The Early History of Rational and Implicit Expectations

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During the 1950s, three alternate models of endogenous expectations formation were developed: adaptive, rational, and implicit. While the first two are well known, the history of implicit expectations has not been dealt with, nor has the early history of rational expectations itself. The objects of this paper are, therefore, to survey the early development of these approaches, the reactions to them during the period of their dissemination, and their initial utilization. In the first section of the paper, we focus on the recollections of those who developed the models—John Muth and Edwin Mills—and those who were intimately involved in their development, including Herbert Simon, Marc Nerlove, Michael Lovell, and others. The recollections were collected by means of identical questions put to these personalities and supplementary questions based upon their initial replies, as will be seen below. In the second section, we will present the recollections of those who attended the December 1959 meeting of the Econometric Society where Muth and Mills presented papers outlining their alternative models. Again, identical questions were
put to those who participated at the meeting. In the third section, we provide the recollections of those who used the alternative approaches in a microeconometric testing program, such as Nerlove, and those who applied one or other of the models on the theoretical level in a general equilibrium model, such as Roy Radner and T. Negishi, and also those who initially used Muth’s approach in a macroeconomic framework, such as Edwin Phelps and Robert Lucas. Before proceeding, however, a brief description of the three approaches—adaptive, implicit, and rational expectations—is called for.

Lovell (1986, 111) provided concise and lucid descriptions of the alternate approaches to expectations “developed in the 1950’s.” The “adaptive approach” developed by Nerlove (1954), among others, emanated from J. R. Hicks’s (1939) notion of the “elasticity of expectations.” In its simplest form, this approach can be set out as follows:

\[ P_t = A_{t-1} + \lambda(P_{t-1} - A_{t-1}), \]

where \( P \) is the predicted value, \( A \) is the actual value, \( (P - A) \) is the forecast “error,” and \( \lambda \) is the weight attached to the forecast error. According to Lovell (1986, 112), by hypothesizing “reasonable stochastic properties,” it seemed “reasonable...to hypothesize that expectations” were unbiased, that is, that there was a zero expected value of the error in the forecast (\( \epsilon \)) such that

\[ \epsilon = P - A. \]

Two alternatives were then proposed to the adaptive approach, by Mills (1957a, 1957b, 1959a) and Muth (1959a, 1959b, 1961). In his seminal studies of inventory behavior, Mills (1957a, 1957b) presented his notion of implicit expectations. This was based upon the conjecture that there was no correlation between the prediction error and the actual realization. On the basis of this “restriction,” as Lovell (1986, 112) put it, “the basic assumption of the regression model is satisfied with the anticipated variable selected as the dependent variable.” This can be represented as follows:

\[ P = \alpha_0 + \alpha_1 A + \epsilon, \]

where \( \alpha_0 = 0, \alpha_1 = 1 \), and \( E(\epsilon) = 0 \), that is, the expected value of the “error term” is zero. Based upon this argument, the actual realization was used by Mills as a proxy “for the unobserved anticipated level of sales” in his study of inventory behavior (Mills 1957b; Lovell 1986, 112).
Muth, for his part, developed a hypothesis just the opposite of Mills’s implicit expectations approach. Muth’s rational expectations hypothesis “required that the forecast error be distributed independently of the anticipated value” (Muth 1961; Lovell 1986, 112). This can be represented as

\[ A = \beta_0 + \beta_1 P + \varepsilon, \]

where \( \beta_0 = 1, \beta_1 = 1, \) and \( E(\varepsilon) = 0. \) In this case, \( P \) must be uncorrelated with \( \varepsilon, \) so that \( \varepsilon \) must be correlated with \( A, \) and thus the variance of \( P \) is smaller than that of \( A. \) As Lovell (1986, 112) noted, “All this is precisely the reverse of Mills’ implicit expectations, which have a larger variance than the actual realizations.” Lovell also characterized the condition for what he called “full rationality” and made the distinction between “weak rationality” and “strong” (full) rationality (113), but more about this below. At this point, let us turn to the recollections of those involved in the development of the implicit and rational expectations hypotheses.

**Early Development, 1950s**

Much has been written about how the rational expectations approach was applied at the macroeconomic level, and the debate between its protagonists and critics is well documented in both the history of thought and methodology literature. What has not been dealt with up to now, however, is the “ancient history”—as Marc Nerlove put it—or as Michael Lovell called it, the “genesis” of the rational and implicit expectations approaches in terms of the events and interactions between the personalities involved. And, as will be seen, the story is essentially one of independent and parallel development, albeit with elements of contact between those involved with the work of both Muth and Mills over the 1950s and early 1960s. A number of key personalities can be mentioned at the outset, who influenced—via direct or indirect contact—and in turn were influenced by the approach of Muth and Mills. These include Nerlove, Lovell, Simon, Richard Cyert, Zvi Griliches, Franco Modigliani, and Martin Bronfenbrenner, among others. Questions regarding the early work of Mills and Muth were put to them regarding the background to their work and other influences on them. Moreover, both Muth and Mills were asked similar questions regarding the development of their models.
Marc Nerlove (1991a) became “familiar with Mills’ work” as early as 1958 when, as he recalled, “I taught part time at [Johns] Hopkins Fall Semester 1958–59, and had lunch with Mills nearly every week. I recall objecting to Mills’ idea [implicit expectations] because realized future prices could only measure expectations with an error which would, at best, lead to estimates of their effects in behavioral relations biased toward zero. But Muth’s idea corrected that problem by substituting conditional expected values (expectation in the statistical sense).”

As for possible outside influences, we also asked about Fritz Machlup’s role and influence upon the personalities involved in the development of rational and implicit expectations. The reason for this was that Machlup had claimed some part in the early development of rational expectations. Machlup’s 1952 book, *The Economics of Sellers’ Competition*, “placed much emphasis” on what he called three decades later “‘induced revisions of subjective expectations,’ induced by the inevitable learning experience of the market participants” (1983, 173). Machlup continued on to say in his 1983 paper “The Rationality of ‘Rational Expectations’” that his 1952 treatment of expectations—especially in the sections titled “Objective Changes and Subjective Expectations” (206–7) and “The Adjustment of Price Expectations” (279–80)—played “a role in what has come to be called . . . the ‘rational expectations’ hypothesis” (1983, 173), although this went unnoticed by most observers at the time, including Nerlove and Mills himself, on their own account.

When asked about his time as a graduate student at Johns Hopkins in the early 1950s, the influence of Machlup, and whether he thought Machlup had developed an “early” version of the rational expectations approach in his 1952 book, Nerlove (1992) replied as follows: “I was a graduate student at Hopkins 1952–54 and took all my economic theory from Fritz Machlup. We graduate students were enlisted to proofread the *Economics of Sellers’ Competition*, but I don’t recall anything like Rational Expectations. I no longer have my copy of the book so I can’t check and have never seen Fritz’s *Kredit und Kapital* paper. . . . My guess, however, is that Fritz may have had something like perfect foresight [Nerlove’s emphasis], which is close to Mills’ implicit price expectations, but not at all the same thing as Rational Expectations.” Nerlove continued: “From my standpoint, the key thing about Rational Expectations was its connection to the stochastic specification [Nerlove’s emphasis]. This is why a conditional expectation, based on information up to the time the action is taken, is an improvement over implicit
Edwin Mills (1991c), for his part, recalled that

I read the *Economics of Sellers’ Competition*, but I cannot tell you when. Probably when I was teaching industrial organization at Hopkins in the late 1950s. I have no recollection of anything in it that approximates the Rational Expectations hypothesis, but I have not read it in several decades. It certainly did not directly affect my work, but most important ideas in economics float around the literature for some time before they are defined precisely and incorporated in formal models. Machlup’s book may fit in that category. I know that I never thought that I was influenced by the book in my thinking on the subject, and I am sure I never discussed the issues with Machlup. I knew him well; he was very kind to me as a junior faculty member. I also knew him when we were both at Princeton.

Michael Lovell also became involved with the work of both Mills and Muth quite early in his own career. When asked about Mills’s early work, Lovell (1991a) recalled that

I became acquainted with Ed Mills’ articles on inventories [Mills 1954–55] in connection with my own thesis research. Later I got to know him quite well when he visited at the Cowles Foundation [Yale] for a semester. I thought that my assumption that the forecast error might be proportional to the observed change, which I attributed to Keynes, seemed to work better than his “implicit expectations.” Ed finished his book while at Cowles, and *I thought then and think now that his contribution has been underappreciated, particularly by those who do not follow Muth in restricting “rational expectations” to the case in which the forecast error is distributed independently of the prediction.* [our emphasis]


As for his contact with Muth, Lovell (1991a) recalled the following in an earlier letter:
I met Jack Muth when he visited Yale for the 1961–62 academic year [Lovell was a staff member at the Cowles Foundation and on the faculty at Yale at the time]—he helped me on a sticky point in a paper I was writing about seasonal adjustment; we talked some about inventories, the Carnegie Tech quadratic modeling of production scheduling and inventory behavior, and Rational Expectations; I had my doubts about Rational Expectations because the evidence that I had seen up to then was that forecasters did not obtain as much precision as they could have obtained with readily available information. I was asked to referee Ed Mills's book [Mills 1962] for *Econometrica*, but because of time pressures passed it along to Jack; Jack intended to run the inventory regressions in the reverse direction from Mills, which was the implication of rational as opposed to implicit expectations. . . . he punched the data on IBM cards but before testing his own approach he ran Mills's model. Jack found he could not replicate the results that Ed had reported in his book.

When asked about this in correspondence, Mills (1991b) replied that “I do not remember if Jack Muth reviewed my 1962 book. I do remember that he contacted me saying he could not reproduce some of the regressions in my book. I redid the regressions and found that the published estimates were indeed in error. My recollection attributes the errors to a very crude piece of computing machinery. . . . I still have the reestimated equations. They would have not made me write anything very different in the book.”

In correspondence on this and other issues, John Muth (1992b) wrote the following: “You asked whether I ever did get to review Mills’ 1962 book. While I visited the Cowles Foundation at Yale, I started to review it, but in the process of moving back to Carnegie-Mellon [Carnegie Tech] in Pittsburgh, the review fell through the cracks. So, although I did a lot of preliminary work on the review of it, the review was never completed. When I found that I could not replicate his results I did contact him and he replied to me.” Muth went on to say that “Mills’ expectations model did not really sink into my consciousness until I read his book [Mills 1962] and Mike Lovell published his book on sales anticipation and inventories [Hirsch and Lovell 1969]. I now believe that Mills’ work has been very much underrated in the literature and that a combination of implicit and rational expectations, sometimes called noisy Rational
Expectations, is in better agreement with the facts than either model alone” (our emphasis).

With regard to his own experience with Muth, Simon, Cyert, James March, Lucas, Leonard Rapping, and Thomas Sargent at Carnegie Mellon, Lovell (1991a)—who was an associate (1963–66) and full professor (1966–69) at the Graduate School of Industrial Administration (GSIA)—recalled that

I went to Carnegie-Mellon in 1963 rather than stay at Yale for a while longer in part because I was impressed by Jack Muth, Herb Simon, by Dean Richard Cyert and by Jim March—this was long after Franco [Modigliani] had departed to Northwestern (which turned out to be only a one year stepping stone) and MIT. Lucas, [Morton] Kamien and [Lester] Lave all arrived directly from earning their Ph.D's that same year; Rapping had been there for a semester or so. This was my first real contact with members of what people at Harvard and Chicago had called the “Chicago School” and their first contact with a “Keynesian”—indeed I was rather lonely as a minority house Keynesian at Carnegie-Mellon, particularly given my feeling that Keynes had been dead for some time. I urged Dick Cyert, a wonderfully understanding Dean, to get someone without a Chicago orientation, and he recruited Tom Sargent from Harvard—Bob Lucas and I, who talked together with Tom when he visited us for an interview, were equally enthusiastic about having Tom on board. Sargent left after a year to go into the Air Force under the terms of an ROTC commitment.

Lovell continued:

Jack Muth patiently reviewed with me the appropriate tests that Al Hirsch and I should run Rational Expectations on our Commerce data [Hirsch and Lovell 1969]—but while everyone knew all about Jack's skiing trips, he did not talk about his current research; the senior faculty decided not to give him tenure, but then promoted [me] the next year, which strikes me as an excellent counter example to the Rational Expectations hypothesis.

At GSIA I worked on my inventory project pretty much alone, talking about it primarily with my co-author Al Hirsch. . . . I talked to my colleagues a lot at GSIA, including Simon and Lucas, but I was not able to interest them in the topic of expectations [our emphasis]. The book with Al Hirsch [1969] was reviewed favorably in the JEL,
but didn’t attract much attention, perhaps because at the time there seemed to be little interest in inventories, business cycles or expectations—and partly because there was little going on, I moved to other topics.

Interestingly enough, it was Mills who reviewed the Hirsch and Lovell volume in the *JEL* (1971). In his review, Mills wrote that their work was “certainly the most important empirical study of expectations in many years.” Mills also said that in the Hirsch and Lovell study the question was posed “whether Ferber’s law of expectations formation provides a better explanation of the . . . data than does Nerlove’s adaptive expectations hypothesis” (108). Mills then said that he was “surprised that both hypotheses seemed to fit the data quite well.” He went on to say that in the Lovell-Hirsch volume, “further analysis tests the structural hypothesis of Ferber and Nerlove against Muth’s Rational Expectations hypothesis, Mills’ implicit expectations hypothesis, and Lovell’s weighted average of static and perfect forecasts. The Muth and Mills hypotheses perform better than the Ferber and Nerlove hypotheses at the industry level, but not as well at the firm level. But Lovell’s hypothesis seems to outperform both sets of alternative hypotheses” (108).

The thing to recall here is that the data used in the Hirsch and Lovell study were microdata. That is, as Mills (1971, 107) wrote, the study was based on “quarterly data on sales and inventories from a sample of . . . manufacturing firms,” and, “in addition to actual sales and inventories,” the firms reported “anticipated sales and inventories one and two quarters in advance”; while at the industry level, the data were “aggregated into seven durable and seven non-durable industries.” Indeed, the Lovell-Hirsch study was perhaps one of the most important microeconometric tests of the Muth and Mills approaches to expectations up to that point, as noted by Mills himself in his *JEL* review of it.

In a letter, Zvi Griliches (1991) also recalled additional aspects regarding the Muth and Mills approaches to expectations. As he put it, “In 1957 and 1958, I think, I shared an office with Jack Muth at the University of Chicago. I was finishing my dissertation while he was a postdoc (with a still unfinished dissertation) from Carnegie Mellon. We talked a lot about his work. I thought it very good *but not revolutionary*. But then one rarely does, when one is standing too close to it” (our emphasis).

1. The data were from the Office of Business Economics of the U.S. Department of Commerce.
Griliches went on to say that

Muth’s work should be seen against the background of two related strands of work: Modigliani on expectations first at Illinois (overlapping with [Dorothy] Brady and [Margaret] Reid) and Eisner (the Hopkins connection for Mills), and then at Carnegie, where the [Charles] Holt-Simon-Modigliani-Muth volume on inventory control preceded Muth’s specific contribution; and the permanent vs. transitory income stream starting from [Milton] Friedman’s contribution to the Kuznets and Friedman volume, the work by Dorothy Brady and Margaret Reid on income and consumption surveys, [Phillip] Cagan’s adaptive inflationary expectations model (under Friedman’s supervision), Nerlove’s extension of it to agricultural price expectations (a Ph.D. thesis at Hopkins and another connection to Mills), [Henri] Theil bringing Koyck’s model to Chicago, and my own subsequent work on distributed lags.

Griliches continued:

Against that background, the first Muth paper was memorable by showing when adaptive expectations were reasonable.2 What started out as a reasonable approximation was given a rigorous theoretical backing, a desirable property for the empirical part of the Chicago School aspiring also to live by Cowles Commission goals. The second famous paper [1961] was interpreted more as a demonstration that if you don’t assume that people are fools and/or can be fooled all the time, you don’t get stupid results (such as repeated cycles). But that was already the Friedman-Cagan-Nerlove message, and hence the paper itself did not feel revolutionary to us. Nobody at that time took seriously the interpretation that economic actors use the econometrician’s models to form their forecasts. That was either a debating point or a technical move to insure the intellectual consistency of the argument. The general Chicago opinion about the quality and relevance of the existing macro models was rather low at that time (and later on) and did not really see its way towards using this idea constructively. It was left to Lucas (and Rapping) to rediscover the traces of these ideas, both at Carnegie and Chicago, and push them to their logical conclusion.

2. The paper was written in 1960 but not published until 1981 in the Lucas and Sargent volume.
One additional recollection deserves to be related at this point regarding the microeconomic origins of the rational expectations approach. As Martin Bronfenbrenner (1992) recalled, “I was a visiting Professor at Carnegie [Tech] in 1955 (substituting for Franco Modigliani) while Jack [Muth] was working on his ‘rational expectations’ dissertation. We talked on several occasions, and I can tell you why I, at least, underestimated the significance of what Jack was doing. The reason was that Jack’s oral explanation to me—I was not on his thesis committee—was exclusively in connection with microeconomic problems and I was not smart enough to see the possible macroeconomic applications of his work, as Bob Lucas would do some years later, also at Carnegie.”

Finally, with regard to the influence of Simon and Modigliani on the development of Muth’s ideas, there is a problem in establishing “direct” as against “indirect” influence. In correspondence Simon (1991) wrote that John Muth “undoubtedly felt [Simon’s emphasis] challenged by my views on bounded rationality (and says so in his REH *Econometrica* paper). . . . Muth, Modigliani, Holt and I were jointly engaged in a study of production smoothing that eventuated in our book on the subject. Muth, then a graduate student, was assigned the task of looking at the problems of prediction of future sales that were involved in the projects, and in the course of this work made his start toward the findings reported in his [1960] *JASA* paper.”

Simon continued as follows:

Jack, along with the economic faculty, were very well aware of my bounded rationality views and my skepticism about neoclassical theory, but that had nothing to do with this particular project, which proceeded with the neoclassical paradigm. . . . a little earlier, Modigliani, together with Emil Grunberg, wrote their paper on the possibility of economic predictions (would they be falsified by reactions if they were published?), and I was closely involved in that project, providing (as indicated in a footnote) a proof of their central theorem using Brouwer’s fixed-point theorem. I did not co-author that paper, however, but published a parallel one on election predictions, based on the same theorem. The fixed-point here is very closely related to the rational expectations equilibrium, and some (e.g. Chipman) have thought that in these papers Grunberg, Modigliani, and I laid the groundwork for REH. I will let history judge that, but if true, it is ironic. In any
event, those two papers may have had some influence on Jack’s thinking about forecasting.

What clearly did have influence on his thinking was, without any challenge, his disagreement with bounded rationality, and his belief that “people are more rational than Simon thinks they are.”

Richard Cyert, who was dean of the GSIA at Carnegie Tech at the time, also replied (1991) that

Jack Muth was on the faculty at Carnegie Tech where I was also a faculty member, so I know a great deal about the paper. . . . I don’t think any of us thought Jack’s paper was revolutionary at the time it was published. It was viewed as an answer to discussions and arguments that we had on the faculty, stimulated by Herbert Simon’s attack on the concept of rationality in economics. Jack wrote the paper in a spirit of rebellion, Jack always being a rebel. He wanted to show that Herb was not only wrong, but that economists should emphasize rationality even more. I don’t think that he himself saw the potential of using the concept in dynamic economics as though it had more validity than other ad hoc assumptions.

But let us leave the last word regarding these points to John Muth himself. In correspondence Muth (1992b) replied to an assertion made in a letter from Marc Nerlove (1991b) in which Nerlove said “the story I recall is that the REH originated at Carnegie in the late 1950s in the form of a dare: Herbert Simon challenged Muth to come up with a theory of information use as ‘rational’ as the theory economists used to explain the allocation of other resources.” In his letter, Muth maintained that Nerlove’s assertion “that Herbert Simon challenged me to come up with a theory of information as rational as the theory economists use to explain the allocation of other resources is definitely not true. There never was any such challenge. The only thing even remotely resembling that is when Franco Modigliani assigned a problem in class to explain executive salaries. Herb Simon presented a model to explain that phenomenon. As a member of Modigliani’s class, I tried to develop one too, but it wasn’t very good.”

Now, much more is involved in the story surrounding the origins and development of Muth’s 1961 paper than has appeared in either the economics or the history of economics literature. When Muth first presented and circulated the original version of his paper (1959a, 1959b), reaction
to his approach was somewhat quiet. While a number of rising stars of economics and econometrics who have since come to prominence, and even some who were already established figures in the profession, actually attended the session at the December 1959 Washington meeting of the Econometric Society, where Muth gave the original version of his 1961 paper, it still made no immediate impact on economics or economists, as will be seen below. In the decade following the original 1959 presentations by Muth and Mills, however, both rational and implicit expectations had considerable influence in microeconomics, albeit this has been overlooked by most historians of economics. For example, the papers presented by Muth and Mills in 1959 generated discussion and debate in one of the foremost economic journals, the Quarterly Journal of Economics, in May 1961, and this even before Muth’s article appeared in Econometrica in July 1961. In addition, Negishi’s 1964 note in Econometrica may be seen as a crucial “missing link” in the transition from the Walrasian microeconomic to the Walrasian general equilibrium macroeconomic application of the rational expectations approach. Finally, Hirsch and Lovell (1969) illustrated the extent of microeconomic testing of the approaches of both Muth and Mills, and this even before the rational expectations revolution in macroeconomics began. Below, we outline the evolution of Mills’s approach to expectations over the 1950s and early 1960s.

From 1954 onwards, Edwin Mills had been working on his own implicit expectations approach. Mills’s earliest expectations paper was based upon his doctoral dissertation, “The Theory of Inventory Decisions,” submitted in 1955 at Birmingham and supervised by Frank Hahn and William Gorman. In fact, Hahn suggested that Mills work on this topic. In that paper, which was titled “Expectations, Uncertainty, and Inventory Fluctuations” and published in the Review of Economic Studies (1954–55), Mills first outlined a “total supply” equation and then went on to develop a notion of “optimal inventory.”

In a completely overlooked paper published in the October 1957 issue of Management Science titled “Expectations and Undesired Inventory” (1957a), Mills presented a more sophisticated version of his approach, which included what he called a “micro-model” of a firm’s output based on a “forecast” or “best guess” of “demand for its product” during a period (105).

The object of that paper, according to Mills (1957a, 105), was actually to present a “model . . . with the aid of which an estimate, however
crude, of the amount of undesired inventory in the economy is made from market data.” In his paper, Mills first presented the micromodel and then, in the section he called “The Macro-Model,” proceeded to obtain an estimate of economy-wide undesired inventory. He went on to say that “estimates of the amount of undesired inventory in the economy are obtained by fitting equation (5) to the postwar national income statistics of aggregate production, sales and inventories” (107). Mills used “the quarterly seasonally adjusted components of Gross National Product at annual rates” (107) for this purpose and went on to even further improve his estimating procedure by “using not equation (5) but rather period to period changes,” that is, first difference form, so as to get around the problem of autocorrelation of the residuals. As he put it, “Better estimates of the coefficients are likely to be obtained if the residuals are randomly distributed through time” (107). Mills reported that the actual first difference form of the equation he estimated by “traditional least squares” showed “no autocorrelation” (107). By combining his supply-demand and inventory analysis with an analysis of the error structure involved in forecasting demand and its implications for inventory adjustment, Mills had actually outlined, for the first time, his “implicit expectations” approach in this almost totally overlooked paper and actually applied it to the macroeconomy.

With regard to Mills’s treatment of uncertainty and error in forecasts, in the February 1959 issue of the Quarterly Journal of Economics, Mills published a paper titled “Uncertainty and Price Theory” (1959b), the contents of which he discussed with Robert Solow, Edward Kuh, Machlup, Evsey Domar, and Abba Lerner (116). In that paper, he said that the firm “is unable to predict $x$ [demand] in advance because it does not know which of its possible values $u$ [the random term] will take” (117). Mills had earlier presented a random variable $u_t$ in his paper “The Theory of Inventory Decisions” (1957b). It should also be noted here that Mills’s model in his paper “Expectations and Undesired Inventory” (1957a) also had a random error term for the forecasts (106). Moreover, in his QJE paper Mills (1959b, 117 n. 1) explicitly used the terms risk and uncertainty interchangeably, albeit recognizing the possibility of a distinction between them.

By 1959, then, Mills had fully developed the “implicit expectations” model he presented at the December 1959 Econometric Society meeting in a paper titled “Expectations, Inventories, and the Stability of Competitive Markets” (Mills 1959a), which forms the core—according
to Mills himself—of his 1962 book *Price, Output, and Inventory Policy*.

**December 1959 and Its Aftermath**

The fact that both Muth and Mills presented their models of expectations at the December 1959 meeting of the Econometric Society has been overlooked by historians of economic thought, so that reactions to their papers given at this meeting have also been overlooked until now. There are two important things to recall here in this regard:

2. There was an important exchange between Mills and Nerlove—who attended both the session at which Mills presented his paper, and that at which Muth presented his paper—on the relative efficacy of the use of implicit and rational expectations as against adaptive expectations in *micro*, not macro, economics in the May 1961 issue of the *Quarterly Journal of Economics*, and this even before Muth’s 1961 paper was even published (Mills 1961; Nerlove 1961a).

Now, the role of conferences and meetings in the dissemination of new knowledge and approaches in economics and new methods of economic analysis is indeed problematic. On the one hand, new ideas are sometimes introduced on such occasions, with papers usually circulated prior to, at, or even after their presentation. On the other hand, as Robert Solow (1991) put it in a letter regarding the issue: “I think that the Econometric Society meeting is the wrong place to look. Nobody ever learns or understands new ideas at those meetings. In fact the sessions are very sparsely attended, except for the occasional blockbuster. The most anyone ever learns is that there is a paper that he or she might like to read. Nothing is absorbed. The presentations are too short; and as soon as one is over it is followed by another.” Or, as Robert Clower (1991) asserted, also in a letter: “Let me assure you, that nothing revolutionary ever appeared in a paper given at the Econometric Society. Revolutions are not in the papers. They come about because someone, or a group of people, later on find something exciting and form a school around it.”
Indeed, most economists who attended the Muth and Mills sessions did not take up the rational and implicit expectations approach, and, as will be seen, they were not too impressed by Muth’s and Mills’s papers even after they were published. However, rational and implicit expectations were possible “solutions” looking for potential “problems,” and it was the microeconomic problem of modeling expectations that faced Lovell, Nerlove, Negishi, and Radner, who used it to model expectations formation or as a benchmark to compare the relative efficacy of alternate approaches to expectations, not at the macroeconomic level, but at the microeconomic level.

In this part of the essay we focus on the sessions at the December 1959 meeting of the Econometric Society where, on the same day (30 December), Mills gave his implicit expectations paper at a morning session, while Muth gave his paper at an afternoon session. The recollections of Muth and Mills regarding the sessions at which they gave their papers, along with those of discussants and participants in the sessions, will be presented. We then go on to present the retrospective impressions and assessments of a number of prominent mainstream economists of the alternative approaches, that is, rational and implicit expectations.


In order to arrive at a fairly simple explanation of the way expectations are formed, we advance the hypothesis that they are essentially the same as the predictions of the relevant economic theory. In particular, the hypothesis asserts two things: (1) information is scarce, and the economic system generally does not waste it, and (2) the way expectations are formed depends specifically on the structure of the entire system. Methods of analysis, which are appropriate under special conditions, are described in the context of an isolated market with a fixed production lag. The interpretative value of the hypothesis is illustrated by the introduction of commodity speculation. Finally, it is shown that the rational expectations hypothesis is in good agreement with the facts, at least if one views the empirical results generously.

An abstract of Mills’s paper, on the other hand, did not appear (“Report” 1960, 699). Albert Hart was the discussant for Muth’s paper, while
Gerard Debreu was the discussant for Mills’s paper ("Report" 1960, 698, 704).

As Muth (1991a) recalled regarding his 1959 paper and other aspects of his rational expectations approach, "My recollection is that the presentation was the early part of the 1961 paper: the basic model involving an isolated market. Albert Hart was the discussant at the 1959 meeting, and he gave me a page of his typewritten comments. . . . My recollection is that Hart emphasized only that the variables used in the paper are deviations from equilibrium values, not absolute quantities."

In the published version of the paper (Muth 1961), Muth had mentioned that Modigliani had commented on his 1959 paper. In this regard, Muth recalled that "the comments of Modigliani were mostly the sort that you get when you circulate the paper among respected colleagues. My recollection is that he did not have any substantial criticism or suggestions, only some of a stylistic and minor nature. He had no problem with the basic concept of the paper, because the model itself is extremely simple, not involving great technical intricacy. (The implications of the theory on monetary theory had, of course, not yet been drawn by Lucas, Sargent & Wallace, and others.)"

As regards the influence of Grunberg and Modigliani’s paper (1954) and that of Simon (1954) on his own 1959 paper—taking into account that the former was cited by Muth in the published version of his 1959 paper (Muth 1961)—Muth replied as follows:

Concerning the Grunberg and Modigliani 1954 *Journal of Political Economy* paper (see also Herbert A. Simon’s *Public Opinion Quarterly* paper), certainly I was aware of the problem and their results. One must draw a distinction, however, between their concerns and those of the Rational Expectations model. Their primary interest was the question, is it necessarily true that “in reacting to the published prediction of a future event, individuals influence the course of events and therefore falsify the prediction”? The forecast is treated as an exogenous variable in a static, deterministic world. According to the Rational Expectations hypothesis, expectations are determined by the structure of the system. The forecasts are endogenous. In addition, the model assumes a system which is dynamic and stochastic.

Muth continued: “Some authors have referred to Rational Expectations as self-fulfilling expectations. Self-fulfilling expectations are expectations in which ‘confident error generates its own spurious confirmation’
(Robert K. Merton: Social Theory and Social Structure, Free Press, 1957, 128). The important word is ‘spurious.’ The expectation is an exogenous variable in this model, as well, and thus differs from the Rational Expectations model.”

In a subsequent letter Muth (1991b) dealt with the influence of Herbert Simon on his work and his 1959 paper:

I was a student of Herbert Simon while in the graduate program at Carnegie Tech in 1952–1956. I took a management course from him in 1953, based largely on his book on public administration; also, a course on mathematical social science in 1955, which involved a number of readings such as his articles in economics, including part of a book by Andronov & Chaikin on the theory of oscillations. I also participated as a graduate student in a project sponsored by the Office of Naval Research on aggregate scheduling. The faculty members involved in that were Charles Holt, Franco Modigliani, Herbert Simon, and initially Robert Schlaifer.

At this time Simon’s work was mostly in organizational theory and economics. It was before he got involved in the simulation of cognitive processes, which started around 1956 or 1957. Needless to say, with all this exposure he has had very important general influence on me.

However, there is very little direct influence on my work in expectations. Indeed, the Rational Expectations model runs counter to his main beliefs about appropriate types of economic models and his own modeling technique in two ways. (1) Rational Expectations involves notions of equilibrium, while he favors a process, or algorithmic, model of steps taken by decision makers. (2) Rational expectations obviously involves rational choice in its purest form, while he advocates approaches based upon aspiration level models of search.

A major project I have been working on recently is much closer to the process and search approach in modeling economic activity, although it does not use the aspiration level model.

Finally, with regard to Mills's 1957 paper on implicit expectations and his view of Mills's approach and its relation to his own work, Muth (1991a) wrote: “Implicit expectations were introduced by Edwin Mills in his 1957 Econometrica paper. I was familiar with the paper but had not studied it carefully enough to notice his expectations model. Several years ago, Michael Lovell [Hirsch and Lovell 1969] pointed out the
distinction between rational and implicit expectations. In Rational Expectations the error is uncorrelated with the forecast, while in implicit expectations the error is uncorrelated with the realization. I now feel that a combination of the two is a closer approximation to reality (see my 1985 *Eastern Economic Journal* paper).

Mills, for his part, also recalled, in a series of letters, some aspects of the session at which he presented his own paper in 1959. As he said, “Until the Econometric Society meeting at which, by coincidence and at separate sessions, he and I presented our papers, I did not know John Muth. . . . I do not have a copy of my paper, but the substance appears in my book *Price, Output and Inventory Policy* [1962] Ch. 3 et seq. . . . I do recall that Marc Nerlove directed me to Muth’s paper and pointed out the similarity of approaches” (1991a). In a further letter he added that “I do not remember if I attended Muth’s Econometric Society session, or if he attended my session. I certainly read a draft of his paper not long afterward. . . . Gerard Debreu discussed my paper. I recall a comment ‘I believe that if Mills’ result is correct, it is true under much more general conditions than he analyzes.’ This could be a figment of my memory, but it is certainly a nice Debreu-like comment” (1991b).

Debreu (1991), for his part, wrote, “I have no reason to doubt the *Econometrica* report on the session I attended. I do not have, however, a copy of Edwin Mills’ paper or of the comments I might have made after his presentation. The sentence that Edwin Mills quotes in his letter of November 6, 1991 to you sounds plausible, but I cannot remember uttering it.” Debreu continued: “Again it seems likely, given the program of the meeting, that I attended the afternoon session at which John Muth spoke. I am certain that I listened at least once to his presentation of his paper at about that time, and I distinctly recall finding its main ideas quite stimulating. I do not believe, however, that I perceived them or Mills’ ideas as ‘revolutionary,’ nor do I remember noticing the ‘similarity’ between the two papers.”

Marc Nerlove, who as Mills recalled “directed” him “to Muth’s paper,” also recalled some aspects of the sessions at which Muth and Mills gave their papers. “I do recall the Washington meetings, but I was familiar with Mills’ work before then. . . . I did go to both presentations, made a lot of comments from the floor (I was pretty brash) and even introduced the two (I also knew Muth slightly through his brother Richard F.). Later I refereed both Muth’s JASA and *Econometrica* papers for their respective journals” (1991a). Nerlove continued: “Muth’s idea was
terribly appealing, at least to me. . . . One reason the idea probably didn’t take off until much later is that it was almost impossible to implement empirically. This is later corrected by the modification called ‘quasi-RE’ discussed in my 1979 book [Nerlove et al. 1979].”

Now, while Edwin Mills did not remember attending John Muth’s session, it would seem that Muth, for his part, did attend that of Mills. For, as Muth (1992b) wrote, “I don’t really recall very much in detail about Mills’ session at the December 1959 meeting of the Econometric Society. After all, it was over thirty years ago. Also, I don’t recall seeing a copy of his paper entitled ‘Expectations, Inventories and the Stability of Competitive Markets,’ which eventually became chapter 3 of his 1962 book. I think my brother, Richard F. Muth, did review one of his articles for publication somewhere. Mills’ expectations model did not really sink into my consciousness until I read his book and Mike Lovell published his book on sales anticipations and inventories.”

William Cooper (1991), who also attended the Muth session, said that while his memory was “at best hazy,”

my main memory is that Albert Hart (who I knew both at Chicago and Columbia) served as the discussant of Jack’s paper and really only talked around it. It may be that Hart was influenced by his collaboration with J. R. Hicks on a text which dealt with a national income approach to economics—a very good text which I used in the course I taught at Chicago. You may recall that the topic of expectations is discussed at length in Hicks’ *Value and Capital* and it also formed a central theme in Hart’s doctoral dissertation. . . . This was probably the reason he was selected as a discussant but, nevertheless, he didn’t seem to understand (or at least he didn’t direct himself) what Muth was saying on *Rational Expectations*—probably because neither he nor Hicks had considered the topic from that standpoint.

Richard Eckaus (1991), who also attended the Muth session, wrote that “Hart rather offhandedly dismissed” Muth’s paper and that he recalled “thinking that there was more to it than Hart acknowledged.” Irma Adelman (1992) attended both the sessions of Muth and Mills and wrote, “I remember attending the presentation of both the Muth paper and the Mills paper. But my feeling at the time was merely that it offered an interesting variant on adaptive expectations. The revolutionary aspect of *Rational Expectations* comes in only when the impact of government policy is included in the rational expectation formation of all institutional
actors in the economy, not just inventories. As I remember, these tie-ins were not made in the Mills and Muth papers.”

Now, when viewed from the perspective of the rational expectations revolution in macroeconomics, the “revolutionary aspect of Rational Expectations,” when it first appeared, was indeed overlooked by many prominent observers. Alternatively, it was not deemed as being revolutionary, then or now. For example, Solow (1991) wrote, “I am also pretty sure that I read Muth’s paper when it came out. And I must have known what Ed Mills was up to because we were friends and colleagues. I did not then think of the concept of rational expectations as revolutionary. To tell you the truth, I would not describe it that way now. My guess is that it picks up some extra aura from being associated with the reversion to complete and perfect market-clearing in macroeconomics, although the two ideas are conceptually entirely distinct.”

Other prominent economists also recalled their initial reaction to Muth’s rational expectations approach. Phillip Cagan (1991), for his part, while not having attended the Muth session at the 1959 meeting, still wrote in a letter that

I certainly did not attend Muth’s paper. However, I was a visiting professor at Carnegie Mellon in the early 1960s where Muth was teaching, and I became aware of his paper. I was especially interested because it extended the concept of expectations that I had introduced in a study of hyperinflations. At that time Muth’s contribution was not of course referred to as a revolutionary new concept of “rational expectations.” It was interesting but did not seem particularly important. It became important, as I see it, when more and more of the literature was taken over by the modelers of theoretical constructs who needed some way to handle expectations. Someone noticed that expectations could be modeled by assuming that agents expected what the model predicted. Eureka! The theoretical modelers were off and running—ever since.

Some of us have doubts about the usefulness for empirical work of Rational Expectations (see my article in the December 91 issue of Journal of International Macroeconomics), though this in no way takes away from Muth’s insightful contribution. Nevertheless, it has obviously been a boon for the theoreticians.

While he also did not attend the session at which Muth presented his paper, M. L. Burstein (1991) wrote that
I do recall that Jack [Muth] worked on his dissertation [at the University of Chicago] in the same room I—and other members of the Harberger Public Finance Workshop worked (...Zvi Griliches, Yehuda Grunfeld, Bill Niskanen, Marc Nerlove, Lester Telser, et al.). ...I can report this:

1. I had no idea that Jack Muth’s work was monumentally important—and it is such.
2. I clearly recall the cobweb-theory orientation of his study—and picked up the flaw in cobweb theory (very important then in what passed for advanced economics) that was thus revealed. But the larger issues, the amplitude of his discovery, completely passed me by. I did get the hang around 1974. Thus I doubt if my 1968 book was significantly influenced by this result.
3. That said I and others wrote about the ineffectiveness of tax-policy and refunding operations in the early 1960s. See my 1963 book—but I don’t think I had Jack’s framework in mind.

Harold Watts (1991), who was assistant director of the Cowles Foundation at Yale (1958–61) and stayed on at Cowles until 1963—spanning the time during which both Muth and Mills were there—also wrote:

While I do not have any recollections about the ES sessions in December 1959, I was acquainted with both Jack Muth and Ed Mills shortly after that time. ...I was familiar with much of the expectational literature, having discussed a paper by Marc Nerlove that involved both stock adjustments and adaptive expectations. I also worked informally with Bob Eisner when he visited at Cowles and was working on plans and expectations in connection with investment forecasting.

My general recollection is that Rational Expectations were met with relatively little interest because there seemed to be many more interesting and relevant things to do at the time. It even seems to me that the originators concurred in that assessment. The basic ideas were interesting and original, but did not seem to be revolutionary (and still don’t in my estimation).

On the other hand, G. Edward Schuh (1991) actually “broadened” the “puzzle” of the initial acceptance of the rational and implicit expectations approaches when he wrote:
You are correct that I did participate in the 1959 meetings of the Econometric Society... In fact, I gave a paper at that meeting—the first professional paper of my career! However, I did not participate... in either of the sessions to which you refer.

I have some thoughts for you, however, for whatever they might be worth. Part of these is directed to broadening the puzzle. To be specific, the work of Koyck and Nerlove with distributed lag models should have set the stage for more ready acceptance of the Rational Expectations model. In the case of both authors, they emphasized that these models could be interpreted either as a means to use past information from the economy to make predictions about the expectations for the future, or as a means of measuring the lag in response to information generated in the current period.

Schuh continued on to say that

from my own perspective, I was using these new distributed lag models in my dissertation research at the University of Chicago. My dissertation was completed on the job at Purdue University, so it took me a couple of years to completion. I remember reading Muth's article and trying to draw on it in the context of the models I was using. In fact, I may well have made reference to it in both my thesis and the journal articles that came out of it. As a young professional, however, I must confess to not appreciating the significance of what he did, although later in my career I repeatedly reminded students that the root of the Rational Expectations approach went back to Muth's article.

Why did people like me not recognize the significance of this early work? I think in part it was because we didn’t find using an expectations model in principle all that significant. After all, most of us were forced to read Keynes, even at the University of Chicago(!), and he talked a lot about expectations. In effect, so what was new other than a means of taking them into account in empirical research by means of the distributed lag models. One of the puzzles to me is that a professional as bright as Nerlove seemed so close to stumbling onto the Rational Expectations approach and did not seem to grasp it—in retrospect. He did, however, realize the horrendous identification problem involved in using the Koyck model. I also think it was in part a consequence of the failure to recognize the full implications of the Rational Expectations perspective for policy and for econometric estimation.
For what it is worth, the professors at the University of Chicago who reviewed my thesis, and they included Friedman and Griliches, did not seem to recognize the significance of the Muth article—or of the work of Nerlove and Koyck which pointed in that direction.

In his letter cited above, William Cooper (1991) made the following salient points regarding the initial reception of Muth's rational expectations approach. As he put it, “You may want to take note of the rule that Jacob Marschak once told me about: it takes 20 years, on average, for a new idea to become adopted in economics. This may also be true in other sciences and may have something to do with generation gaps, generation turnovers and like phenomena. In addition you can add that Jack was young and relatively unknown at the time and it was not even clear that he was an economist, as the term was used in those days, and in fact he subsequently turned more towards management science and operations research.”

Moreover, Stanley Lebergott (1991) actually broadened the question regarding the initial reaction to and early adoption of rational expectations when he wrote:

You ask why “Rational Expectations ... (was) not immediately adopted by the economics profession” in 1959?

... Aside from any other reasons it appeared in the midst of many other signals, most of which were also plausible and were more in line with prior expectations.

I do not remember the original 1959 presentation but you could also ask: why was it not adopted even a decade later? I was a member of an FRB Consultants Committee, and we sponsored the Econometrics of Price Determination Conference, October 31, 1970 (Published June 1972 by the FRB).

Lucas presented a paper on the Natural Rate hypothesis, adopting Muth. You can read (p. 113) Frank Fisher's dismissal: “Lucas appears to believe that the notion that one cannot fool all the people all the time implies one cannot fool all the people even some of the time. ... when policies change average expectations will (not necessarily) be right in the short run.”

Time dating is obviously not trivial. And where you predict what the outcome will be, between the short run and the limit, depends on your prior perspective. In this instance it also was confounded with
a) Those of us involved in say the SSRC-Brookings model were still trying to work out the awful empirical fitting. Persistently going back to every first principle was not the up front task when the sub-optimizing was so onerous. Do note (in the FRB volume) the extended paper by Nerlove had to face the Rational Expectations critique, and really did not. Shirley Almon’s article also appeared in *Econometrica* in 1965—well after Muth, and did not. (Both were included, even handedly, in Zellner’s *Readings in Economic Statistics and Econometrics* (1968).) Surely that forcibly suggests adaptive applications [expectations] was alive and well at Harvard and Chicago. (And Nerlove was at Yale somewhere in this period.)

b) Theil’s insights still prevailed. Adopting a new paradigm, in sum, was mixed up with the war between Keynesians and monetarists. But Frank Fisher’s point about dating expectations was not trivial, may even still be with us.

Franklin Fisher (1991), for his part, wrote,

So far as I can remember I did not attend either of the sessions at the 1959 meetings in which you are interested.

For what it is worth, I did read Muth’s paper when it was published. My view of why it had so little impact is (although I have not reread it) that it is quite obscurely written. It does not convey the sense of something truly important.

And, as for how he saw the “empirical validity” of the rational expectations approach when it first appeared, Wassily Leontief (1991) wrote: “Let me admit that I must have been, at that time, skeptical of the empirical validity of the theory of Rational Expectations as I am now.”

In contrast to all this, Arnold Zellner (1991), while not being able to recall having attended either of the sessions “at which Muth and Mills presented their papers,” still wrote:

However in 1961, I read the Muth paper in *Econometrica* and was quite impressed by it since it put forward a novel and attractive approach. I included the paper in my advanced graduate course in econometrics, Dept. of Economics, U. of Wisconsin at Madison and then in a 1968 book, *Readings in Economic Statistics and Econometrics*, Little, Brown and Co., Boston that I edited.

I was just as surprised as you are that Muth’s ideas did not have an instantaneous impact. Perhaps his paper was somewhat difficult to
read or many were taken up with adaptive expectations. In any event, the profession was slow to recognize Muth’s contribution.

But let us leave the last word on this issue to Muth himself, who in a letter (1992a) gave us his views regarding the initial acceptance of rational expectations:

I would like to react to a statement made by Mike Lovell that rational expectations finally took hold after lying dormant for a decade. A similar view was expressed by Robert Lucas and Thomas Sargent in the introduction to their readings book, *Rational Expectations and Econometric Practice* (University of Minnesota Press, 1980). The editors’ introduction opens with the statement: “After a remarkably quiet first decade John Muth’s idea of ‘Rational Expectations’ has taken hold, or taken off, in an equally remarkable way.”

The *Social Science Citation Index* lists 17 references to my paper for the years 1966–70, their first years of publication. The paper was referred to, but not used, by Kenneth Arrow in his “The Economic Implications of Learning by Doing” (*Review of Economic Studies* 29 [1962], pp. 155–173). It was also reprinted in 1968 by Arnold Zellner (*Readings in Economic Statistics and Econometrics*. Little, Brown). This attention is not much compared with that of later years, but it does not really qualify as neglect.

Having said that, there is indeed a time lag in adopting new concepts and ideas in economics. Maybe something weird was happening during the decade of the 1960’s. Perhaps new ideas take hold only as the older generation is gradually replaced by the new. Most of my former teachers at Carnegie-Mellon have only very limited enthusiasm for the rational expectations hypothesis, even though it is now commonplace in financial and macroeconomics.

Based upon the material presented above and that to be discussed below, we would agree with Muth that the reaction to his rational expectations and Mills’s implicit expectations approaches after their initial presentation (1957–59) and publication (1961–62) cannot be characterized, in his words, “as neglect.” For, what was actually overlooked were their *macroeconomic* implications. Indeed, over the period 1959–69, both the rational and implicit expectations approaches did affect microeconomic and microeconometric analysis, as will now be shown.
Utilization, 1959–69

In an article titled “Rational Expectations in Microeconomic Models: An Overview,” published over two decades after Muth and Mills presented their original papers on rational and implicit expectations, economists were reminded by James Jordan and Roy Radner (1982, 203) that Radner, for his part, had introduced the notion of a rational expectations equilibrium in a paper in French (Radner 1967; also see Radner 1968). Despite this, Radner’s 1967 paper was not mentioned in Hashem Pesaran’s important book The Limits to Rational Expectations (1987), nor in Omar Hamouda and Robin Rowley’s work Expectations, Equilibrium, and Dynamics (1988).

But this is not all, for there is also no mention in Pesaran’s book of Mills at all, nor of the Mills-Nerlove QJE debate in 1961. Moreover, Negishi’s important 1964 paper linking rational expectations and general equilibrium is not mentioned by Pesaran, nor is Radner 1968, Hirsch and Lovell 1969, or even Muth 1985. Hamouda and Rowley (1988), for their part, also do not mention Negishi 1964 or Hirsch and Lovell 1969. And, for that matter, neither Muth 1985 nor Lovell 1986 is mentioned by them, although other much less important articles do receive attention. Indeed, it is as if the literature on rational and implicit expectations and on its microeconometric testing over the period 1959–69 and even later has been simply overlooked.

In this section, we focus, therefore, on what we consider to be the seminal works on rational and implicit expectations on the microeconomic and microeconometric levels over the period 1959–69. In this context, we will also consider the three “missing links” between the micro- and macroapplications of the notion of rational expectations, the work of Negishi (1964, 1965) and Radner (1967, 1968), who linked rational expectations to general equilibrium theory, and that of Edmund Phelps (1966), who linked “endogenous expectation formation” to macroanalysis of inflation and unemployment processes, thus providing them with appropriate microfoundations.

Now, the original version of Muth’s 1961 paper was actually circulated as “Carnegie Institute of Technology and ONR Research Memo No. 65” (Muth 1959b), and Nerlove, for one, cited it at length in his 1961 paper “Time-Series Analysis of the Supply of Agricultural Products” (Nerlove 1961b), published in the book edited by Heady titled Agricultural Supply Functions (1961). Indeed, it would seem that it was
this “research memo” that was submitted to *Econometrica* for publication by Muth, being the written and expanded version of the paper he had presented at the December 1959 Econometric Society meeting; this, in light of the fact that Muth, as he explained in a letter (1991a), could not find a copy of his original conference paper and also was not sure if a written version existed. For, as Nerlove recalled (and as cited above), he refereed Muth’s paper for *Econometrica*. And the fact of the matter is that in his 1961 paper, Nerlove (1961b, 47–48) wrote that “from the standpoint of economic theory, the Rational Expectations hypothesis is the most attractive hypothesis concerning the formation of expectations which has been formulated to date and which is sufficiently simple to be used in connection with time-series analysis.”

He went on to list three reasons why the rational expectations hypothesis appeared to him to be “far more reasonable than it first sounds.” Because of the importance and relevance of Nerlove’s points, and in light of the fact that they comprise one of the first published reactions to Muth’s approach, they are cited here at length:

First, the Rational Expectations hypothesis does not require that every farmer or business-man formulate a correct and relevant economic model. Economists cannot even do that! What it does require is that the representative firm behave as if it had made predictions on the basis of the same economic model used by the economist to analyze industry behavior. It implies expectations which are constructs of the same nature as “certainty equivalents,” “adaptive expectations,” and “supply functions”—indeed almost any other economic concept. Furthermore, the expectations thus generated will be entirely consistent with the economic model used and will have the additional advantage of not assuming less rationality in the formation of expectations than in other forms of economic behavior. If one is prepared, for the purposes of a predictive model, to assume that on average producers maximize profits, it does not make sense to assume that they err greatly in making forecasts on the average, or at least err more than the model used to predict their behavior. The Rational Expectations hypothesis [is] an attractive one from the aesthetic standpoint and because of its consistency both with general economic theory and the particular economic model underlying the statistical analysis undertaken [Nerlove’s emphases].
Second, if expectations were not rational, at least on the average, then insofar as our economic model approximates reality we should tend to find a small group of individuals, whose expectations are better than those of the rest, gradually driving the others out of business. This is essentially the same argument used to support the hypothesis of profit maximization under conditions of competition: Those who do not maximize do not survive; therefore, those who survive must achieve maximum profits on the average.

Third, insofar as this argument is unconvincing, Muth shows that it is possible to introduce elements of irrationality into the picture. Such deviations from rationality are, of course, unimportant when they are unsystematic. This is what we mean when we speak of rational expectations “on the average.” But if the deviations are systematic, biased expectations may result. Muth [here Nerlove is referring to Muth 1959b] gives an example of how such biases may be introduced. (48–49)

Nerlove went on to say that

the acid test of any hypothesis is whether it proves useful in explaining actual behavior, not what it assumes and what it does not assume. The Rational Expectations hypothesis has only recently been proposed, and so faced the test of application to only a very limited extent. However, what limited evidence has been brought to bear tends to support the rational expectations hypothesis: Simple cobweb models which are based on extrapolative or adaptive expectations suggest that we should observe negative serial correlations in prices and cycles of relatively short duration. Both predictions are contradicted by experience. On the other hand, as Muth [once again referring to Muth 1959b] shows, simple cobweb models based on Rational Expectations suggest that prices will exhibit positive serial correlation and cycles longer than three or four production periods (depending on which way cycles are measured).

Interestingly enough, in his “debate” with Mills on the efficacy of adaptive versus rational and implicit expectations, Nerlove (1961a, 336 n. 2) wrote that in his treatment of Muth's model “the discussion here follows the somewhat simple derivation of the appropriate Rational Expectations given in my paper [Nerlove 1961b]” as it appeared in the conference volume mentioned above. In fact, as early as in his 1961
Young and Darity Jr. / Rational and Implicit Expectations

“debate” with Mills, Nerlove had made a number of crucial points regarding the Muth and Mills approach to expectations. Nerlove (1961a, 338) concluded that

it may be said that Mills has left an important element out of his analysis, namely the stochastic. When this element is introduced, it is possible to exhibit circumstances under which adaptive expectations are “...always wrong whenever the market is out of equilibrium,” but they are not [Nerlove's emphasis] “...wrong in a very simple and systematic way.” . . . It follows that Mills is quite right concerning the unsatisfactory nature of adaptive expectations in general. It remains to be seen, however, how successful we will be in introducing the general principles stressed by Mills and Muth into econometric practice and the analysis of stability problems.

Mills (1961, 330), for his part, in his 1961 paper “The Use of Adaptive Expectations in Stability Analysis,” asserted that “an important problem in economics, which has received little systematic study, is to decide how much information decision makers should be assumed to possess in making price, output, purchase and other decisions. At the extreme, some writers argue that it is best to assume that decision-makers are always right in the sense that they know the true probability distribution of the variable they are trying to predict.” Mills added the following in a footnote: “This position has been taken independently by John Muth in an unpublished paper [referring to Muth 1959b, it would seem] . . . and by the author in a forthcoming book [referring to Mills 1962].”

Mills went on also to take issue with Nerlove’s earlier QJE paper “Adaptive Expectations and Cobweb Phenomena” (Nerlove 1958) and said, “It is not plausible to assume that a decision-maker, who is otherwise assumed to behave rationally, continues to form expectations in a way which is continuously contradicted by experience in a mechanical and easily perceived fashion” (333–34). In response to this, Nerlove (1961a, 338) mathematically showed “that under certain circumstances adaptive expectations are not subject to the objection which Mills raises.” He did this by comparing the stability conditions in the Muth and Mills approaches and the concomitant expectations generated with, as he put it, “the stability conditions given in my earlier paper [Nerlove 1958].” He concluded that “it is easy to see that instability is impossible when the adaptive expectations are rational” (338; our emphasis). Indeed, in this exchange with Mills, Nerlove actually repeated arguments he made in
his 1961 paper mentioned above (see Nerlove 1961b, 53), in which he mathematically generated adaptive expectations which “are also rational ones” (Nerlove’s emphasis) and said that “the estimation techniques proposed for models based on adaptive expectations are inappropriate in the case of Rational Expectations, despite the fact that Rational Expectations turn out to be of the adaptive form” (Nerlove’s emphasis). Nerlove (1961b, 53) concluded that “it may be said that Rational Expectations are difficult to find even for very simple economic models. This does not mean, however, that they are not worth finding. They have the property of being entirely consistent with the economic model into which they are introduced. The little qualitative evidence developed supports the Rational Expectations hypothesis. There is clearly a need for more evidence of a quantitative character.”

It should also be noted that besides generating reactions from those interested in its microeconomic and microeconometric applications, Muth’s rational expectations article (1961) generated another reaction in *Econometrica*, that is, a somewhat overlooked—albeit important—paper by John Bossons and Modigliani (1966) titled “Statistical vs. Structural Explanations of Understatement and Regressivity in ‘Rational’ Expectations.” Now, while dealt with by Hamouda and Rowley (1988, 59, 93–94), neither the problem of “regressivity” nor the article by Bossons and Modigliani (1966) is even mentioned by Pesaran (1987) in his book. This omission would not be that significant, if not for the fact that, as Muth (1991c) himself recalled, “the article by Bossons and Modigliani pointed out that I had misinterpreted the regressivity phenomenon and identified it improperly with the regression effect (underextrapolation in their terminology). Bossons was primarily responsible for the article. We had discussed it while it was still in draft form. He got me cold.”

Perhaps the most significant microeconometric testing of rational expectations and implicit expectations over the period 1959–69 was that undertaken by Hirsch and Lovell (1969), at least according to Mills—who reviewed their volume for the *JEL* (1971, 107), as noted above—and to Muth. Indeed, as Muth indicated in correspondence, the Hirsch and Lovell volume did influence him, although he was not at Carnegie Tech when it was written and did not participate in the reviewing process of the manuscript, as others—later to be very actively involved in the rational expectations revolution in macroeconomics—were. For, in his letter cited above, Muth (1991c) also wrote that “by the time the Hirsch and
Young and Darity Jr. / Rational and Implicit Expectations

Lovell book was being written, I had already moved to Michigan State from Carnegie Tech, and so was not as conveniently available for review of the manuscript even if Lovell had wanted me to do it. At that time both Sargent and Prescott were at Carnegie Tech. I later read the book and enjoyed it.” In a subsequent letter, as we have noted above, Muth (1992b) added that “Mills’ expectations model did not really sink into my consciousness until I read his book [Mills 1962] and Mike Lovell published his book on sales anticipations and inventories [Hirsch and Lovell 1969].”

It is indeed interesting that Muth mentioned the fact that both Edward Prescott and Thomas Sargent were at Carnegie Tech at the time Lovell was writing his book with Hirsch, for both Prescott and Sargent actually read and made “constructive comments on preliminary drafts of the manuscript” (Hirsch and Lovell 1969, vi). In fact, when queried as to the nature of his “constructive comments” on the manuscript of the Hirsch and Lovell volume, and his view of rational expectations and the results they published—which did not support either Muth’s rational expectations or Mills’s implicit expectations approaches (Hirsch and Lovell 1969, 169–81)—Prescott (1991) replied that “I do not remember exactly the nature of my discussions with Mike Lovell concerning Rational Expectations. I believe that we discussed the meaning of the question ‘What is your expectation of . . . ?’ My position is that this is an ambiguous question and that we as economists should not treat answers to this question as measurements of anything.” Prescott went on to say that

in taking this position, I was influenced by what I learned in a statistical decision theory course that Morris H. DeGroot taught on the question of eliciting priors. For this question to have meaning, the loss function of the respondent must be known. Another problem is that what people say they would do in a given situation is often not what they actually do in that situation. I adopted the Lucas position that Rational Expectations, or for that matter maximizing behavior, is not something that can be tested. A model with Rational Expectations properties can be tested, but not Rational Expectations itself.

Prescott concluded, “Muth displayed genius in proposing the Rational Expectations requirement for economic models.”

In fact, here Prescott was repeating his argument that the rational expectations hypothesis cannot be directly tested empirically. For, as he said almost fifteen years before (Prescott 1977, 30), “like utility,
expectations are not observed, and surveys cannot be used to test the Rational Expectations hypothesis. One can only test if some theory, whether it incorporates Rational Expectations or for that matter, irrational expectations, is or is not consistent with observations.” For his part, Sargent (1991), in a note to one of the present authors, wrote the following in answer to the same questions put to Prescott: “I can’t remember my reactions to Lovell’s findings in detail. Lovell was the person who first taught me about rational expectations. I have thought for a long time that the line of work he started is a good one, and that it deserves attention. Dave Ruble at the Minneapolis Fed has done some very interesting and good stuff along these lines recently. The measurement problems and measurement error problems are continuing to receive attention.”

In our view, the approach of and the results reported by Hirsch and Lovell in 1969 regarding both rational and implicit expectations deserve detailed consideration here, for their study represents the culmination of a period over which those at the forefront of the utilization of rational and implicit expectations (Nerlove, Negishi, Radner) and in macroeconomics (Lucas, Sargent, Prescott) familiarized themselves with the Muth and Mills approaches. Indeed, as Robert Lucas (1991) wrote, “I got the idea of Rational Expectations from John Muth, who was a colleague of mine at Carnegie Tech in the mid 1960s. The first use I made of this construct was in a 1966 working paper entitled ‘Optimal Investment with Rational Expectations,’ which was published as chapter 5 of a book Thomas Sargent and I edited, *Rational Expectations and Econometric Practice*. Edward Prescott and I used Rational Expectations in our 1971 *Econometrica* paper, ‘Investment Under Uncertainty.’” Lucas continued: “I know nothing of the history of this idea before Muth’s work. I had been influenced by work at Chicago on expectations by Phillip Cagan, Marc Nerlove, and Milton Friedman, but none of them was very close to Rational Expectations. According to Muth, Edwin Mills . . . was closer, but he didn’t have it either. I was not aware of Hart’s work, or (except through Muth) of Modigliani’s.” Lucas concluded as follows: “I must say, I continue to be amazed at the originality of Muth’s formulation. I just do not know of any predecessors that were at all close. I have no idea how he came up with it.”

Hirsch and Lovell (1969, 169) opened their discussion of what they termed “‘rational’ or ‘implicit’ expectations” by saying that “in contrast to the behavioral models of Ferber and Nerlove, the Rational Expectations hypothesis advanced by Muth [1961] is a normative [our
emphasis] proposition rather than a theory of how expectations are actually formed. Muth reserved the term ‘rational’ for expectations possessing certain desirable attributes.” “Is it reasonable,” they went on to ask, “to expect that respondents will report anticipations that are rational in the sense in which Muth defines the term? A brief review of three types of forecasts [considered in chapter 4.2.2 of their book] will remind us that this is an empirical question that cannot be resolved solely on the basis of theoretical consideration” (170). Hirsch and Lovell continued: “We shall first consider the empirical question of whether observed anticipations satisfy certain restrictions that Muth imposed in formulating his concept of Rational Expectations then [in section 5.4.2 of their book] we shall examine empirical evidence concerning Mills’ [1957a, 1957b, 1962] conjecture that the actual realization can be fruitfully employed as an implicit proxy for ex ante variables in econometric applications whenever anticipations are not directly observable.”

In their test of Muth’s rational expectations approach as applied to microeconomic data from the Office of Business Economics (recall the discussion of Mills’s review of their book, cited above), Hirsch and Lovell (1969, 175–77) concluded that “in summary, it appears that firms do not rationally employ the information provided by past sales experience in preparing either their short or their long sales forecasts. . . . Unless the decision maker has a long history of experience with the nature of the forecasts that are being provided to him, he is unable to achieve the degree of forecasting precision suggested by Muth’s definition of Rational Expectations.”

Now, when dealing with “actual realizations as a proxy for anticipations,” that is, implicit expectations, Hirsch and Lovell suggested “that actual realizations may be employed as a proxy for anticipations in econometric work when observations on expected sales are not available. This is the ‘implicit expectations’ approach used by Edwin Mills [1957a, 1957b, 1962] in his pathbreaking investigations of the production decision” (177). Interestingly enough, Hirsch and Lovell added that “the procedure was implicit in a pioneering econometric model by Klein [1950] based on data on the US economy for the interwar period.” In a footnote to their statement, Hirsch and Lovell said that “Mills’ concept of ‘implicit expectations’ involves an alternative specification from Muth’s of the stochastic properties of the disturbance. . . . If the forecast is derived from a survey of the firm’s customers the disturbance will have the desired property; ‘implicit’ as opposed to ‘rational expectations’ will
be obtained” (177 n. 32). Hirsch and Lovell went on to say that “the fruitfulness of the ‘implicit’ expectations concept depends, in part, upon how precise firms are at predicting sales volume” (177) and found that “the use of the actual realization as an implicit proxy fails only at the individual firm level” (185) and also that “Lovell’s [1961] slightly more complicated proxy procedure for approximating anticipated sales” (185) worked better than that of Mills at the aggregate.

With regard to his own 1961 paper titled “Manufacturers’ Inventories, Sales Expectations, and the Acceleration Principle,” which was also originally given at the December 1959 meeting of the Econometric Society and was also published in the same issue of *Econometrica* [July 1961] as Muth’s paper, Lovell (1991b) recalled the comments of the discussant, John Meyer, and also those of Arthur Okun regarding it: “I checked my back files and found a letter from John Meyer responding to a query from me to elaborate on the comments he made on my inventory paper at the Washington meetings—his question had to do with the accuracy of the commerce data on inventories rather than about expectations. I also found a comment by Art Okun, a colleague at Cowles, suggesting that I should allow for the possibility that production levels might be revised during the three month observation period, which enters into the interpretation of the expectations coefficient in my model.”

Lovell also said that he was indeed familiar with Mills’s work, having cited Mills 1957a in his own *Econometrica* paper (Lovell 1961). Lovell went on to say that “I looked in the latest *Social Science Citation Index* and observed that Muth’s paper on ‘Properties of some Short-Run Business Forecasts,’ *Eastern Economic Journal* July–September 1985, is not being cited. This is a revised version of the paper that he presented at a session I organized of the Eastern Economic Association. . . . I think this is unfortunate, because Jack made some important modifications of his original theory.”

Lovell is indeed correct in focusing upon Muth’s 1985 *EEJ* paper, since Muth also referred to its importance in the ongoing development of his own approach to expectations, as he wrote in his letters cited above. For example, in a letter, Muth (1991a) referred to his 1985 paper when he said that “I now feel that a combination of the two [rational and implicit expectations] is a closer approximation to reality.” Muth (1991c) expanded on this when he wrote, “I have myself done a little bit of empirical work and the results were not entirely favorable to the Rational Expectations hypothesis. This article appeared in the *Eastern Economic*
Journal. . . . I wrote Lovell about my results and he invited me to present them at the 1985 Eastern Economic Association in Pittsburgh and also in the AEA meeting in NY the following winter.”

Now, this is not the place for a comprehensive discussion of Muth’s 1985 paper, which is, as Lovell (1991b) observed, a paper that unfortunately “is not being cited.” However, we would agree with both Lovell and Muth as to its importance, since, as Lovell (1986, 117) noted in his own American Economic Review paper “Tests of the Rational Expectations Hypothesis,” “Muth was led by the negative empirical evidence to substantially modify his original model of Rational Expectations” (our emphasis).

Negishi, for his part, did not attend the sessions of Muth and Mills at the 1959 Washington meeting of the Econometric Society, but, as he recalled, “I recognized, however, the significance of their contributions soon after they were published, at least in my own way of interpretations. I published a small note ‘Stability and Rationality of Extrapolative Expectations’ in Econometrica (1964) in which I referred to Muth’s 1961 Econometrica paper. Also, in the Chapter on Rational Expectations of my 1965 book Kakaku to Haibun no Riron (Theory of Price and Allocation) I referred to Mills’ 1962 book as well as Muth’s 1961 paper” (1991).

In Negishi’s 1964 “small note” he used Muth’s approach to “give some rational basis to extrapolative expectations” (1964, 649). In other words, Negishi proposed rational expectations as the basis for the endogenous expectational assumption underlying “the dynamic stability of multiple markets” in the system originally proposed by Arrow and Debreu (1954), as manifest in Enthoven and Arrow 1956. In fact, Negishi actually extended Muth’s approach in this regard. As he put it,

The rational expectation hypothesis advanced by Muth . . . is that expectations are essentially the same as the predictions of the relevant economic theory; that the economy generally does not waste information; and that expectations depend specifically on the structure of the entire system. However, since there is cost of information and computation, expectations may also be called rational when they are formed as the prediction based on a simplified and approximated version of the economic theory, using only limited amounts of information on a part of the system. Extrapolative expectations will be derived below as the prediction of the equilibrium by the use of estimated excess
demand functions, and it will be shown that the coefficients of expectations thus derived are such that the system of multiple markets is stable when gross substitutability and tâtonnement are assumed. (649)

By making rational expectations the expectational basis of the Arrow-Debreu general equilibrium model, Negishi provided fertile ground for Radner to further develop the Arrow-Debreu approach. For, as Radner (1992) wrote, “my own interest in the subject arose from my attempt to extend the Arrow-Debreu model to the case of incomplete markets. The first results of this attempt were published in 1967 [Radner 1967]... This paper dealt simultaneously with two aspects of ‘rational expectations’: consistency in the expectations of future prices, and making inferences about other agents’ information from equilibrium prices. I like to think it had little impact because it was published in French!”

In fact, Radner expanded on this the next year, this time in English, in his seminal paper in *Econometrica* titled “Competitive Equilibrium under Uncertainty” (1968). This paper, as Radner (1991, 452 nn. 11–12) later noted, “explored the consequences and problems of extending the AD [Arrow-Debreu] model to the case in which different agents have different information,” and in it he “argued that heterogeneity of information among agents would lead to incomplete markets, and hence to a sequence of markets.”

Finally, as regards the extension of the application of endogenous expectations formation, we turn to Phelps 1966 as the “missing link” between micro- and macroanalyses of inflation and unemployment. For, in a highly significant Cowles discussion paper, Phelps (1966, 2, 59) initiated the process of establishing “dynamic” microfoundations for the macroeconomic analysis of inflation and unemployment by providing an endogenous expectational basis for public prediction of policy making by economic agents (63–64). As Phelps (1991) recalled, “I conceived of the expectations-augmented Phillips curve in 1965, possibly in the spring, and waited to get to work on it until my work on growth theory was finished and my sabbatical leave began (Jan. 1, 1966). I worked singlehandedly on it in Room Q at LSE from mid-January until May. After a long refereeing process, this work appeared as Discussion Paper 214 in the Cowles Foundation Series, August 25, 1966. This became the *Economica* paper (1967).”

Phelps went on to say the following:
But I had not come up with a real model, or even a concrete image, of how expectations drive price setting and wage setting. In the summer, at Sidgwick Avenue in Cambridge, I struggled with the problem of how to model wage-setting in a way giving a central role to expectations. At first I focused on price expectations as a factor—but always these formulations failed to satisfy me. Finally, in Philadelphia, probably in October, I realized that the firm’s wages will be conditioned by its expectations of other firms’ wages—in view of its concern about its competitiveness in the labor market, hence its quit rate, etc. This led to the at places clumsy model of the wage-wage spiral and the equilibrium unemployment rate, which was first a Discussion paper at Penn . . . published as a supplement to the July–August 1968 JPE.

Phelps described those who influenced him: “Influences? Fellner on expectations—but he didn’t have the spatial dimension, across firms. I had to dream that up myself. Much later, Alchian’s paper suggested that this spatial aspect was in Keynes’ General Theory. But I had not understood that! And Keynes’ model was quite different.” To this he added the following:

I had not read Hart, nor the Modigliani-Grunberg piece. I may have seen the Richardson piece, but I don’t remember. Simon? No influence. Through Fellner I acquired an appreciation for Lindahl and for Hayek, so through Fellner I became a vessel ready to carry the continental emphasis on expectations to the “econometric” terrain of wage and price dynamics. Of course, the econometric modeling by Cagan, Nerlove, and Mills must have given me some [Phelps’s emphasis] support in embarking on an expectational approach. But those papers, while containing a xε somewhere, did not point to the conception of the informational imperfection of the labor market that I was finally drawn to.

Phelps concluded that “because Friedman’s model was easier, it got more attention than mine—but it was not a model of unemployment [Phelps’s emphasis], and it was of modest empirical appeal. I felt that the Lucas-Rapping model suffered the same limitations, and it contained a theoretical flaw (see my May 1969 AEA Papers and Proceedings piece). For those who continued to want to use a market-clearing approach I suggested the islands parable in that 1969 essay.”
To sum up, the following may be said. Over the decade 1959–69, a growing number of economists became aware of rational and implicit expectations (especially those who were later at the forefront of the rational expectations revolution in macroeconomics), albeit it was first applied at the microeconomic and first tested at the microeconometric level only. Over the period 1964–67, the link between rational expectations and general equilibrium was made by Negishi and Radner. During the mid-1960s, Sargent, according to his own account of events, had been taught rational expectations by Lovell, and both Sargent and Prescott had commented on the manuscript of what became the Hirsch and Lovell volume (1969)—in which rational and implicit expectations were empirically tested at the microeconometric level. As early as 1966, Lucas had applied rational expectations to model expectations formation regarding investment at the macroeconomic level, according to Lucas himself. Finally, between 1966 and 1969, Phelps extended the endogenous approach to expectations to deal with problems of analyzing inflation and unemployment. Thus, by the early 1970s the outcome of the extensive microeconomic and microeconometric utilization of rational and implicit expectations that had occurred over the 1960s was its extension beyond the microeconomic sphere of inquiry, bringing about the rational expectations revolution in macroeconomics. In other words, there was a continuity in the process of diffusion and dissemination of rational and implicit expectations, rather than a discontinuity between their early microeconomic history and their eventual utilization in macroeconomics.

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