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### Beyond mechanical markets – asset price swings, risk and the role of the state

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## BOOK REVIEWS

**Beyond mechanical markets – asset price swings, risk and the role of the state**, by Roman Frydman and Michael D. Goldberg, Princeton, NJ, Princeton University Press, 2011, 304 pp., \$35.00 (cloth), ISBN 978-0-691-14577-8\$35.00 (e-Book), ISBN 978-1-400-83818-9

In their provocative and fascinating new book, Roman Frydman and Michael Goldberg attribute some measure of the blame for the financial crisis that started in 2008 and for the inadequacies of subsequent policy to the poor state of monetary and financial theory. They accuse the practitioners of rational-expectations macroeconomics and efficient-markets finance of populating their models with robots rather than people and of building ‘castles in the air.’ The charge reminds me of a mnemonic that a psychiatrist friend once told me: a *neurotic* builds castles in the air; a *psychotic* lives in them. That more or less summarizes what Frydman and Goldberg find wrong with mainstream macroeconomics and finance: too many economists live in their models and mistake them for reality itself. Such criticism can logically be made only from a standpoint that is better grounded in reality. Frydman and Goldberg find their ground in a new approach to economic theory that they call *imperfect knowledge economics* (or *IKE*). IKE was developed most fully in their earlier book on exchange rates, *Imperfect Knowledge Economics: Exchange Rates and Risk* (Frydman & Goldberg, 2007), and in empirical applications jointly with Katarina Juselius and Søren Johansen (Johansen, Juselius, Frydman, & Goldberg, 2010). The earlier expositions of IKE were formidably difficult. So much of *Beyond Mechanical Markets* is taken up with the failings of mainstream theory that Frydman and Goldberg miss the opportunity to provide a more accessible exposition of IKE itself. To some degree, I hope to be able to use this review to recover that opportunity and provide an Econ 1 (*Principles*) version of IKE. Still, I wish that Frydman and Goldberg had themselves provided an Econ 101 (*Intermediate*) version for those who find their treatment of IKE in the *Imperfect Knowledge Economics* too daunting. I hope that they will do so in a future book.

### I

IKE can be seen as starting with Keynes’s (1921) distinction between insurable and uninsurable risk or Knight’s (1921) functionally equivalent distinction between *risk* and *uncertainty*. Lucas acknowledges the distinction, but argues that ‘[i]n cases of uncertainty, economics reasoning will be of no value’ (1977, p. 15). Frydman and Goldberg disrespectfully disagree. Their major thesis might be stated: *there is something theoretically important to be said about uncertainty after all*. And IKE can be seen as the consequence of taking uncertainty seriously.

Frydman and Goldberg do not identify which of the various interpretations of probability they support, but it is fair to say that they are not Bayesians; for, like Keynes, they hold that there are some events for which it is not possible to provide secure attributions of probability and that probability distributions in economics, while they may be stable for some longer or shorter periods, are subject to abrupt and discontinuous change. As a result, one of the key requirements for straightforward rational economic calculation (e.g., computing the present value of a long-lived investment) is often missing.

They give a nice Keynesian illustration with the case of a fisherman who may well have a good (presumably implicit) probability model of his daily catch, but who is at sea when it comes to assessing the probable value of catches far into the future. It is this long-term assessment that is needed to calculate the present value of a new boat that should guide the fisherman's investment decision.

Keynes writes with respect to the same problem faced by Frydman and Goldberg's fisherman:

In practice we have tacitly agreed, as a rule, to fall back on what is, in truth, a *convention*. The essence of this convention – though it does not, of course, work out quite so simply – lies in assuming that the existing state of affairs will continue indefinitely, except in so far as we have specific reasons to expect a change. This does not mean that we really believe that the existing state of affairs will continue indefinitely. We know from extensive experience that this is most unlikely. The actual results of an investment over a long term of years very seldom agree with the initial expectation. Nor can we rationalise our behaviour by arguing that to a man in a state of ignorance errors in either direction are equally probable, so that there remains a mean actuarial expectation based on equi-probabilities. For it can easily be shown that the assumption of arithmetically equal probabilities based on a state of ignorance leads to absurdities. We are assuming, in effect, that the existing market valuation, however arrived at, is uniquely *correct* in relation to our existing knowledge of the facts which will influence the yield of the investment, and that it will only change in proportion to changes in this knowledge; though, philosophically speaking it cannot be uniquely correct, since our existing knowledge does not provide a sufficient basis for a calculated mathematical expectation. In point of fact, all sorts of considerations enter into the market valuation which are in no way relevant to the prospective yield.

Nevertheless the above conventional method of calculation will be compatible with a considerable measure of continuity and stability in our affairs, *so long as we can rely on the maintenance of the convention*. (Keynes, 1936, p. 152)

Keynes believes that such conventions are both, descriptively, how we in fact manage uncertainty and, normatively, how we ought to manage it.

For Keynes, rationality is not defined by the ability to solve an optimization problem as an omniscient being would (i.e., omniscient up to a well-defined random error). Nor are we irrational when we adopt *reasonable* expedients for confronting our inevitable cognitive and epistemic limitations. Indeed, rationality *demand*s that we adopt such expedients. In a passage cited by Frydman and Goldberg, Keynes writes:

We should not conclude from this that everything depends on waves of irrational psychology. On the contrary, the state of long-term expectation is often steady, and, even when it is not, the other factors exert their compensating effects. We are merely reminding ourselves that human decisions affecting the future, whether personal or political or economic, cannot depend on strict mathematical expectation, since the basis for making such calculations does not exist; and that it is our innate urge to activity which makes the wheels go round, our rational selves choosing between the alternatives as best we are able, calculating where we can, but often falling back for our motive on whim or sentiment or chance. (Keynes, 1936, p. 162)

Although it is perfectly rational to confront uncertainty with a convention, there is no reason in Keynes's view to presume that such conventions are unique or that we should all adopt the same one. Rather, our choice of a reasonable convention must be conditioned on the fact that reasonable responses differ among people and that they may, in the event, prove to be wildly and asymmetrically wrong. Thus, while economists have often tried to capture risk by a symmetrical quadratic loss function, there is in Keynes's view no reason at all to think that we have a reliable estimate of the mean of, say, future GDP or that we regard positive and negative variations of GDP as indifferently as bads. Most of us would

be quite happy to have underestimated our future income and deeply unhappy to have overestimated it.

The history of the analysis of liquidity preference (i.e., the demand for money) provides an example (not cited by Frydman and Goldberg) of the difference that Keynes's treatment of uncertainty can make to economic theory. Keynes noted that the decision to hold money (in which he included short-term, interest bearing bonds) was the foregone yield on longer-term bonds. The holding-period yield of a bond depends in part on the change in market interest rates – that is, on an uncertain fact about the future. Keynes believed that each investor takes some view about the value of the market rate of interest that should be regarded as 'normal' and that these judgments are not unique. Those investors who see current market rates as high relative to normal should hold bonds as they expect capital gains on bonds as market rates fall in the future; while, those investors who view rates as low relative to normal should hold money as it provides a safe haven from capital losses on bonds as market rates rise in future. The actual market rate of interest is that rate that divides the market in such a way that the bulls hold all the outstanding bonds and the bears all the outstanding money. A continuous diversity of opinion about the normal rate implies a smooth aggregate relationship between the level of interest rates and liquidity preference, and allows the central bank to adjust the interest rate smoothly by changing the stock of money.

Mainstream economics soon rejected Keynes's analysis of liquidity preference. Tobin (1958) noted that the Keynesian investor is a plunger – all in bonds or all in money – but actual investors diversify. In a move that is emblematic of the theory that Frydman and Goldberg criticize, Tobin presented an alternative model of liquidity preference featuring a tradeoff between money (zero capital risk, zero return) and bonds (positive capital risk, positive return). Tobin measured risk as the *symmetric* fluctuations of returns around their mean. Each individual could choose from a menu of risk/return combinations supported by different mixtures of money and bonds. Tobin's model is the two-asset version of the capital asset pricing model that became the foundational model of modern finance.

Contrary to the conventional folklore, Tobin had not advanced Keynes's analysis; he had changed the subject. His analysis applied to a single individual and could be easily aggregated only if all individuals were alike, so that the one individual was *representative* – a move that in other contexts draws heavy fire from Frydman and Goldberg. Although Keynes's investors were in fact plungers, there is nothing in the essence of his analysis that would prevent him from grafting Tobin's portfolio diversification onto his own account of liquidity preference. It does not change anything fundamental. Diversified investors would still face uncertainty and would have to take a conventional view about the normal mean and normal variance of returns. It is no surprise that Tobin's paper was titled 'Liquidity Preference as Behavior Towards Risk,' since it simply ignored uncertainty and the possibility of abrupt, discontinuous shifts of the whole probability distribution of returns. It is precisely the implicit assumption that the probability distribution of returns is stable over time that makes much of the modern theory of finance irrelevant in Frydman and Goldberg's view.

The diversity of conventional, but nonetheless rational, estimates of the normal rate of interest is a fundamental response to uncertainty in financial markets. So far, so Keynes. Yet, Keynes did not give any further account of the formation of the normal rate. Frydman and Goldberg pick up where he left off. One can see their strategy as analogous to the introduction of stochastic models into many areas of science. The motivation for stochastic models was that many processes – even if they were metaphysically deterministic – were too complex to model in detail. We could not practically trace all paths and collisions of

trillions and trillions of gas molecules. But we could give up on providing such a detailed account and instead concentrate on global characteristics in which the momenta of molecules were regarded as randomly distributed with well-defined probability distributions.

Frydman and Goldberg offer a solution of the same genus. They see investors as adopting various strategies for forming expectations of future prices. These strategies are not unique, so that there is a distribution of strategies and investors may alter their strategies from time to time. Where Keynes had been content to assert a distribution of normal rates, Frydman and Goldberg want to think about how investors decide on the path of normal rates over time. The strategies of forming expectations are, then, the dynamic equivalent of Keynes's normal rates. Frydman and Goldberg regard modeling each underlying strategy as a hopeless enterprise and the assumption that they can be captured by the strategy of a representative agent as simply false. Giving up on the hopeless, they instead ask, how would the economy behave if there were a distribution across agents of reasonable strategies subject to change over time? In the current volume, this is all painted with a broad brush, though it is set out in considerable detail in *Imperfect Knowledge Economics*.

Like Keynes, Frydman and Goldberg emphasize the heterogeneity of the strategies of expectation formation while denying that it arises from irrationality or the dominance of psychological factors. Rather, heterogeneity and instability arise from uncertainty. Rational responses are not univocal: there are many reasonable guides to an uncertain future. Frydman and Goldberg offer empirical evidence – largely drawn from published commentaries on the movements of stock prices – to show that 'fundamentals' are the principal drivers of asset prices. This is a striking claim that is a thrust at the heart of behavioral finance and other claims of market irrationality.

One implication, they contend, of the heterogeneity of the strategies for expectation formation is that price movements – though they generally move in the same direction as the fundamentals – typically rise or fall faster than they would if the actual, *ex post* movements of the fundamentals had been known precisely *ex ante*. The empirical evidence and the argument for overshooting are suggestive, but, again, the details of IKE are not laid out in the volume, so the best that can be said is that they have made a plausible case.

Even the notion of the *fundamentals*, though commonplace, is never clearly defined. Roughly, it means any factor that is related to future profit streams. Measures of the fundamentals such as price/earnings ratios show over very long periods a tendency to long swings about relatively stable average values. Frydman and Goldberg contend that, far from asset prices irrationally moving against the fundamentals, they almost always move in the same direction, but they are often far away from these very long average values or *benchmarks*. When fundamentals change direction, the tendency is for rapid adjustment of prices in the same direction toward the benchmarks. Frydman and Goldberg provide suggestive empirical evidence for such nonlinear relationships between the movements of asset prices and their distance from various long-term benchmarks.

## II

The run-up to the financial collapse of 2008–2009 is widely regarded – at least in retrospect – as a bubble, where 'bubble' is glossed variously as a psychological phenomenon similar to mass hysteria ('the madness of crowds' to use McKay's famous phrase) or as pure irrationality or, to the stalwarts of the rational-expectations hypothesis, a rationally self-fulfilling deviation from fundamentals. Each of these accounts of the rise in asset prices can be thought of as *imperfektionist*, but for some imperfection in the

economic system, the bubble would not have occurred. Cure the imperfection; cure the bubble.

In contrast, Frydman and Goldberg offer a *perfectionist* account of asset prices. The observed long swings around benchmark values are not the result of remediable failures in the market but are the aggregate consequences of rational responses to irremediable uncertainty. The structure of their argument echoes Keynes's view that unemployment is not the result of a deviation from 'perfect' markets, but is, in fact, the way in which markets behave when working perfectly. Like Keynes, Frydman and Goldberg do not take their view as ruling out interventions. Keynes was not, of course, denying that psychology, irrationality and market imperfections might play some role in unemployment, just that, even if they did not, the problem of unemployment would still be with us. Similarly, Frydman and Goldberg do not deny that psychology, error, irrationality and fraud may affect asset prices, just that, even if they did not, prices would still show long swings and abrupt readjustment to benchmark values. The crisis exposed the Bernie Madoffs of the financial system; the Bernie Madoffs did not cause the crisis.

Frydman and Goldberg go further: long swings in assets are not just unavoidable, they are in some sense desirable. They are partisans for the market as the best system of resource and capital allocation known to man. Tight regulation could prevent asset swings, but only at the too high price of diminished allocative efficiency. It is no surprise in their view that the economies with the most enduring real success are also the ones with developed and relatively free financial markets. Frydman and Goldberg are declared enemies of central planning, making telling references to the madness of the Soviet economic ideal. In this, they build on the insights of Friedrich von Hayek and – surprisingly to some – of Keynes.

Against the *bien pensant* commentators of the Left, Frydman and Goldberg see financial players who trade only for short-term gains as vital to the success of the economy even though they magnify the fluctuations of asset prices. Their idea is that short-term traders guarantee a market for realizing financial assets and, thus, for connecting financial markets to the real economy. For example, if most long-term traders agree that asset prices are correctly priced, then it will be difficult for any of them to find a buyer (or a seller for that matter) among other long-term traders. But short-term traders play a different game. Their diversity of opinion and hope of a quick profit implies that, in most cases, some short-term trader is ready to buy or sell any asset at any time. Thus, short-term traders are not simply gamblers, who ought to be suppressed, but rather serve a vital role in allowing finance to promote real economic activity. For, though some long-term traders are also speculators who can sit on their portfolios when they see them to be priced correctly, others use their portfolios to build savings and need to liquidate them – even when correctly priced – when they are ready to use the funds for the purchase of actual goods or for making real investments. Short-term traders smooth that process. Some evidence for this view is provided by the deleterious real effects of the seizing-up of commercial paper and other short-term asset markets in the financial collapse of 2008.

In our troubled economy, there are few such robust defenses of financial markets and their personnel. Yet, even Frydman and Goldberg believe that asset price swings can go too far – that is, that they can lead to the misallocation of real capital. They propose various devices to limit price swings, though not to eliminate them altogether – for example, capital requirements or adjustable margin requirements. These policy proposals are the least worked-out part of the book, as Frydman and Goldberg do not provide any analysis or any metrics on which to evaluate misallocation or to determine the boundary between virtuous and vicious interventions.

## III

While I admire Frydman and Goldberg's intellectual project and believe that it is an important contribution to economics, I do wish that they had focused more of their energy on an accessible exposition of IKE rather than on the battle with mainstream macroeconomics and finance. There is no doubt little hope of converting the founders of the new classical macroeconomics or efficient-markets finance to IKE. But many, younger economists are not yet intellectually dug in. Labeling the mainstream as 'Orwellian' is a tactical mistake, not calculated to win over the uncommitted. They would be more likely to be drawn in if IKE were presented as a natural continuation of the impulses that led to the rational-expectations hypothesis.

Frydman and Goldberg, to be sure, give a favorable and even-handed account of John Muth's motives in formulating the rational-expectations hypothesis. And while they are harder on Lucas, in fact even he is more aware of some of the limits of his modeling strategy than they acknowledge. In an assessment that would surprise many of his more aggressive acolytes, Lucas writes:

The problem is that the new theories, the theories embedded in general equilibrium dynamics of the sort that we know how to use pretty well now—there's a residue of things they don't let us think about. They don't let us think about the U.S. experience in the 1930s or about financial crises and their real consequences in Asia and Latin America. They don't let us think, I don't think, very well about Japan in the 1990s. We may be disillusioned with the Keynesian apparatus for thinking about these things, but it doesn't mean that this replacement apparatus can do it either. It can't. In terms of the theory that researchers are developing as a cumulative body of knowledge—no one has figured out how to take that theory to successful answers to the real effects of monetary instability. Some people just deny that there are real effects of monetary instability, but I think that is just a mistake. I don't think that argument can be sustained. I do think that most of the post-WorldWar II fluctuations of GDP about trend can be accounted for in real terms. I've estimated that would be something on the order of 80 percent. People can argue with that. But that's not because money doesn't matter. That's because monetary policy in the postwar United States has been so good. (Lucas 2004, p. 23)

Lucas certainly does not go as far as Frydman and Goldberg would wish; yet, by the castles-in-the-air standard such a stance relative to mainstream macroeconomic models is at worst 'neurosis,' not 'psychosis.'

Here is another example of potentially common ground. Frydman and Goldberg take the efficient-markets hypothesis as the claim that capital is always allocated optimally. This is the reading frequently given in the news media. But in my economic education, I was always taught a narrower interpretation: financial markets are *efficient* when it is impossible to outperform the market based on publicly available information. A market can be efficient in that sense and yet determine a set of asset prices that would hardly be regarded as efficient in the sense of promoting economically desirable outcomes. In fact, Frydman and Goldberg endorse this narrower doctrine, believing it to be a mistake to think that traders can readily pluck hundred-dollar bills from the sidewalks. The efficient-markets hypothesis, then, on some readings could be not a deep intellectual mistake but a starting point for IKE itself.

Frydman and Goldberg correctly bracket behavioral economics as equally unable as the rational-expectations and efficient-markets hypotheses to cope with true uncertainty. Behavioral economics, however, is not on all fours with the two mainstream economic doctrines. The fundamental insight of economics is that intentional actors adapt their plans to their constraints so that a reasonable explanation of what they *in fact do* is what they *ought to do* to advance those plans most effectively given the constraints. (Popper referred to the economists' explanatory trope as *situational analysis* and held that it was the only

reasonable basis for any social science.) The rational-expectations hypothesis, whatever its failings, is, as Frydman and Goldberg seem to acknowledge in their rather gentle handling of Muth, a legitimate attempt to apply the fundamental insight; many versions of behavioral economics (those that focus on irrationality, cognitive failure, error and mechanical rules of thumb) are not.

Frydman and Goldberg are correct that the rational-expectations and efficient-markets hypotheses are not adequate in the face of uncertainty. Lucas and many in the mainstream do not disagree. If they stuck to their models under the great duress of the current crisis, it arose less from 'psychosis' than from the belief that economics had nothing useful to say about uncertainty. Lucas always stressed that the development of economics is limited by its toolkit. Frydman and Goldberg's achievement in the current book, as in their earlier work, is to enlarge the economist's toolkit and to show that there is something useful to be said about uncertainty after all.

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### Fear and loathing in the economics of science

**Science-mart: privatizing American science**, edited by Philip Mirowski, Cambridge, Harvard University Press, 2011, 464 pp., \$39.95 (Hardcover), ISBN 978-0-674-04646-7

No sympathy for the devil; keep that in mind. Buy the ticket, take the ride ... and if it occasionally gets a little heavier than what you had in mind, well ... maybe chalk it off to forced conscious expansion: Tune in, freak out, get beaten. Hunter S. Thompson, *Fear and Loathing in Las Vegas*, 1998 [1971].