1983). The environment has random shocks and private information, and the issues are, roughly speaking, what kind of mechanisms deliver good outcomes, and when do these mechanisms resemble the process of exchange in a monetary economy? Alternatively, one can put it this way: When does monetary exchange deliver something that is close to efficient, and what kinds of monetary policies help or hinder efficiency? They make substantial progress on these matters, which are inherently quite difficult and technical. Intuitively, they show that monetary trade is nearly efficient when agents are very patient and can accumulate large enough stocks of money to be close to fully self-insured. In addition to the formal results, a feature of this work that makes it an important contribution to the current volume, is that it is a nice example of the recent approach of using mechanism design theory to discuss monetary institutions in a concise way.<sup>2</sup>

The article by Kahn, McAndrews, and Roberds (2005) takes a novel approach to thinking about money. Recent authors have emphasized that imperfect record keeping (or memory or monitoring) is critical for money

to be essential—i.e., essential in the precise sense that we can support better outcomes as equilibria with money than we can without it. This is clear enough, intuitively, since with perfect record keeping, without anything like tangible cash ever actually changing hands we could support any outcome that we could accomplish using money; see Kocherlakota (1998) and Kocherlakota and Wallace (1998) for discussions. This might suggest that over time, as record keeping gets better, a role for money will disappear. Kahn, McAndrews, and Roberds argue that an advantage to using money is precisely that there is not a record of the transaction, since when you pay with cash your identity need not be revealed. They formalize this idea. Independent of the details of their setup, over which one can always quibble, the notion that there may always be a role for money based on the fact that it preserves privacy is surely correct. This research also provides another example of using relatively formal methods to think about monetary institutions in a concise way.

Howitt (2005) considers economies where trade is organized around *shops* where agents can exchange goods.<sup>3</sup> The title “Beyond Search” reflects the idea that most of the literature going back to Kiyotaki and Wright (1989, 1993) assumes agents meet at random, while in the real world, as Howitt puts it, trade is “organized by specialist traders, who mitigate search costs by providing facilities that are easy to locate.” In particular, “when people wish to buy shoes they go to a shoe store; when hungry they go to a grocer; when desiring to sell their labor services they go to firms known to offer employment. Few people would think of planning their economic lives on the basis of random encounters with nonspecialists.” Now, monetary economists working in the search tradition have recently made it clear that it is really record-keeping and double-coincidence problems that are key in these models, and although random meetings may be a convenient way to generate these features, one can make meetings endogenous and nonrandom and still preserve the essence of the approach (Corbae, Temzelides, and Wright 2003). In any case, Howitt’s goal is to determine which objects emerge as media of exchange when meetings are endogenously organized through shops.

<sup>2</sup>Wallace (2001) provides a general discussion of why the mechanism-design approach is well suited for monetary theory.

<sup>3</sup>These shops are similar to the trading posts in the market game literature, which has been used to think about money since Shapley and Shubik (1977); see Hayashi and Matsui (1996) or Alonzo (2001) for modern contributions.

The next group of articles is concerned more with policy, or with applying versions of existing theoretical structures to understand substantive issues. Bhattacharya, Haslag, and Martin (2005) ask about the properties of the Friedman rule—the policy of disinflating at the rate of time preference, or achieving a zero nominal interest rate, which turns out to be the optimal policy in a wide variety of environments—in models where agents are heterogeneous. They consider three models: the turnpike model of Townsend (1980) from the original conference; a version of the overlapping-generations model advocated by Wallace (1980) at that conference, as extended in Schreft and Smith (1997) and Smith (2002) to incorporate spatial separation; and the search model in Lagos and Wright (2005). In each case the models are extended to have additional heterogeneity (all models of money have some type of heterogeneity). A key result is that the Friedman rule does not typically maximize social welfare due to redistributive effects. In general, it is desirable to analyze monetary policy in a variety of different environments, to see which results are robust and which are not, and it is also desirable to analyze policy with heterogeneous agents. This article makes progress on both dimensions.<sup>4</sup>

Berentsen, Camera, and Waller (2005) also pursue the effects of monetary injections in a version of the Lagos–Wright model. Originally, the Lagos–Wright model was designed to deliver a degenerate distribution of money holdings and hence avoid the complicated problem of dealing with an endogenous nondegenerate distribution that comes up in many other search models (e.g., Molico 1997, Green and Zhou 1998, Camera and Corbae 1999, Zhu 2003). The way it works is that agents have access to centralized markets, where they can reoptimize their cash balances after each round of decentralized trade, which, combined with quasi-linear utility, delivers the result. One could object that this trick throws out the baby with the bath water, in the sense that distributional issues are at the heart of monetary economics. Or at least that it might be nice to have a model that is simple enough to deliver analytic results, but can still be used to think about distributional issues. Berentsen, Camera, and Waller give agents access to centralized markets not after every round, but after every two rounds of centralized trade. This allows them to reintroduce distributional considerations while keeping the analysis tractable. They derive several results, such as unexpected money injections may increase short-run output. Although this has been shown in other models,

they highlight novel effects, help us understand the robustness of results, and provide a model that may have many applications in the future.<sup>5</sup>

Lagos and Rocheteau (2005) study the effects of fully anticipated inflation in a model with endogenous search intensity. Also, they consider two different pricing mechanisms: bilateral bargaining and a notion of competitive pricing appropriate for search models. This is important for the following reasons. A large variety of physical environments have been studied in monetary theory—from overlapping-generations models to turnpike models to search models and beyond—but there has been relatively little comparison across pricing institutions in a given environment. When results differ across models that have both different environments and different pricing, it is hard to know what drives the results. Many economists seem overly wed to their favorite pricing mechanism—the auctioneer for some, bargaining for others, posting for others still, and so forth. Lagos–Rocheteau explicitly show how it matters which pricing system we adopt.<sup>6</sup> They prove that bargaining generically delivers inefficient outcomes, for any policy. By contrast, if prices are posted and agents can direct their search, a solution concept due to Moen (1997) and Shimer (1995), and known as *competitive search equilibrium* in labor economics, the Friedman rule delivers the efficient outcome.<sup>7</sup>

Head and Kumar (2005) provide another contribution to our understanding of the effects of different pricing arrangements. They use the Burdett–Judd (1983) model, where sellers post prices as in Lagos–Rocheteau, but search is undirected. A key assumption in Burdett–Judd models is that different buyers see different numbers

<sup>4</sup>The concluding essay by Kocherlakota [2005] . . . provides more discussion of the literature on the Friedman rule and optimal policy generally and suggestions for future work in the area.

<sup>5</sup>It is interesting to contrast the models in Berentsen, Camera, and Waller (2005) and Green–Zhou (2005), which are similar except that in the former, agents get to go periodically to a centralized market where past histories can be cleared. One thing this implies is that the “difficulty with the optimal quantity of money” in Bewley (1983) need not arise. In particular, Berentsen and coauthors prove the perhaps surprising result that the Friedman rule yields full efficiency. The key to this result is simply the assumption that agents have a finite number of rounds before they can adjust their money holdings in the centralized market.

<sup>6</sup>Obviously, the mechanism-design approach does compare different institutions (and also looks for good ones). Here, I am thinking about work that takes the equilibrium approach but is willing to consider different definitions of equilibrium based on different price-setting institutions (Rocheteau and Wright 2005).

<sup>7</sup>This is interesting vis-à-vis the previous footnote because, although Lagos–Rocheteau are not doing mechanism design, it turns out one of the institutions they consider is an optimal mechanism: Under price posting and the Friedman rule, we get efficiency.

of prices. Of course, Head and Kumar have to adapt Burdett–Judd to study money, so they introduce double-coincidence and imperfect-memory problems as in search theory, in this case keeping things tractable by adopting the large family structure of Shi (1997). The model delivers a distribution of posted prices in equilibrium, and thus provides a natural framework for discussing the interaction between price dispersion and money. For example, there is much talk about the relation between inflation and price variability, and this can now be analyzed explicitly. They prove inflation increases price dispersion in one version of their model. They also show that when the degree of incomplete information is chosen by the agents endogenously, inflation above the Friedman rule may be optimal. By considering a novel approach to pricing in monetary theory, we get a framework within which many new things can be done (some of which appear in a follow-up project by Head, Kumar, and Lapham 2004).

The article by Shi (2005) is another application of the large family model. Here, he tries to integrate a version of this model with the literature on limited participation following Lucas (1990), in order to study the interactions between money and bonds. He introduces a legal restriction along the lines of the one in Aiyagari, Wallace, and Wright (1996) that says government goods cannot be purchased with bonds. This “gets around” the rate-of-return-dominance issue, as agents in the model are willing to hold cash even if bonds bear interest because at random they will need it for certain types of trades (those with government agents). Because of randomness in meetings, even a very small legal restriction can have a big impact. The effects of policy are analyzed, and some differ significantly from what is found in models with markets that are frictionless (in the sense that they may have limited participation and cash-in-advance restrictions, but not random matching). This is a nice continuation of efforts to study monetary economics in the Shi (1997) framework.<sup>8</sup>

The next group of articles is concerned with banks. Wallace (2005) extends earlier work by Cavalcanti and Wallace (1999a, 1999b) that introduces some agents called banks into an otherwise fairly standard search-based environment of the type in Shi (1995) or Trejos and Wright (1995), and does mechanism design. Unlike other agents, banks can be monitored and hence punished by a social planner. In equilibrium, inside money issued by banks may circulate. It can be shown that allowing private money, as opposed to giving

government a monopoly, is a good idea. Basically, the desirable feature of inside money is that banks cannot run out of it, whereas they can always run out of outside money for some realizations of their trading histories. The current article extends the class of feasible outside money allocations by allowing the planner to make certain monetary transfers, and also weakens the types of punishments the planner can impose. The earlier results about the desirability of inside money continue to hold. More generally, this project makes progress over many of the models in monetary theory that are extremely primitive in the set of institutions that operate—often, only outside money, to the exclusion of any related institutions like banks.

The article by He, Huang, and Wright (2005) also introduces banks into monetary theory. The motivation is the old story about goldsmiths in England originally accepting deposits for safekeeping, after which several developments followed: The receipts for these deposits began to circulate as early banknotes; agents began to write checks against their deposits; and loans were made against which only partial reserves were held. Several progressively more involved models are developed, which are based on the search literature, except that now cash is subject to theft, which generates a role for safekeeping by banks.<sup>9</sup> The article shows how in equilibrium, cash, checks, or both can be used as a means of payment, and how the equilibrium set is affected by parameters such as the cost of managing bank accounts and the amount of outside money in the system. The article discusses fractional reserves and develops a simple money multiplier, very similar to the one in standard money and banking textbooks, except now the roles for both money and banks are derived from microeconomic foundations.

The article by Cavalcanti, Erosa, and Temzelides (2005) also pursues banking and monetary theory. It builds upon Cavalcanti, Erosa, and Temzelides (1999), which constructed a random-matching model of private money issue and redemption. Again, a fraction of agents

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<sup>8</sup>One might argue the so-called legal restriction that government agents do not accept bonds is close to a cash-in-advance assumption. The counterargument is this: It is one thing to assume government policy takes a particular exogenous form, and quite another to assume private agents’ trading strategies take an exogenous form. This is not to say the assumption is not problematic—just that it is different from the usual cash-in-advance model.

<sup>9</sup>Notice the contrast to the article by Kahn, McAndrews, and Roberds discussed above: There it is assumed that transacting with money is relatively safe, since it preserves privacy, whereas here it is relatively risky, since it may be stolen.

called banks can be monitored, and in equilibrium they issue a substitute for outside money that can circulate as a medium of exchange. Float opportunities generate an incentive to issue more inside money, but random redemption requirements work in the other direction. This generates a reserve-management problem. The article demonstrates the existence of an equilibrium with illiquid banking, where each period banks might fail. They also analyze the nature of banks' decisions as a function of their liquidity position. Some results are illustrated through numerical examples. For instance, it is shown how an infusion of reserves from the central bank during a liquidity shortage may increase trade and reduce bank failures. Again, this kind of work is an advance over models in monetary theory that ignore alternative institutions like banks and inside money.

This completes my summary of the conference papers. I will not summarize the formal comments on the papers, but it is worth noting that the discussants did a first-rate job. Clarifying the theoretical structure, as many did, is obviously useful. Others clarified what the issues are, or what they ought to be, both in terms of theory and quantitative issues. Some of the discussants highlighted facts that monetary theorists should be encouraged to think more about. Putting an article in one literature in the context of another literature, like monetary economics and pure game theory, as some of the discussants did, is illuminating. Some of them also provided relatively simple examples that make similar points to models in the articles; others developed full-fledged original models as alternatives to the ones in the articles (although in the interests of space, typically these are only discussed briefly in this issue and, therefore, they will have to appear in all their glory elsewhere). I would say that there is much to be recommended in these discussions.

The final essay in the volume, by Kocherlakota (2005), was written after the conference. I view it as some suggestions for future work in monetary economics, especially policy analysis. The article reviews work in what the author calls "basic" monetary economics, including some of the conference papers, and in what he calls the "applied" monetary literature. By "basic" he means that the papers try to be explicit about the frictions that allow money to be valued and make it beneficial, and by "applied" he means that the papers are less explicit. It is not hard to be critical about either literature. In one case, the point that it is better to be explicit about one's assumptions is obvious. In the other

case, as Kocherlakota points out, it is true that the existing "basic" work on monetary policy ignores assets other than fiat currency and ignores fiscal considerations (i.e., alternative policy instruments like tax rates), and the applied literature suggests both are important. I agree with Kocherlakota on these points, but am also optimistic that progress will soon be made.

I think, for instance, that an important next step in "basic" monetary economics that is just around the corner is to incorporate fiscal considerations. On the other hand, getting multiple assets with different rates of return into any model will be more difficult, at least if we stop short of shortcuts like simply assuming one asset needs to be used, as in a cash-in-advance model, or that it is valued for something other than its asset value or exchange value, as in a money-in-the-utility-function model. Although one would be foolish to deny the benefits of shortcuts generally, these approaches miss the boat in terms of what Kocherlakota is talking about for the following reason: If it is true that the presence of multiple assets is important for the results, one would really want to know what is generating the presence of multiple assets in the first place. If it is private information, limited commitment, or whatever story that may be used to implicitly justify a shortcut, we need to make these things explicit. Why? Because putting these features into the model may well have implications other than simply rationalizing the use of money that presumably ought not to be ignored when analyzing policy.<sup>10</sup>

I do not claim that the "basic" approach currently provides a satisfactory explanation for rate of return dominance—this is a challenge today, just as it was when Hicks (1935) wrote. Kocherlakota argues that the "applied" literature, which in a sense assumes away the issue, has still added to our understanding of the welfare-maximizing inflation or interest rate. Even if one agrees with this claim, it could be said that it is a fairly narrow issue. As an example of another policy question that may be at least as interesting, if not considerably more, and which would seem difficult to address with the typical shortcut approach, consider: "Should Europe have one currency or many, and why?" It is not that I know the answer, although a lot can be learned from Matsuyama,

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<sup>10</sup>Wallace (2001) says that one of the two reasons for taking microfoundations seriously is to avoid logical inconsistencies (the other reason is that it can lead to new insights). See also Kareken and Wallace's (1980) introduction to the previous conference for more on this point.

Kiyotaki, and Matsui (1993); the point is simply that even if one concerns oneself with policy and not pure theory, there are many issues for which it seems clear that one needs to follow the “basic” approach.<sup>11</sup>

Let me reiterate that the previous conference was a major achievement, and although one cannot reasonably expect to live up to those standards, there have been developments in monetary theory since then and it was useful to get together to talk about them at this conference. Several of the models from the first conference are still in use today, and some of the work presented here involves continuing projects started back then, but there are also new ideas. Articles in the previous volume utilized models with various types of frictions including spatial and temporal separation, but perhaps they were less explicit than more recent work as regards record keeping, memory, and information.<sup>12</sup> I think introducing some search-theoretic considerations was useful for a variety of reasons, and also very much in line with what some people in the first conference seemed to have in mind, even though it was not fully worked out. As suggested above, when combined with specialization, random matching is a natural way to generate a double-coincidence problem, which has been at the center of informal discussions of money for ages, as well as a record-keeping problem.

Moreover, some of the standard tools from search theory as used in other branches of economics fit nicely into monetary economics. For example, since not having everybody together at the same place and time is part of what leads to a double-coincidence problem, and since bilateral meetings are a simple way to generate this, one is led to consider bilateral bargaining, or maybe posting, as in many labor market models. It is not that it is necessary to abandon the Walrasian auctioneer to do monetary economics, but it is clearly necessary to deviate somewhat from the classical equilibrium paradigm, and once you do that, it is natural to consider alternatives to simple price taking. Moreover, the search literature has well-defined notions of things like thick-market effects, free-entry decisions, and so on, that are readily introduced into monetary economies. Time will tell how important these will be, both conceptually and, ultimately, quantitatively, but they are definitely worth investigating.

On the subject of quantitative economics, in the past some people seemed to think that “basic” or “pure” monetary theory is somehow not conducive to numerical implementation. Obviously some of the models in this

literature are too abstract to be seriously quantifiable. Not all good models are quantifiable—it would be silly even for a Minnesota student like myself to calibrate the textbook prisoner’s dilemma, or, worse, to claim that the prisoner’s dilemma idea is interesting only if we can calibrate it. Many papers in monetary theory are conceptual, by which I mean that they are designed to ask “why” and not “how much.” But anyone who thinks that people currently working on the microfoundations of money are not taking their models to the data is, like Bogart in *Casablanca*, “misinformed.”

As one example, in Lagos–Wright (2005), we present a quantitative welfare analysis of monetary policy using an approach quite similar to the one used by Lucas (2000) or Cooley–Hansen (1989) in “applied” models, although—interestingly enough—the results turn out differently, and in particular we find a much higher cost of inflation.

Craig and Rocheteau (2005) provide a survey of some related quantitative work in the area. From these exercises, it is clear that it is not especially more or less difficult to calibrate a search-based model than an “applied” monetary model. It is of course true that, as always, when one introduces new elements, like general bargaining or matching technologies, one gets new parameters, and hence one needs new empirical observations or theoretical considerations to pin them down. As an example, in models with generalized Nash bargaining the relative bargaining power of buyers and sellers can be easily calibrated to match the average markup (price over marginal cost) in the data. Alternatively, one can derive the effective bargaining weights endogenously in competitive search equilibrium models—i.e., in models with price posting and directed search. It is also true that many models in monetary theory appear far removed from the workhorse of quantitative macroeconomics, the

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<sup>11</sup>To make one more reference back to the original conference volume, Cass and Shell (1980) also use the expression “basic approach,” where at the time they were referring to microfoundations provided by the overlapping-generations model. They argue it “has two general features which we believe are indispensable to the development of macroeconomics as an intellectually convincing discipline. . . . First it is genuinely dynamic. . . . Second it is fundamentally disaggregative.” They go on to say that they “firmly believe that a satisfactory general theory must, at a minimum, encompass some diversity among households as well as some variety among commodities.” These words still ring true today, but can be used I think to even better describe some of the developments in monetary theory in the generation since they were written.

<sup>12</sup>I want to mention that there is a less well-known alternative to standard search theory that has been used in the microfoundations of money, based on private information (Williamson and Wright 1994, Trejos 1999, Berentsen and Rocheteau 2004).

stochastic growth model, but this appearance is superficial. It is easy enough to integrate, say, Lagos–Wright (2005) with a standard business cycle model along the lines of Hansen (1985), with capital, labor, firms, technology shocks, and so on.<sup>13</sup>

Admittedly it is fairly recently that microfounded monetary economics has become quantitative—although earlier examples of Molico (1997) and Shi (1998) are notable—whereas people have been calibrating cash-in-advance models of the type advocated by Lucas (1980) at the original conference since Cooley–Hansen (1989). This is as it should be. Much less was known about search-based theories back then, and more work had to be done before they could reasonably be taken to the data. Of course it was *feasible* to calibrate Kiyotaki and Wright (1989) at the time, but it would probably not have been a good idea. One ought not go to the data too soon; any class of models should reach a certain level of maturity first. This may involve years of trying alternative formulations, different assumptions, and so on. We are closer to that stage in “basic” monetary economics now, and I look forward to seeing new empirical applications. Yet there is also much to be done in monetary *theory*, and the articles published here pursue this line, very much in the spirit of the original conference. Is it the case that all the issues in monetary economics are quantitative? Surely not. Should modern monetary economics endeavor to become more quantitative? Sure, but these conferences are mainly about models.<sup>14</sup>

To conclude, the first thing to say is that I am very pleased with the way the conference turned out. I also want to say that I find it interesting to look back at the progress made since the first conference. When I began thinking about monetary economics in the 1980s and made some tentative first steps with Kiyotaki, our models were embarrassingly primitive. We had indivisible goods and money, which along with severe inventory restrictions meant prices were exogenous; we thought we needed exactly three goods and three agent types; agents interacted exclusively by bumping into each other at random; there seemed no hope of getting standard labor, capital, or banks into the mix; and so on. The articles [mentioned] here indicate how far the science has come. Yet there is much to be done and no one should be satisfied with the status quo—which is what keeps it fun. I will resist offering suggestions or predictions for future work beyond what has been mentioned already (e.g., introducing fiscal considerations, pursuing rate of return dominance, continuing quantitative work, and

thinking more about international monetary issues), but I look forward to seeing what develops. It would be nice to be around for the next conference, “Models of Monetary Economies III: Deep Space 9” or whatever they call it. With luck, we won’t have to wait another 25 years.

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<sup>13</sup>See, e.g., Aruoba and Wright (2004) for a very simple approach and Aruoba, Waller, and Wright (2005) for more complicated but potentially more interesting approaches.

<sup>14</sup>I have two coauthors, one of which is reputed to have said, “I can’t imagine an interesting question in economics to which the answer is a number;” whereas the other said, “I can’t imagine an interesting question in economics to which the answer is not a number.” As with most extreme positions, both of these seem wrong. “How much would you pay as a fraction of your income to get inflation from 10 percent to 0?” does seem like an interesting question, but so does “Should Europe have one currency or  $N$  currencies?” And to anyone who claims that the latter is really a question to which the answer actually is a number, I would add, “and why?”

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