The Exchange Rate in a Behavioral Finance Framework, by Paul De Grauwe and Marianna Grimaldi, looks outside the box for new insights on exchange-rate dynamics. The “box” is the familiar “rational-expectations–efficient-market” paradigm, as the authors refer to the longstanding set of mainstream models in international macroeconomics, most notably the monetary/portfolio balance model and the intertemporal optimizing model of Obstfeld and Rogoff (1995). The authors examine, instead, a chartist-fundamentalist model based on the Frankel and Froot original (Frankel and Froot 1986). The model includes only speculators, who forecast in two ways: “chartists” extrapolate past changes and “fundamentalists” always expect a reversion to fundamentals.

The authors are never shy about their motivation: they title their first chapter “The Need for a New Paradigm” and they title their introduction to the model “The Broad Outlines of an Alternative Approach.” Mainstream models, they remind us, have little value in predicting exchange-rate dynamics and cannot explain a number of anomalies, including boom-bust cycles in exchange rates, fat tailed returns, volatility clustering, and the apparent disconnect between exchange rates and fundamentals. Their proposed solution is the chartist-fundamentalist model, which, as the book shows, can explain all these anomalies.

This review asks two questions. Question 1: Are DeGrauwe and Grimaldi correct that it’s time to look for a new exchange-rate paradigm? Since many economists would disagree, Kuhn is engaged as an impartial judge, and the review’s answer is: Yes. Question 2: Is the book likely to catalyze its intended paradigm shift? The review’s answer is a qualified: No. There are good scientific reasons to include the chartist-fundamentalist dichotomy in the next paradigm. However, the model itself seems unlikely to be widely accepted because it is internally inconsistent and the rest of its underlying structure is inconsistent with the evidence.
Is it time to look for a new exchange-rate paradigm? The most prominent authority on scientific revolutions, Thomas Kuhn, shows that they typically begin with the emergence of anomalies – such as those that have long puzzled exchange-rate economists (Kuhn 1970). As Kuhn recounts, the conflict between paradigm and evidence leads more and more researchers to become skeptical of the dominant paradigm and to challenge the remaining paradigm adherents. Paradigm skeptics are not hard to identify in exchange-rate economics. Beyond the authors of this book, the lengthy list of researchers critical of the prevailing paradigm includes such luminaries as Kouri (1983), Goodhart (1988), Frankel, Galli, and Giovannini (1996), Flood and Rose (1995), Taylor (Flood and Taylor 1996), and Lyons (2001). The failure of the old paradigm to account for anomalies generates increasing “anxiety” among researchers and a “crisis” ultimately ensues. To intensify the anxiety in exchange-rate economics, the dominant paradigm has never been blessed with strong supporting evidence, unlike the natural science paradigms discussed by Kuhn (Meese and Rogoff 1983, Lane 2001).

According to Kuhn, adherents and skeptics differ in their definition of rigorous scientific inquiry and, in consequence, they often talk past each other. Adherents view any research outside the paradigm as scientifically irresponsible, whereas skeptics are committed to exploring new scientific terrain because they see no point in studying what is, to them, a failed paradigm. These phenomena, too, can be identified within exchange-rate economics today. Today's skeptics typically cite the microfoundations of the dominant exchange-rate paradigm as its critical weakness – but this is the aspect of the paradigm its adherents admire most. The internal consistency of those microfoundations, the key source of pride among adherents, helps that paradigm avoid being ad hoc in an important dimension, one that plagued earlier exchange-rate paradigms. Skeptics likewise value internal consistency but consider those mi-
crofoundations to be ad hoc in a different dimension: they are at best tenuously connected to reality. As described below, skeptics find the dominant paradigm to be substantially counterfactual in its macroeconomic structure, its market microstructure, and in the way it structures expectations – which leaves little else. In the skeptics’ view, researchers could analyze infinitely many imaginary worlds, but the challenge of science is to explain the real world. In meeting that challenge there may be little value in a paradigm that is internally consistent yet profoundly inconsistent with reality.

Today’s exchange-rate skeptics began detailing their concerns in the 1980s, as evidence accumulated that the dominant paradigm’s assumptions of continuous purchasing power parity and uncovered interest parity utterly conflict with macroeconomic reality. In the 1990s, the skeptics’ focus broadened to include the absence of any correspondence between the dominant paradigm’s microstructure and the microeconomic reality of currency markets as observed at trading floors, asset management firms, and corporate treasuries. (This concern is illustrated in Goodhart (1988), and Frankel, Gali, and Giovannini (1996); market reality is described in Rime (2005) and Sager and Taylor (2006).) For example, the dominant paradigm has long assumed that all information is public and the exchange rate’s reaction to news necessitates no trading, but both these assumptions are now contradicted by substantial evidence from the “new microeconomics of exchange rates” (Lyons 2001).

De Grauwe and Grimaldi take issue with a third aspect of the dominant paradigm, its assumption that speculators are perfectly rational. Here, again, the empirical evidence lines up squarely on the side of the skeptics. The assumption of rational expectations has long conflicted with evidence from psychology (Yates 1990) and is difficult to reconcile with evidence...
from international economics itself (MacDonald 2002). As the authors point out, the assumption of rational expectations also conflicts with more recent evidence from behavioral finance. In fact, the normal failure of normal people to be perfectly rational was so well established by 1980 that cutting-edge psychologists began asking, instead, whether people can ever overcome their cognitive biases (e.g., Hoch 1985). In foreign exchange markets even the marginal agent is apparently irrational: currency dealers appear to be distinctly overconfident, as a group, and show no tendency to become less so over time (Oberlechner and Osler 2006).

In pointing out that the rational expectations assumption is counterfactual, De Grauwe and Grimaldi admirably fulfill Popper’s (1959) agenda for scientific inquiry in its purest form, which involves the falsification of existing paradigms. Nonetheless, taking a stand on this issue is nothing short of courageous. Behavioral research still gets a decidedly mixed response even in finance, and in international economics such research has not been warmly welcomed. Indeed, Frankel and Froot already tried to generate widespread support for this model by stressing its behavioral foundation, with limited success.

According to Kuhn, then, it seems fair to say that exchange-rate economics is in the midst of a scientific revolution, as claimed by De Grauwe and Grimaldi. Kuhn stresses, however, that a dominant paradigm can only be dethroned by a new paradigm with substantial empirical support, the search for which can take many decades. In their book, De Grauwe and Grimaldi attempt to generate support for their favored challenger. The very attempt invites disdain from adherents, but Kuhn affirms that the effort is inherently worthy of respect, since he views research within a dominant paradigm – though difficult and important – as “mopping up.”
Nonetheless, we must still inquire: *Is The Exchange Rate in a Behavioral Finance Framework likely to catalyze a paradigm shift to the chartist-fundamentalist model?* To evaluate the model a more detailed exposition is in order. Speculators in the model choose whether to be chartists or fundamentalists on the basis of lagged risk-adjusted returns, and thus their allocation across types is constantly in flux. Since the market’s structure depends on this allocation, agents would presumably be unable to know that structure all the time, which is an important justification for excluding rational agents. Equilibrium in the model requires equality between the stock demand and stock supply of foreign bonds. The equilibrium exchange rate aggregates the agents’ varied expectations, an approach outlined by Frankel and Froot (1986) as an alternative to their own “portfolio manager approach,” in which managers choose a single forecast as a weighted average of available forecasts. Interest rates in the model are exogenous, as is the fundamental exchange rate which follows a random walk. Since the model is highly nonlinear its properties are deduced from simulations.

Chapter 2 presents a toy version of the model to highlight key intuitions; the serious version is presented in Chapter 3. The next few chapters introduce additional complexities, such as endogenous risk aversion, transactions costs, and an endogenous supply of foreign assets. Chapter 8 presents empirical evidence for the model’s predictions. The last few chapters consider the model’s chaotic properties and apply it to foreign exchange intervention.

The model has three significant strengths. First, there is ample empirical evidence that chartists and fundamentalists are both active in currency markets. The existence of these types emerged as a stylized fact in the mid-1980s, based on numerous surveys of professional exchange-rate forecasts (*e.g.*, Frankel and Froot 1987, 1990). Subsequent research has continued to paint a consistent picture (MacDonald (2002) provides a recent survey). Currency market
participants assert that technical analysis – which can involve either extrapolative or regres-
sive forecasts – is a critical guide to their short-run trading decisions. Many other market par-
ticipants claim that they base their forecasts on fundamentals, thus providing further evidence
for mean-reverting expectations (e.g., Cheung and Chinn 2001).

A second strength of the model is the sensible explanation it provides for many famil-
lar exchange-rate anomalies. The model has multiple equilibria, some of which are nonfund-
damental. Exchange rates swing between fundamental and nonfundamental equilibria and,
since the model exhibits sensitivity-to-initial-conditions, the timing and magnitude of the
swings could be essentially unpredictable in reality. The model is thus consistent with the “ex-
change-rate disconnect puzzle” and with dollar’s wide swings under floating rates, another
phenomenon that has resisted explanation within the dominant paradigm (Flood and Rose
1995). The simulations show that nonfundamental equilibria are more likely when fundamen-
talists are discouraged from speculating and when chartists speculate more aggressively.

The model’s nonlinearity generates region-specific exchange-rate dynamics that bring
to mind current analyses of purchasing power parity (e.g., Chowdry et al. 2004). For example,
the exchange rate tends to stay near its fundamental value during tranquil periods, wandering
within a range determined by transactions costs. During volatile periods the rate can gravitate
towards a non-fundamental chaotic attractor.

A third strength of the model is the generality of some of its key conclusions. Most
importantly, the tendency for wide swings survives even if rational agents are present. Semi-
nal finance research by De Long et al. (1990) examines a model with chartists and rational
speculators. By sensibly exploiting the boom-bust cycles generated by chartists, the rational
agents amplify those cycles rather than dampening them as we tend to expect. Thus the ten-
dency for wide swings depends uniquely on the influence of chartists, and not on the rational-
ity or irrationality of the marginal agent.

After reviewing the evidence cited above for the model’s assumptions and seeing how
well some of its key predictions fit reality, it is hard to imagine we could ignore the chartist-
fundamentalist dichotomy and still achieve a thorough understanding of exchange rates. Un-
fortunately, however, the book does not do justice to the model's strengths. For example, it
provides none of the evidence for the existence of regressive expectations and it does not cite
De Long et al.’s (1990) evidence for the model’s generality.

Even if its strengths had fully substantiated in the book, however, the model would be
unlikely to become a dominant paradigm because it fulfills neither the adherents' nor the skep-
tics' criteria for scientific rigor. It violates the adherents' criteria because it is internally incon-
sistent. Consider, for example, the authors’ assertion that, when goods market transactions
costs rise, the fundamentalists expect less goods market arbitrage and thus less underlying
mean reversion. But the fundamental exchange rate follows a random walk, by assumption,
and mean reversion that is identically zero cannot decline.

The model violates the skeptics’ criteria for scientific rigor since its macroeconomic
assumptions and its currency market microstructure are inconsistent with reality. The macro-
economic assumption that goods-market arbitrage is impotent is exactly the opposite of the
dominant paradigm’s assumption that purchasing power parity holds continuously, but both
assumptions are inconsistent with the evidence. This matters, because if purchasing power
parity operates even weakly the model no longer has stable nonfundamental equilibria and
much of the book’s technical analysis would have to be reconstructed. (Nonetheless, as
Frankel and Froot (1986) show, the tendency for wide swings would be sustained.) The
model's market microstructure permits no role for private information and thus order flow, even though the empirical record shows that order flow has a huge influence on exchange rates (Lyons 2001). Instead, the model’s equilibrium condition, taken from the dominant paradigm, relies on the assumption that all speculators invest in a fixed supply of bonds. This implicitly assigns the supply of bonds a key role in determining exchange rates, a role that has no empirical support despite decades of research. The irrelevance of bond supplies presumably reflects the simple fact that, in reality, short-run exchange-rate speculators generally use deposits or forward contracts – which are in flexible supply – not bonds.

Presentation of the model. To challenge the dominant paradigm a model must generate widespread enthusiasm, and this requires an exposition that meets the highest standards of scholarship. Unfortunately, this book does not always fulfill that aspiration. Consider the book’s treatment of behavioral finance. Though the title promises a “behavioral finance framework,” the book’s behavioral underpinning is shallow. Behavioral finance, though unfamiliar to most international economists, is summarized in just two pages in the introduction and gets only a few scattered pages elsewhere. Inevitably, readers are denied a wealth of additional insights and evidence, much of which would have bolstered the book’s position.

It was particularly disappointing to find a significant behavioral assumption that conflicts with the behavioral evidence. Specifically, when risk aversion is endogenized it is assumed that “agents’ risk aversion declines with the size of the losses,” a choice that is justified in terms of prospect theory. In reality, however, this only applies to extremely large losses. According to the “snake-bite effect” – familiar to us all from the saying “once burned twice shy,” and by now empirically documented (Nofsinger 2004) – small and medium-sized losses

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2 By contrast, in an earlier book on chaos in exchange-rate dynamics De Grauwe and co-authors devoted an entire lengthy chapter to chaos theory per se (De Grauwe et al. 1993).
bring increased risk aversion. This issue matters, since it determines whether fundamentalists amplify or dampen exchange-rate swings.

The book could also have reached for a higher standard of scholarship in its citation choices. Though the authors never directly claim to have originated the model, they rarely cite its antecedents or its close relatives in the literature. The one citation to Frankel and Froot’s (1986) original chartist-fundamentalist model, for example, is delayed until after the model’s initial exposition and relegated to a footnote. The exclusion of relevant literature weakens the case for the model, since some research in the chartist-fundamentalist tradition provides regression based evidence that complements the book’s simulation-based evidence (e.g., Westerhoff and Reitz 2003). The exclusion of relevant literature also makes it difficult to identify the book’s contribution. For example, from the text one might conclude that the model’s potential for boom-bust cycles was previously unknown – but that was the whole point of Frankel and Froot’s original analysis.

Finally, the implications of the book’s evidence are sometimes overstated, and in one case the evidence conflicts with the model while the book claims otherwise. Specifically, the model’s simulated returns have an autocorrelation of roughly 0.2 at the one-period horizon, a figure that only reaches zero at the five-period horizon. This conflicts with one of the best documented properties of exchange rates, a lack of significant autocorrelation at short horizons. The authors ignore this contrast: “We find that, except for a few initial lags, our raw returns are not autocorrelated” (p. 133).

To conclude: In *The Exchange Rate in a Behavioral Finance Framework*, the authors rightly suggest that exchange-rate economics today in the midst of a scientific revolution, as defined by Kuhn (1970). The authors propose a chartist-fundamentalist model as the
next paradigm, showing that it is consistent with a number of exchange-rate anomalies that have eluded explanation by the dominant paradigm. The next dominant paradigm may well include chartists and fundamentalists, since strong evidence supports their existence and their influence. Nonetheless, the model is not ready to serve as a paradigm since it lacks internally consistent fundamentals and its macroeconomic and microeconomic assumptions are not based on today’s best evidence.

References


