

**The Fisher Relation in the Great  
Depression and the Great Recession**

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## **The Fisher Relation in the Great Depression and the Great Recession\***

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**Abstract.** The Fisher relation played a very different role in debates surrounding the Great Depression and the more recent Great Recession. This paper explores some of these differences, and suggests an explanation for them derived from a sketch of the idea's evolution between the two events, thus providing a brief case study of the interaction of economic ideas and economic events that is a central feature of the History of Economic Thought.

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**Key words:** Interest rates, nominal vs. real, Inflation, deflation, expectations, depression, recession, Keynesian economics, monetarism, monetary policy.

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The so-called *Fisher relation* plays a central part in today's work-horse models of monetary policy and it often finds a place in general discussion of such issues far beyond the boundaries of the academic literature. It had been making appearances in monetary debates long before the Great Depression began, indeed long before Irving Fisher himself discussed it with such skill and thoroughness in (1896) that his name became firmly and perpetually attached to it.<sup>1</sup> Even so, this idea's specific place in macroeconomics has changed considerably over the years. This paper explores the remarkable differences between the roles it played in the late 1920s and early '30s on the one hand, and in the last decade or so, on the other. It also sketches an explanation for these differences in terms of the more general evolution of macroeconomics over the intervening years. In short, it presents a brief case study of the reciprocal interaction between the evolution of economic ideas and economic events which makes, or at least ought to make, its own history integral to the study of economic analysis.

### *The Fisher relation*

The phrase *Fisher relation* covers more than one idea. Nowadays, it is routinely used to denote a systematic tendency for variations in the expected inflation rate to be reflected - fully, it is usually assumed - in the difference between market rates of return expressed in units of current and constant purchasing power respectively. It is often set out in a simple equation

$$(1) \quad r = R + p(e)$$

Here,  $r$  is the nominal interest rate in question,  $R$  is its real counterpart, often as a practical matter treated as a constant determined by the fundamental forces of productivity and thrift, and  $p(e)$  is the expected rate of price inflation.<sup>2</sup>

The idea embodied in this equation is, however, more helpfully labeled the *Fisher effect*. It expresses a rather complicated hypothesis about the economy's behavior which requires both that agents form expectations about the future time path of prices, and that asset markets permit these expectations to be reflected fully in the difference between the nominal and real interest rates. Underlying this hypothesis is, of course, a less complicated idea, also sometimes referred to as the Fisher relation, but which is better called simply the *Fisher distinction* between nominal and real interest rates.<sup>3</sup> While the Fisher effect, depending as it does on expectations, is inherently forward looking, the

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<sup>1</sup> Robert Dimand and Rebesa Gomez Betancourt (forthcoming ) survey this early history, and the later roles played by Fisher's ideas in the evolution of macroeconomics.

<sup>2</sup> Equation 1 is obviously written in deterministic form. In modern dynamic stochastic general equilibrium analysis,  $R$  is treated not as a constant, but as a long run steady state value of the real rate of interest, around which its actual value varies in response to various shocks, to productivity, for example. Whether this steady state value is itself constant depends upon the particular model, of course, but again as a practical matter it often is so presented. Discussion with Michael Parkin on this point has been very helpful.

<sup>3</sup> Though it is really Marshall's (1887), including the vocabulary of real and nominal, as Fisher himself acknowledged. It is a somewhat paradoxical feature of the subsequent development of the Cambridge tradition in monetary economics, that though expectations of rising prices played a key role in expositions of its analysis of monetary elements in the cycle well into the early 1930s, with the exception of Hawtrey (1913) , these did not make explicit use of the real-nominal interest rate distinction. Bridel, (1987) is a classic source on the development of this tradition, parallel to the one explored here, which is not dealt with in any detail below.

Fisher distinction can be applied with equal ease when past, current or future values of inflation and interest rates are under discussion. In what follows, I shall try to be clear about these semantic matters, even at the risk of being a little ponderous from time to time, because they can be a source of confusion.

*The Fisher relation in the Great Depression*

It is at first sight curious that the Fisher distinction, let alone the effect, seems to have played no significant part in the monetary policy discussions of 1929-1933, academic and otherwise, even though both were already established in the literature of what we would now call macroeconomics. Meltzer (2003) sums the matter up as follows

"The minutes [of meetings of the Federal Reserve Board] of the period, statements by Federal Reserve officials, and outside commentary by economists and others do not distinguish between real and nominal interest rates. Surprisingly, even Irving Fisher did not insist on this distinction. Although Fisher pointed to the decline in demand deposits in conversation with [Governor Eugene] Meyer, his preferred explanation of the prolonged decline was the asymmetric effect of deflation on debtors" (pp. 412-413)

Just about everyone at the time, that is to say, knew that prices were falling, ought to have known that this implied that real interest rates were high even though nominal rates were very low by historical standards, but no-one seems to have put matters this way.

Setting aside Fisher for the moment, to whose views we shall return in due course, Meltzer's basic point is well taken. A good example, recently documented by Romer and Romer (2012), of how even well informed contemporary commentary stopped short of explicitly deploying the distinction is provided by *Business Week* magazine. Here, the observation that deflation was interacting with the nominal interest rates set by the Fed to create further deflation was clearly and often made, but to judge from the passages quoted by Romer and Romer, the corollary that this amounted to keeping *real rates high* is explicitly noted only by these modern authors, not by those who wrote those passages in the early 1930s.

I would suggest that the close to universal absence of such explicit references from the discourse of that time is accounted for by the fact that, by the late 1920s, discussion of the Fisher distinction had already become relatively uncommon, and was largely confined to discussions of the Fisher effect, notably those of Fisher himself. Furthermore, the literature of the time treated the latter mainly as an hypothesis about predominantly long-term relationships among price level variations and interest rates, whose relevance by then had become subject to many doubts and qualifications. Such leading authorities as Ralph Hawtrey and John Maynard Keynes, both of them highly visible and influential in United States in these years, had long since either, in the first case, stopped deploying the effect altogether - compare (1919) with (1913) - or, in the second, had expressed serious doubts about its practical significance, cast in the following terms.

"Nevertheless in a period of rapidly changing prices, the money rate of interest seldom adjusts itself adequately or fast enough to prevent the real rate from becoming abnormal. For it is not the *fact* of a given rise of prices, but the *expectation* of a rise compounded of the various possible price movements and the estimated probability of each, which affects money rates." (1923, p. 20).

It is also worth noting that, because its exponents were from the outset reluctant to place any emphasis on such aggregate variables as the general price level, neither the Fisher distinction nor effect figured systematically in the then emerging and novel Austrian theory of the cycle which nevertheless focused on the interaction of bank lending rates and the expected rate of return on capital.

As to Fisher himself, he was perhaps already beginning to be regarded as something of a crank by the late 1920s, and his remaining public reputation would soon be thoroughly undermined by his very public failure to foresee the stock market crash of October 1929 or the seriousness of the real downturn that then ensued. More to the specific point, he had already himself ceased to claim anything more than marginal relevance for the effect that bears his name. *The Theory of Interest* (1930) included a lengthy chapter (Ch. XIX, pp. 399-451 [439-495]) that thoroughly updated his work on the topic, which he had begun in (1896) and carried further in (1907), and he summarized its overall lessons as follows.<sup>4</sup>

“It should be noted that in so far as there exists any adjustment in the money rate of interest to changes in the purchasing power of money, it is for the most part (1) lagged and (2) indirect. The lag, distributed, has been shown to extend over several years. The indirectness of the effect of changed purchasing power of money comes largely through the intermediate steps which affect business profits and the volume of trade, which in turn affect the demand for loans and the rate of interest. There is very little direct and conscious adjustment through foresight. Where such foresight is conspicuous, as in the final period of German inflation, there is less lag in the effects.” (p.494 [538])

It is also worth recalling that, on the threshold of the Depression, discussions of monetary policy in the United States had been conditioned by a prior six or seven years of price-level stability, and had centered on plans to give the Fed a binding legal mandate to continue to pursue it. Neither the Fisher effect nor the Fisher distinction had any direct role to play here, for obvious reasons.

It is indisputable that subsequent discussions would have been clearer, particularly to later readers in whose intellectual equipment the Fisher relation occupies a central place, had the real-nominal interest rate distinction been deployed in their course. But whether this want of clarity was crucial to the quality of anyone’s decision making in the early years of the Depression is another matter. Even the Fisher distinction was not needed by anyone familiar with then common ways of thinking about monetary policy in order to assess the significance of the behavior of interest rates under Fed control. These taught, among other things, that policy's tightness or looseness could be assessed by comparing the level of interest rates to expectations about what Henry Thornton (1802) had long before called the "rate of mercantile profit", and such a comparison could of course be made in either nominal or real terms, so long as consistency was observed. From this standpoint, the Fisher relation was an unnecessary extra embellishment.

This is why Hawtrey (1919), for example, had been able to drop it from his analytic armory without any loss of substance, and write only about nominal interest rates and expected rates of return, and why it made only occasional appearances in Knut Wicksell's later discussions (e.g 1915). To come to the Depression years themselves, it is

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<sup>4</sup> In all references to Fisher, two sets of page numbers are given, those in the original printing, and in square brackets, those in the relevant volume of the Barber, Dimand and Foster’s (1997) edition of his works

also why contemporary commentators such as those associated with *Business Week* referred to above, were able to assess correctly the significance of falling prices as an indicator of the stance of monetary policy without explicitly invoking the Fisher distinction. In addition, as Frank Steindl (1995, Chs. 1 & 3) has stressed, enough data, not least some related to the shrinking money supply, were available to judge the stance of policy along explicitly quantity-theoretic lines. Many contemporary critics of the Fed, including Hawtrey (e.g. 1932) and his sometime assistant Lauchlin Currie (e.g. 1934), not to mention Fisher himself on many occasions, derived policy advice from such data about the need to bring about vigorous money growth without referring to the real-nominal interest rate distinction. Perhaps then, the Fed's own neglect of it should not be listed high among the reasons for its unfortunate failure to heed such advice.

But though Fisher, like everyone else, had come to downplay the empirical significance of the Fisher *effect* by 1930, he nevertheless stood out from the crowd in emphasising that the very fact of its empirical unimportance implied that the *distinction* between nominal and real interest rates was of great significance:

“In actual practice, for the very lack of this perfect theoretical adjustment, the appreciation or depreciation of the monetary standard does produce a real effect on the rate of interest, and that a most vicious one . . . when the price level falls, the interest rate *nominally* falls slightly, but *really* rises greatly, and when the price level rises, the rate of interest *nominally* rises slightly, but *really* falls greatly.” (1930, pp. 493-4 [537-38] italics in original)

At first sight, then, his failure explicitly to stress the distinction subsequently, as claimed by Meltzer (2003) seems puzzling. But only at first sight, because in fact Fisher did deploy the real-nominal distinction when dealing with what Meltzer refers to as the “asymmetric effect of deflation on debtors”<sup>5</sup>. In the just quoted passage, Fisher is pointing to an example of the phenomenon that, in (1928), he had called *The Money Illusion*; and the “vicious effect” he invokes does not involve *perceived* rising real interest rates leading maximizing agents to reduce their expenditure plans. Rather it involves agents’ *failing to perceive* this rise – an error well documented by Fisher’s own empirical studies of this matter – and hence making *ex ante* choices, about borrowing in nominal terms to buy real assets, whose market outcomes will *ex post* ruin them.

This is the phenomenon underlying Fisher’s now celebrated, but then largely ignored, “debt deflation theory of great depressions” (1933), to which Meltzer obliquely refers. Fisher’s original exposition of this theory in (1932) explicitly invoked money illusion and was cast in terms of the behavior of “nominal” and “real” dollars in deflationary times. In one place it even offered its readers the following (not quite precisely formulated) advice, albeit in a passage enclosed in parentheses at the end of an appendix (number 5) rather than in the main text:

“To find the “real” interest for a given period, take the percentage by which the dollar has increased and add to it the annual interest, raised by said percentage. For 1929 to 1932 the dollar increased by 53 per cent and to the third week of June 1932, by 62 per cent.” (1932, p. 211 [271])

In short, though the Fisher effect played no positive role in discussions of the behavior of the economy during the early years of the Great Depression, the Fisher distinction in a

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<sup>5</sup> Of course the effect in question is anything but asymmetric if debtors are unable to pay off their real losses, and hence creditors to collect their gains, and this is surely what Fisher had in mind..

straightforward backward looking application did, at least in Fisher's own account of matters, and precisely because his work had convinced him of the minor empirical irrelevance of the effect itself. As we shall see in due course, the Fisher effect's role in discussions at the time of the Great Recession that began in 2007 has been very different, but to understand how this change came about, we need to say something about its history in the intervening years.

### *The Fisher relation between the crises*

Keynes, who had already expressed doubts about the Fisher effect's importance in (1923), set the dominant tone for much of its subsequent treatment during the years of the so-called Keynesian revolution in the *General Theory* (1936), albeit advancing arguments that were not quite so far removed from Fisher's own views on these matters as summarized in (1930), as he was inclined to claim.<sup>6</sup> True, Keynes denied any direct effect at all of expected inflation on nominal interest rates where Fisher had allowed for the possibility of a small one – “The mistake lies in supposing that it is the rate of interest on which prospective changes in the value of money will directly react” (1936, p. 142) - but he was much closer to Fisher, though perhaps less definite, in accepting the possibility of an indirect causative channel, running through the influence of rising prices on the nominal value of what he called the marginal efficiency of capital, a concept, incidentally, that he himself agreed was identical to Fisher's own (1930) “rate of return over cost” - see (1936, p. 140) - though Fisher, of course, habitually discussed the real variant of this variable.

“The stimulating effect of the expectation of higher prices is due, not to its raising the rate of interest (that would be a paradoxical way of stimulating output – in so far as the rate of interest rises, the stimulating effect is to that extent offset), but to its raising the marginal efficiency of a given stock of capital. *If the rate of interest were to rise *pari passu* with the marginal efficiency of capital, there would be no stimulating effect from the expectations of rising prices. For the stimulus to output depends on the marginal efficiency of a given stock of capital rising *relatively* to the rate of interest”* (1936, pp. 142-143, italics in original)

Keynes' disciple Sir Roy Harrod, one of the two authorities – the other being Sir John Hicks – who in 1959 informed the quintessentially “Keynesian” Radcliffe Committee that the long run equilibrium value of the long rate of interest was three per cent, was sticking firmly to this line as late as (1971, pp. 179-80) and indeed took it further, denying not just the empirical but the very logical possibility of expected inflation affecting the yield on bonds, on the ground that, like money itself, these were nominal assets. And though Hicks did not follow Harrod into the particular analytic error of ignoring the margin between nominal and real assets when dealing with this issue, he still wrote as follows as late as 1989<sup>7</sup>.

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<sup>6</sup> And which lay directly in the Cambridge tradition of discussions of the effect of inflation on investment documented by Bridel (1987)

<sup>7</sup> It is nevertheless an unexplained puzzle in the Keynesian literature that the *Radcliffe Report* itself (Committee on the Working of the Monetary System 1959) of all documents, provided an exception to the general tenor of British Keynesian discussions of these matters, by suggesting (p. 211) that sustained inflation in the two per cent range might create expectations that would raise the above-mentioned normal value of the long-run rate of interest from 3 to 5 percent. The *Report* made nothing further of this point however.

"it is commonly thought that these high rates of interest [since 1950] are a consequence of inflation: that if prices are rising at 4 per cent per annum, a nominal rate of interest of 8 per cent per annum is equivalent to a *real* rate of 4. It is true that inflation makes these high rates of interest bearable, so that their consequences are not so desperate as they would have been in the past. But to make these consequences into causes surely takes things the wrong way round." (1989, p.79)

Even so, this quotation tells us quite clearly, and accurately too, that by the 1980s, the Fisher effect was back in play and "commonly thought" to be true; and it also hints, again surely accurately, that this was in part the result of the so-called Great Inflation of that time and, by inference, of what Harry Johnson (1971) would call the Monetarist counter-revolution whose success this episode helped to promote. But the idea's revival had nevertheless not been quite straightforward.

To be sure, the Fisher effect was explicitly expressed in eq. (9) (p. 9) of that counter-revolution's opening manifesto, Friedman's (1956) "The Quantity Theory of Money, a Restatement", but with no citation of Fisher himself, and accompanied by the warning that "differences of opinion [about expected inflation] cannot be neglected, so we cannot suppose (9) to hold; indeed, one of the most consistent features of inflation seems to be that it does not" (pp. 9-10).<sup>8</sup> What Friedman had to say about the Fisher effect in this seminal essay thus implied doubts about its empirical relevance not so far removed from those expressed, in the 1920s, not least by Keynes (1923) as we have already noted, and of by course Fisher himself.

Twelve years later, in his famous AEA Presidential address (1968), Friedman's treatment of both the Fisher distinction and the Fisher effect was less hesitant. He cited their role in the theory of interest rates as the inspiration for his introduction of inflation expectations into the Phillips curve and he expressed no doubts about the effect's empirical significance or policy-relevance when embedded in it. The main burden of Friedman's address was to promulgate the "natural unemployment rate" hypothesis, whose validity required full adjustment of wage-inflation to expectations about price inflation, but when it came to the parallel case of nominal interest rates, he rested content to point out that experience showed the adjustment of nominal interest rates to inflation to be "slow to develop and slow to disappear" (p. 101) and left the explicit question of its completeness unexamined. He was more definite in "The optimum quantity of money" (1969) however, as of course he had to be in a primarily theoretical study dealing with the monetary economics of fully anticipated inflation, but he still stressed that "it takes a long time for people fully to adjust their anticipations to experience" (1969, p. 41)

Empirical work on the expected inflation rate as a (negative) own rate of return on money, largely set in motion by Philip Cagan's paper in the (1956) Friedman volume, was already well developed by the late 1960s, but explicit studies of its role in determining nominal interest rates were still rare. Only two papers from the 1960s directly dealing with US evidence on this matter are cited by Laidler and Parkin in their

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<sup>8</sup> That Friedman took this warning seriously is confirmed by the fact that the nominal rate of return on bonds and the expected rate of inflation figured as separate variables in that essay's equation (11) which restated the quantity theory as a theory of the demand for money.



then comprehensive and widely read “Inflation – a Survey” (1975): Gibson (1960) and Yohe and Karnowsky (1969) – see (Laidler and Parkin, 1975 p. 216). Still, these, along with the slightly later work of Feldstein and Chamberlain (1973), not only seemed to confirm the slowness of nominal rates to adjust to inflation, but also provided evidence that, albeit over periods that could be as long as one to three decades, it was essentially complete. Friedman’s brief (1969) comments on this matter thus had an up-to-date empirical basis.

The most important development of the 1960s, however, as far as the Fisher effect was concerned, was the growing acceptance that emerged at this time, mainly in theoretical work, that it should be treated as complete in the long run, and that decisions on the margin between real and nominal assets therefore should be modeled as free of money illusion. Thus, Martin Bailey (1962) and Robert Mundell (1963) introduced the effect into the then ubiquitous IS-LM model, an essential step if the latter was to be deployed in analyzing inflation – though it never really was with much success - but it would soon find a more comfortable home in the literature on the long run equilibrium characteristics of a monetary economy, to which Friedman’s already cited (1969) paper was a rather late but nevertheless distinguished contribution.

This is not the place to digress into what this literature did or did not reveal about the possibilities for variations in a fully anticipated inflation rate to have long run effects on real variables in general, and economic welfare in particular. These were many and various, and nowadays largely forgotten - interested readers are referred to Section 2 of Laidler and Parkin (1975) for further information - but one approach to this issue, pioneered by Miguel Sidrauski (1967) would have a pervasive influence, long after his untimely death in 1968, and is important to our story.

What made Sidrauski’s paper seminal was his modeling of asset accumulation as the outcome of explicitly analyzed forward-looking utility maximization rather than of the application of rules of thumb about propensities to save and the like. His deployment of a constant rate of time preference in formulating these decisions pinned down the real rate of interest to a constant as well, and ensured not only that the behavior of real variables was independent of the inflation rate – a result that contradicted much contemporary wisdom, and which he emphasized – but also that the Fisher effect worked entirely on the nominal interest rate.

Obviously, from the viewpoint of analytic rigour, Sidrauski’s model represented an enormous step forward.<sup>9</sup> Nevertheless, widely read though it immediately was, this paper appeared at a time when empirical evidence still trumped formal analysis when it came to getting ideas into general circulation, and its full impact had to await a change in economists’ priorities in this regard. Thus, in 1975, Laidler and Parkin noted it (p. 178, fn.) but nevertheless expressed skepticism (p. 200) about there being any empirical basis for assuming that the real rate of interest was in fact invariant to monetary, or indeed any other, disturbances.<sup>10</sup> Their ambivalence here was symptomatic of the fact is that, although the Fisher distinction and effect had come to play an increasingly important role in macroeconomic analysis as inflation gathered speed in the 1960s and into the 70s, the

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<sup>9</sup> Friedman for one immediately recognized this, witness his moving memorial speech delivered at a 1968 American Bankers Association conference, and reprinted in Gordon (ed.) (1974), a volume dedicated to Sidrauski’s memory.

<sup>10</sup> Though this author recalls that the degree of skepticism expressed here was to some extent the result of Brian Reddaway’s editorial reactions to earlier drafts of the relevant section.

literature dealing with inflation was still in a state of considerable flux, even confusion, in 1975.

The very idea that inflation was “always and everywhere a monetary phenomenon” -- to borrow Friedman’s famous phrase – still generated heated controversy at this time, and so therefore did the question of how much the genesis of its then current manifestation owed to the neglect of earlier macroeconomic ideas - including the Fisher relation - in the wake of the Keynesian revolution. Furthermore, many of those who broadly accepted Friedman’s view, and like Laidler and Parkin recognized the important role played by inflation expectations to its theoretical foundations, were nevertheless far from comfortable with questions about just how those expectations were formed and fitted into a bigger picture. Nothing illustrates this better than that their own main discussion of the Fisher effect itself occurs when they deal with “The Redistributive Consequences of Inflation”, not when “The Quantity Theory and Perfectly Anticipated Inflation”, “Wage and Price Setting Behaviour and Expectations” or even “Complete’ Short-run Models of Inflation, Output and Employment” are their topic. Furthermore, this discussion itself ends on a tentative note, by pointing out that the full adjustment period of “one to three decades” implied by empirical results on the Fisher effect

“would appear to be inconsistent with the evidence [of much faster adjustment] generated in Phillips curve type studies . . . However the relevant period over which expectations must be formed when setting interest rates on a long-term loan is much longer than that involved in striking a wage bargain. It is quite plausible that the same agents might adjust their expectations of inflation over the next twenty years at a much slower rate than their expectations of the next twelve months’ inflation” (pp. 217-8)

Such indecisiveness would, of course, soon and quickly disappear from mainstream macroeconomics when formal treatments of monetarist ideas about inflation evolved into new-classical models, as they were already beginning to do by 1975, and analytic rigour came to be valued above all other virtues among the sub-discipline’s practitioners. It was very difficult to write down an explicit model of an economy in which money-wage and price inflation rates responded fully and rapidly to inflation expectations but nominal interest rates did not, particularly when the idea of rational model-consistent expectations had become the foundation for modeling their formation and, crucially, also their simultaneous deployment across all markets.<sup>11</sup>

By the early 1980s, therefore, empirical uncertainty notwithstanding, the Fisher effect more or less as set out in equation 1, had become a routine and uncontroversial feature not only of long run theoretical analysis of money’s super-neutrality that Sidrauski had inspired, which by then had faded into the intellectual background, but also of short-run models of money, inflation and the cycle, which were nevertheless based like his on explicit forward looking maximization. And in due course this effect would become equally well established in both the theory and practice of monetary policy.<sup>12</sup>

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<sup>11</sup> Though as Michael Parkin has pointed out to me, these developments paid little if any attention to questions about the term structure of inflation expectations that we had tentatively raised in (1975), and as he has also stressed, this inattention was not obviously an advance.

<sup>12</sup> The key papers of what is usually called the “Rational Expectations Revolution” are conveniently gathered in a single edited volume (Lucas and Sargent (eds.) (1981) whose rather austere title does not do

### *The Fisher relation and the Great Recession*

The Monetarism of the 1960s and early 70s, and the new-classical economics that followed it, made major and long lasting changes to macroeconomic theory and policy. Its interpretation of the essentially monetary nature of inflation, its claims about the likely futility of trying to engender improved real economic performance by pursuing a rising price level, and hence the desirability of making low and stable inflation the central goal of monetary policy, became widely accepted, and remain so today. However, new-classical economics' so-called "monetary surprise" model of the cycle, which embodied the monetarist hypothesis about the prime importance of variations in the growth rate of the money supply in driving short-run macroeconomic behavior, did not survive its encounters with empirical evidence – see, e.g. Robert J. Barro (1979) - while monetarism's preferred means of pursuing medium term price stability - money growth targeting – would also prove fragile when actual policy was based upon it: one such scheme arguably served as a useful cover for the Volcker disinflation of the early 1980s, but the approach was otherwise soon judged impractical in the U.S., and similar regimes suffered the same fate in many other places too.

What then emerged on the policy front under these twin impulses, aided and abetted by Milton Friedman's spectacularly wrong forecast in 1983-4 that the rapid money growth then in progress would soon lead to a resurgence of inflation (See e.g. Friedman 1984), was a sort of "Monetarism without money".<sup>13</sup> The micro-foundations of this new approach were grounded in real business cycle theory, itself drawn from the same tradition in neo-classical growth theory to which Sidrauski (1967) had been a seminal contribution, and on the policy front, it retained earlier emphasis on low and stable inflation as a goal and continued to stress the importance of inflation-expectations in models of how to attain it. Crucially, however, it replaced money growth with a nominal interest rate as the central bank's key instrument. Formal models of monetary policy with these features, whose classic exposition is that of Woodford (2003) where their descent from Sidrauski's work is explicitly acknowledged, were well entrenched in the research departments of central banks by the turn of the millennium, and provided a background to the formal inflation targeting regimes that many of them had by then adopted. Much of the academic debate about monetary policy in the run up the crisis that began in 2007 was also cast in such terms.

Only the briefest reminder of these models' generic form is needed here. They have three components: an expectations augmented Phillips curve, which determines inflation as a deviation from expected inflation that is positively related to an "output gap", the deviation of aggregate output from some long-run "normal" level; a misnamed – here we shall let this pass - IS curve, whereby this output gap is inversely related to the real rate of interest, which in turn has a "normal" "neutral" or "natural" value at which the

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full credit to its wide range. But it was not just New-classical economists who took to embedding versions of equation 1 in their models, and uncritically assuming a constant long run equilibrium real rate of interest. The current author must also confess to having done so in a number of places. See for example, Laidler (1988, eq. 13, p. 88)

<sup>13</sup> Friedman's forecast was in error largely because he overlooked the effect that rapidly falling inflation, expectations, were having on the demand for money. Ironically, the mechanism at work here had been much stressed in his earlier writings, (e.g. 1969), albeit in the context of rising, rather than falling, inflation..

output gap is zero, unemployment is at its natural rate, and inflation is therefore constant at its expected rate; and a policy reaction function, these days typically an explicit “Taylor rule”, according to which the central bank sets the *nominal* interest rate, and hence, through a Fisher effect, a *real* rate, so as to influence the output gap and keep inflation on some predetermined low and steady time path.

In strong contrast to its marginal place in the conventional monetary policy wisdom of the late 1920s, the Fisher relation, and more specifically the Fisher effect, was thus completely central to "Monetarism without money", and in equally strong contrast to its virtual absence – Fisher’s work excepted - from the debates that marked the Great Depression, it remained very much present even after 2007. Specifically, the relation was fundamental to discussions, set in motion by Lawrence Summers (1991), of the so-called *zero lower bound problem* posed by the possibility that, with a “low” inflation target in place, the economy might encounter some shock in response to which a mechanical application of the Taylor rule would prescribe an impossible to achieve negative value for the nominal interest rate. And when, after 2007, this possibility became a reality, the Fisher effect also and necessarily figured prominently in arguments about the extent to which additional "unorthodox" measures might, or might not, be needed to supplement what by then had become "orthodox" interest rate based monetary policy. It also played a key part in the renewal of earlier rather low-keyed debates about whether price level time path targeting (and more recently, nominal GDP growth targeting) might not in general be a superior alternative to inflation targeting in any event. The argument here is that when, in a recession, inflation falls and output slows down, these regimes, if credible, automatically give rise to an increase in the expected inflation rate, and hence lower the real rate of interest, even if the nominal rate is at its minimum, in a way that a simple inflation targeting regime in which by-gones are by-gones does not. Clearly the question of the policy regime’s credibility, and the capacity of agents to penetrate its complexity so as to form accurate expectations, is crucial to Fisher effect based arguments along such lines<sup>14</sup>

The Fisher effect was also in recent years an important component of the case that the Fed's policy rate had been set “too low” in the years before 2007, and therefore contributed to - in some versions, notably that of John Taylor (2011) caused – the crisis that began in that year. There is a parallel of sorts between these criticisms of Fed policy before 2007 and some that were advanced with respect to its policies in the run-up to the Great Depression. In each case it was argued that interest rates were kept too low for too long and fuelled speculative bubbles – mainly in the housing market before 2007 and mainly in the stock market before 1929 – whose collapse ushered in serious and cumulative dislocations of the financial system. The reasons given for these mistakes however, if mistakes indeed they were, are very different. In the late 1920s the Fed's alleged error was put down to its desire to help other countries, and particularly the UK, to cope with the stresses of returning to the gold standard at parities that in some cases were, to say the least, optimistic. More recently, the charge has been that the Taylor rule was ignored in the interests of sustaining the economy's recovery from the collapse of the preceding dot-com bubble.

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<sup>14</sup>. The relative merits of inflation and price-level targeting are discussed along these lines by Parkin (2009), while Scott Sumner (2012) makes the current case for nominal GDP targeting.

In the late 1920s, Fed policy unambiguously did not lead to any general upsurge in inflation, so the Fisher relation is irrelevant to judging its appropriateness, even with benefit of eight decades of hindsight. In the mid 2000s, however, judgments about whether or not the Taylor rule, to whose deployment the Fisher relation is central, was violated hinge critically on how policy interest rates moved relative to inflation, and seem to depend, in part at least, upon how inflation is measured. To be more specific, between 2002 and 2008 CPI inflation in the US rose from a little under 2 per cent to over 4 percent, and as Taylor's work (e.g. 2011) has clearly shown, a policy model using this index would have called for a higher interest rate long before 2006, but over the same period, the Fed's preferred measure, core PCE inflation, remained firmly in a "comfort zone" around 2 per cent per annum, and on this criterion, policy was about right.<sup>15</sup> The moral here is surely that when an important debate about the appropriate conduct of monetary policy hinges upon just which measure of the inflation rate agents are thought to form their expectations about, then the Fisher relation surely has become deeply embedded in the theory of monetary policy.

Be that as it may, the Taylor rule in general, and recent debates in particular about how it should have been applied, make most sense when cast in terms of a model in which private sector agents form their inflation expectations by observing, among other variables, the actual inflation rate, and then use this information, along with their understanding of the economy's structure and of the monetary policy regime, in making spending decisions. This idea underlies the requirement that, in implementing the rule, the monetary authorities should always raise (lower) their policy rate by more than any observed change in the rate of inflation, in order to ensure that their response results in a stabilizing increase (fall) in the real rate of interest that figures in the IS curve.

But the same class of model can yield very different policy implications if a more radically rational view of the formation of inflation expectations is embedded in the system alongside the assumption that the central bank is a completely credible inflation targeter so that policy announcements are in and of themselves sufficient to move expectations. In such systems, provided also that long run equilibrium value of the real rate of interest is uniquely determined independently of monetary policy by the fundamentals of productivity and thrift and is constant, or at least very slow to move over time, and provided that markets work so as to keep the decisions of individual agents fully coordinated, – not assumptions that I would be any more willing to take for granted now than when contributing to Laidler and Parkin (1975) – the way for the central bank to lower (raise) the inflation rate is to lower (raise) the nominal interest rate.

This is because, in such a world, interest rate policy is interpreted by rational agents as embodying credible announcements on the central bank's part of its intentions concerning the future inflation rate, and the expectation that they engender then play a dominant role in the evolution of the equilibrium time path, and hence also the actual time path, of inflation. Given the premises, the conclusion follows. As Minneapolis Fed

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<sup>15</sup>This is not a debate to be settled here, but permit me the luxury of quoting my own pre-crisis (early 2006) view of this matter: "Local experts know more about how strong a case can be made for the existence of a housing market bubble, and about how much of it can be attributed to monetary policy as opposed to other features of the US scene . . . but even so, perhaps a Fed constrained by, say, a 2 per cent inflation target for the CPI (as opposed to some measure of core inflation) would not have responded to the "dot com" collapse quite so vigorously and for quite so long, while still managing to maintain financial stability in its wake" Laidler (Feb. 2006)

President Narayan Kocherlakota was a little while ago quoted as saying, "Most of our monetary models tell us that, if the Fed maintains a constant nominal interest rate for ever, that will essentially determine the inflation rate, by way of the Fisher relation" <sup>16</sup>

More specifically relevant to recent circumstances, these premises also imply that an economy can escape from stagnation accompanied by an essentially zero level of short term nominal interest rates such as the US experienced after 2008, and the zero lower bound policy problems that go along with it, by having the central bank pre-announce a strategy of *raising* those rates. Stephanie Schmitt-Grohe and Martin Uribe (2010) have put the point as follows:

"Perhaps the most problematic aspect of the analyzed exit strategy in regard to credibility is the need to communicate to the public that the increase in nominal interest rates is intended to raise inflationary expectations . . . . We believe that after observing falling inflation with near zero interest rates for a sufficiently large number of quarters the public will come to intuitively internalize the notion that the Fisher effect has become dominant and accept the monetary authority's argument of raising interest rates to fight deflationary pressures" Schmitt-Grohe and Uribe (2010)

In the light of such analysis, whose logical coherence is not in question, but whose empirical relevance depends critically on the perhaps implausible assumption that the economy always remains exactly on its equilibrium time path, as Peter Howitt (1992) demonstrated some time ago, it is hard to avoid the conclusion that, although the absence of the Fisher effect and distinction from policy discussions during the Great Depression rendered these unnecessarily opaque to modern readers, the Fisher effect's central role in the debate about the Great Recession has in recent years sometimes led to a little too much clarity for comfort.<sup>17</sup>

### *Summing-up the Story*

Over the century or so that our story of the Fisher relation's changing place in macroeconomics has spanned, the behavior of the economy and of those making policy for it has changed quite radically, partly in response to changes in economic ideas. These ideas have in turn changed, again quite radically, in response to events, some of them policy induced. The evolution of the Fisher effect's, and even of the simple Fisher distinctions, from something close to fringe ideas, through almost total neglect, into a core component of mainstream macroeconomics has been part of this broader process.

To be more specific: we have seen that the Fisher effect's position in the sub-discipline, already precarious in the 1920s for want of empirical support except in extreme inflationary conditions in remote times and places, was further weakened by Keynesian revolution that the Great Depression helped bring about; that its revival came with the onset of serious inflation in major economies in 1960s and '70s; and that this

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<sup>16</sup> This quotation was attributed to Kocherlakota by Steven Williamson, and caused Nicholas Rowe (2010) to "lose it" - in Paul Krugman's (2010) well-chosen phrase. See Williamson, as quoted by Rowe, as quoted by Paul Krugman Aug 25<sup>th</sup> 2010

<sup>17</sup> Specifically, Howitt showed that the equilibrium time paths along which today's interest rate settings determine the future time path of inflation are unstable, in the sense that any disturbance to them, including of course an unanticipated change in the interest rate itself, would set up dynamics that take the economy away from, rather than back towards, its equilibrium path..

revival gathered further momentum from the emergence of an approach to macroeconomic theory that stressed logically rigorous and consistent analysis of forward looking maximization as something to be valued above anything else. In today's intellectual environment, then, the Fisher relation, not only as a distinction but more particularly as an effect to which inflation expectations are central, cannot help but be at centre-stage.

At first sight this looks very much like a Whiggish story of permanent scientific progress, but some of the recent deployments of the Fisher relation discussed at the end of the preceding section of this paper might just lead to over-ambitious policy developments that could at some future date turn our narrative into a cautionary tale of how particular economic ideas can after all come and go over time. Even the possibility that a few prominent central banks might adopt formal nominal GDP targeting, only to find that this scheme's purported advantages are too reliant for comfort on agents' sophistication in forming inflation expectations, could lead to such an outcome. Time will tell.

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