Beliefs, Uncertainty and Decision-Making in Commercial Real Estate Markets

Colin Lizieri [*]

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It's an honour to be asked to speak to the whole conference. May I start by thanking all those involved in the organisation of the Conference, but particular thanks go to Megan Renouf and, most of all, to Dr Carolin Hoeltken. Carolin's tireless work has really made all this possible and I am really grateful. I confess that I have not found it easy composing this keynote, as my colleagues are well aware. I couldn't even decide on a title. For a while it was called "*On Liminality: An Academic Journey into the Real Estate Market*" so perhaps I will start there.

The Concise Oxford Dictionary defines liminal as "*relating to a transitional stage; occupying a position on, or on both sides of, a boundar*y." I have always liked the idea of liminality, that sense of being "in between", the dusk that lies between the day and the night. There is, though, also a sense of the unease of that transition: "*A ... space or phase of transition in which a person is no longer what they were, but is not yet what they will be. The liminal is the in-between, neither one thing nor the other. Anxiety and disorientation at that transition*"[1]. My working life is now, I suppose, liminal: I am retired and not retired. Actually, I have no intention of "retiring" in the sense of stopping research and shutting down intellectual curiosity. That said, I was complaining to a friend about writer's block, and she responded: "*maybe you just don't have anything left to say*". Well, I *thought* she was a friend.

But, in a way, my academic career also has that sense of "in between". I initially trained as a geographer[2] and the spatial is very much part of my focus, as is fascination with cities and the ways in which they develop. I rather fell into finance. In some settings I'm considered "a quant", in others, as virtually innumerate. My PhD reflected the betweenness of

[*]Colin Lizieri, Emeritus Professor of Real Estate Finance; Co-Director, Cambridge Real Estate Research Centre, University of Cambridge. This is an annotated version of the keynote delivered on 21st July 2023, Please cite as: Lizieri, C. (2023) "Beliefs, Uncertainty and Decision-Making in Commercial Real Estate Markets", keynote speech to *International AREUEA Conference*, Cambridge, UK, 2023, Cambridge Real Estate Research Centre.

[1]Adapted from the Oxford Dictionary of Critical Thinking (online). You can take the boy out of Oxford but you can't take Oxford out of the boy.

[2]As an undergraduate, I was drawn to physical geography, to glaciers and volcanoes, but perhaps spent too much time on athletics: in that liminal way I was, of course, a middle-distance runner.

geography's quantitative and relevance revolutions, informed by critical urban social and political sciences, of David Harvey, Manual Castells, Gramsci and the French structuralists but at the same time using multivariate quantitative techniques, factorial ecology methods but also drawing on cultural theory (e.g. Raymond Williams) and on social psychology, using statistical analyses and the construct theory of Bannister, Fransella and Osgood to explore concepts of community. I like to think my research work has continued to reflect that diversity of influences. My fascination with cities, their structure and development, drove me to that doctoral work and then to housing and urban policy and, somewhat accidentally, into finance, investment and global office markets and that pathway leaves traces in my approach to thinking about real estate.

In that sense, it has been a roundabout journey to Cambridge and, again, a privilege to work in an interdisciplinary Department with expert colleagues from many different subjects and interests, an opportunity to learn from those different perspectives. Cambridge, though, can be cruel on your self-esteem. All around you are reminders of the intellectual leaps made here, the discoveries (gravity, evolution, the electron, artificial intelligence, DNA), the 121 Nobel prizes and more. It is easy to get imposter syndrome, we warn our students about it, but it applies to us, too.

Nonetheless, as we have brought you to Cambridge, I thought I should at least begin with Cambridge. I want to take you back a hundred years, to 1923 and, as this is AREUEA, to economics in Cambridge. Which, surely, must mean Keynes. So, one hundred years ago today, Keynes published *A Tract on Monetary Reform*. In keeping with my age, this is, of course, famous for one quotation:

> *"In the long run, we are all dead."*

I am not sure in my liminal, dead/alive state that is really the message I want to hear. But it is worth recalling what he is *actually* saying here:

> *"The long run is a misleading guide to current affairs. In the long run we are all dead. Economists set themselves too easy, too useless a task if in tempestuous seasons they can only tell us that when the storm is past the ocean is flat again."*

This is, in effect, a critique of unthinking equilibrium economics: the assumption that supply and demand will generate an efficient price, and that all we need to do is to wait for that to occur. Although early, it will form the basis for arguments in favour of intervention and the idea that there is no natural state of equilibrium. At that time Cambridge had many economists who were concerned about inequality and the impacts of harsh economic policies on society, observing the widespread distress of the time: liberals like Keynes and Pigou, socialists like Dobb, communists like Sraffa. And then there was Frank Ramsey.

In conversations with international colleagues, relatively few seem aware of Frank Ramsey. Those of you with formal economics training might be in passing, although he seems to have disappeared from many standard textbooks. Yet if anyone can give you impostor syndrome, it would be Ramsey.

Ramsey arrived at Cambridge aged 17 in 1920, having missed the Great War but not its consequences. He was appointed a fellow at Kings in 1924 and a university lecturer in 1928. He made seminal contributions to economics in his time at Cambridge which have had lasting consequences. Those trained in economics may well have encountered his work on taxation, which provided a mathematical framework for differential taxation, linked to the elasticity of demand that paved the way for the Diamond-Mirlees optimal taxation Nobel prize. They are perhaps more likely to have met his work on savings in an economy to maximize utility over infinite time horizons to ensure inter-generational equity (the Koopman-Cass-Harvey model) that is still fundamental in macro (although his name is often omitted from textbooks).

It has been argued that the mathematics in these papers (in particular the use of calculus in the utility function central to the arguments) directly lead to the concept of a representative agent, alongside the concept of the best feasible solution. Ramsey would not have been happy with that: his model was very much in an interventionist spirit, with, rather, a disembodied social planner (no doubt from Cambridge) making decisions to maximise social welfare - but the mathematical approach required a simplification of diverse utility functions. In other writings (many delivered orally but unpublished before his death), Ramsey is clear that he thought that utility varied between individuals and, in particular, differed between the wealthy and those on low, or no, incomes.

His thoughts on social discounting over the very long run strongly influenced Pigou's later work (so if you work with externalities, welfare economics or long-run sustainability, there's a bit of Ramsey in there) and his thoughts on social discounting and inter-generational equity prefigure the Nordhaus versus Stern arguments on discount rates in climate change economics. If you are in a quantitative Marxian tradition, it is evident that he helped Sraffa on the mathematics of the labour theory of value, without accepting it.

And yet: he wasn't an economist. He read mathematics at Trinity college and his lectureship at the University was in Mathematics. He, however, considered himself to be a philosopher, working primarily on formal logic and truth (and, from that, the foundational and symbolic basis of mathematics).

For all that focus on philosophy, he made significant mathematical contributions. For example, he worked on combinatorics, set theory and graph theory. There is a class of numbers, Ramsey Numbers, that carry his name and are still taught as Ramsey theory. He was close to making the breakthrough that Gödel would make on set theory and incompleteness, that undermined Principia Mathematica.

More significantly for our purposes (more accurately, for my purposes), he made a major advance in probability and subjective probability, formalising Bayes theorem (along with, independently, Finetti) and rescuing probability from a frequency blind alley with the concept that an individual's prior probability can be quantified in terms of a a fair bet (that is a bet for which there is no Dutch book): an idea that strongly influenced von Neumann (who acknowledged it) and led directly to Game Theory and to von Neumann and Morgenstern (1944). No Ramsey, no Nash. Hence, if you work in a game theoretic framework – there's a bit of Ramsey in there.

But he was a philosopher (the subjective probabilities come out of his thinking on logic, truth and conditionals). He provided the first English translation of Wittgenstein's Tractatus (Ramsey, 1923) and published a review/critique which ultimately led to Wittgenstein comprehensively revising his thinking - as an undergraduate. His work on truth, logic and belief influenced Russell and Moore (and the Vienna School) and is still there in formal logic in the form of Ramsey Sentences.

Aside from making major contributions to three (or more) academic fields, he was very interested in the emerging area of Psychoanalysis (and was himself treated by one of Freud's first students): he did not think much of Freud's formal theories, but did think psychology was important in understanding actions and beliefs and individual difference – this might well have fed into subsequent work on economics and on subjective probability and beliefs. And he was part of the Bloomsbury set, albeit somewhat peripherally (he does not feature much in the accounts of them) and had radical views on religion and society for the time (he was avowedly an atheist: his brother became Archbishop of Canterbury!). Shockingly though, he contracted a liver infection and died in 1930. He was just 26. What more could he have achieved? Had he lived a few years longer, he would almost certainly have been Alan Turing's tutor[3] and his unpublished work (some collected together in a posthumous volume by R B Braithwaite give tantalising insights into future directions.

Aside from making us feel very small and trivial, what can we draw from Frank Ramsey's contribution to knowledge? Specifically, what might they tell us about how we might think about research into commercial real estate? I want to take this in a general and a specific direction: first the strong cross-disciplinarity aspect of his work and then to the question of beliefs and their role in subjective probability.

It's worth dwelling briefly on the way that Ramsey, Keynes and their contemporaries worked: in solitary of course (and with no distracting internet access or social media, no television) and in direct conversations with colleagues (mostly local although there were in-

[3]For those unfamiliar with Ramsey's life and work, I would urge you to read Cheryl Mishak's "Frank Ramsey, A Sheer Excess of Powers" published in 2020ust a few years ago, which captures the breadth and depth of Ramsey's talent and achievements.

ternational visits, with complex travel arrangements made by post). Most of all though, they met in groups where one or more would read a paper (no circulation, no slides, no phones to distract). Those groups were sometimes single discipline, but more often than not cross-disciplinary. Clearly – it is Cambridge – it was cliquey and, to a considerable extent, exclusionary. But it meant that early work, developmental work was scrutinised, debated and contested from a much wider perspective than is the norm now. Over time, we have seen a growing separation as disciplines and sub-disciplines have become more specialised and siloed, with alternative perspectives seen more as "the enemy" than as providing useful insights. This is, of course, reinforced by academic structures: the necessity to publish and the editorial boards of journals acting as gatekeepers to repel non-conforming views. I exaggerate of course: but many of my generation have direct experience of the major real estate economics and finance journals desk rejecting articles that were not about US markets or that did not conform to the prevailing models, and of being ghettoized in conferences into "international sessions" (often inconveniently time-tabled, thematically incoherent and ill-attended) and of the difficulty of getting, for example, behavioural or survey-based papers published in the field journals.

In a sense, this takes me back to liminality: my academic background and my interests to an extent leave me sitting between disciplines: at times it has felt like being tethered to horses whipped to ride in different directions. This, I think, is a problem for us working in real estate (and urban economics) and, in particular, in commercial real estate: I do not think you can understand the functioning of cities without that multiplicity of perspectives, and denying insights from different fields is a narrowing of horizons and a loss of nuance which is damaging.

Let me provide two brief examples of this process. To an extent, they stereotype and simplify areas of enquiry as they apply to real estate, so I will pass over them swiftly. First, I have picked out economics and financial economics as an example of a silo, where increasingly specialised modes of analysis and sets of assumptions dominate. This has led to attacks, of course: from behavioural science questioning the decision-making processes of individuals, from critical social science and its cultural turn, and in public and political discourse ("we've had enough of experts"), particularly in the aftermath of the global financial crisis. This is not new of course: much of my early research thoughts surrounded the nature of data and its production and Keyne's 1923 quotation, above, is a critique of equilibrium thinking from a century ago. At issue here is what impact has this had on specialist output in the field, wedded to an increasingly rigid approach and style? Has there been agonising on methods and methodology? A questioning of embedded assumptions? A welcoming of diverse approaches? No: if anything, more an entrenchment of views and disengagement with the debate. Fama's quotation here is not atypical of that rejection:

> "[T]*he behavioral literature has not put forth a full blown model for prices and returns that can be tested and potentially rejected—the acid test for any model proposed as a replacement for another model*" (Fama, 2014)

In its turn, behavioural finance (not least in its applications in real estate research) has a tendency to present a picture of "conventional", rational utility finance that lacks the nuance that is present in much of the work from that tradition (for example, the incorporation of noise traders and uncertainty into arbitrage models) and to date everything from Kahneman and Tversky's *Prospect Theory* (1979). As Thaler (2016) noted, over-confidence and loss aversion are present in Adam Smith and, again, as we are in Cambridge, we should note Keynes's "animal spirits" and "beauty contests", both found in the 1936 *The General Theory of Employment Interest and Money.*

Lest it look like I am biting the hand that feeds me, let me turn to the "new" literature on "financialisation" from critical social science. Now there are clear methodological and ontological stances here that should shape what is knowable and what is done (often neglected down the lineage). That, though, often brings a denial of the validity of alternative views. In my case I have had referee-author and symposia arguments on the role of demand in asset pricing (which has to be denied from this tradition since all is supply – finance capital creating products and vehicles to sell and with demand as a driver just an obsession of conventional economics) and on key differences in the behaviour of actors, their motivations and their strategies in this monolithic "financial capital" category. Clearly, some of the best authors in the field are well aware of this and are engaged[4], but many others are not. For me, too, some of the concepts and definitions are fuzzy and vague – Brett Christophers (2015) provided an early warning of this from within the field and there are echoes of some of the criticisms of the critical regional studies literature in the 1990s (for example Ann Markusen's 1999 critique). I am also concerned that there appears to be a very ahistorical view in this literature that seems blind to the fact that much of these arguments have already been made, in Manuel Castells, in David Harvey, in Doreen Massey for example, even prefigured in early Lizieri (1991), too![5] A later Lizieri (2009) appears occasionally and briefly, too, via references to *Towers of Capital* but some of the interpretations (and my royalty statements) make me doubt that anyone had actually read it. This myopia is echoed in a tendency to ignore the historic origins and antecedents of many of the "new" products and vehicles claimed to have been created by "financial capital": those claiming financialization of real assets is a

[4] It's invidious to give examples, but, for example, Aalbers, Christophers, Dörry.

[5] I reread this, probably for the first time in a quarter of a century, in preparing the keynote: it includes a discussion of product innovation in property investment and finance, the treatment of City office property as a financial investment asset and barriers and inertia to adjustments required in a more flexible working environment. Sadly, it also contains a discussion of the property implications of the UK joining the Single European Market. *O tempora, o mores ...*

new phenomenon would be well-advised to research the 1890 Barings Crisis, for example.

There's another disjuncture, too, specific to our fields: a growing gap between academic work and the commercial real estate market as practiced. How many academics now are fully engaged with practice? This gap has ebbed and flowed across my career, with closer connections at the beginning, but with growing distance as real estate research became more technical and specialised. It's a two-way street: practitioners reject innovation and technical work – until they embrace bowdlerised versions of it and claim that they always knew that anyway[6]. But academic practice increasingly ignores process, the importance of institutional structures, the very nature of decision-making too. To be fair, there is very little incentive for an early career researcher to engage with the industry when their tenure depends on publishing a non-parametric spatial econometric analysis of south-facing houses in the *Journal of Obscure Property Econometrics* or rail against an amorphous financial capital in *Commanding Heights Quarterly*. But if you don't engage, how can you analyse process? This, for me, seems critical particularly in commercial real estate markets: in a sense, as individuals we are "in the market" in residential property but this is not the case in commercial real estate.

There is another aspect here, which does not really fit the flow of this paper, but I felt should be included. I suppose it in part reflects the "uncertainty" in the title. We are not able, with any precision, to say what will happen next, this isn't physical science and you cannot say that in physical science either post-Heisenberg. However, when we are asked to comment on – for example – the impact of working from home on office demand or cities, the impact of artificial intelligence on work and, hence, the need for real estate, on the role of place-making in rental growth, saying "*I don't know*" or "*it depends*" no longer seems to be an allowable answer. And nuance and caution don't make headlines or get attention. Consultants and research houses and those anxious to be seen as "thought leaders" seem readily able to make definitive pronouncements (and there seems to be limited damage to status and reputation as those definitive pronouncements are revised or are not fulfilled and are quietly forgotten. Academia is not immune to this, of course although I had better not name names here.

Here is an example, sent to me in the days before the keynote by a former student: the McKinsey Global Institute's (2023) "*Empty spaces and hybrid place*s". To be fair to the pub-

[6]As an example, consider green building premia. As recently as 2016 when I chaired the World Economic Forum's Industry Agenda Council on the "Future of Real Estate and Urbanization" a majority of senior practitioners refused to accept that there were rental or capital value premiums for certified green office buildings: the academic research showing them must be flawed, the results driven by missing factors. Now it is part of received wisdom and major real estate consultants headline research studies showing these premia (using methods inferior to the earlier academic research and not having the grace to acknowledge it). I have had a similar experience discussing spillover and contagion effects in global office markets with professional audiences (e.g. with Zhu & Lizieri, 2021).

lication, they set out three scenarios (but only really focus on one and do not provide any sense of the probability or weight of their occurrence, nor the effect of sample selection bias and the over-representation of US cities) and there is an extensive survey basis for it: but there is so much missing in terms of embedded assumptions, the jumps from survey responses to assumed outcomes, missing error margins and confidence limits, that the leaps to highlighted statements "*there will be 13 percent less demand for office space in the median city we studied*", "*demand growth for residences will be muted, especially in urban cores*", "*up to 7 percent of the people in urban cores left for good*" seem too definitive and confident. I have no doubt that the authors are less dogmatic than this, but these are assertions about unknowns and a set of recommended actions follow on from them that offer normative practice. It is unfair to single out this publication as it is representative of many that have appeared in the wake of the pandemic (alongside academic papers rushed to publication) where making bold statements seems to trump the need for nuance and caution[7].

So, why does any of this matter? It matters because we are dealing with decision-making in a private market characterised by noise and uncertainty. It's that list that many of us use to start our introductory lectures:

- Heterogeneity
- Large Lot Size
- Holdings, Tracking Error and Specific Risk
- Private Markets and Information Asymmetry
- Thin Transactions and Noise
- Limits to Arbitrage
- Illiquidity, Holding Period & Time Horizon (etc. etc.)

That list has implications in what we teach and what we research, but we do not always follow them through. It implies that market processes matter critically in outcomes. It means that institutional structures matter. To read real estate finance literature, you would think the world was America: but massive differences in the organisation of financial markets, mortgage markets, accounting and tax rules alter outcomes. As an example, how can you understand international REIT behaviour without accounting for differences in depreciation allowances or how assets are expressed on the accounts? Yet something like a book

[7]With the "working from home" and also the co-working academic literature, there is again a tendency to ignore history and past literature: I have seen few references to the flexible work / serviced offices literature from the late 1990s and early 2000s, even though many of those papers touched on key themes in the current papers – again, drawing from my own work, Gibson & Lizieri, (2001), Lizieri (2003).

to market ratio is treated as if it is unproblematic. Agents matter: the "large lot size" "thin transaction" dyad means that big deals happen infrequently so what the individual actors do at that point shapes the market. Moving from real estate finance to financialization as an academic perspective, it is worth a reminder that critical social science's "structure and agency" is "*and agency*" not structure alone. Difference matters: the investment motivations and strategies of a long-term investor or a risk-averse investor are not the same as those of a finite life private equity fund or a hedge fund and it is not sensible to lump them all together into a single category nor to assume that there is some representative investor whose behaviour approximates to each actor[8].

One way of thinking about those things (and in a sense, I hope this follows from my own body of research) is to look at the individual decision-makers and actors themselves: another is to see how embedded beliefs can persist and influence market outcomes. I've coded this "Gurus and Mythologies" with apologies to anyone who thinks that the "guru" there is cultural appropriation.

Defining terms here, by mythology I mean a belief that may have some basis of fact or foundation but is believed irrespective of facts or evidence. I have encountered many of these in my career, some do fade away or are replaced by new orthodoxies. They *matter* because they shape behaviour. Here are some examples:

- *Real estate is an inflation hedge.* It's almost an act of faith. Analytically we know it isn't really especially for listed real estate, it certainly doesn't hedge unexpected inflation in any technical sense, and the relationship between inflation, interest rate policy and capital values is complex[9].

- *Office rents capture economic growth and deliver real growth.* They really don't, not on a building or a per square metre basis.

- *Major global cities offer better risk-adjusted returns.* Well, the evidence is a bit mixed but my work (thanks to all my co-researchers) shows downside risk, volatility, plus loss of diversification benefits[10].

- *Cap rate is an indicator of risk.* Yes, probably, but not the way the market believed[11].

- *Managers time their leverage decisions.* Yes, they probably gear up at the top of the market, assisted by the banking sector[12].

[8]It would be ironic if the financialization literature had created the "representative financial capitalist" in an echo of rational utility economics it rejects and critiques.

[9] *For example, Glascock* et al. *(2002), Hoesli* et al. *(2008); Muckenhaupt* et al. *(2023). For a contrary view, Amenc* et al. *(2009).*

[10] *For example, Lizieri & Pain, (2014); Zhu & Lizieri, (2021); Hoesli & Johner (2022)*

[11] *See Beracha* et al. *(2017), Plazzi* et al. *(2010)*

[12] *As an example, Alcock* et al. *(2013), Giacomini* et al. *(2015).*

- *Best managers deliver consistent alpha.* Evidence suggests not, particularly if you look at follow-up funds[13].

As an example, consider the Figure 1: it shows UK office rents and capital values in real terms (deflated by the RPI, which might overstate inflation in the latter period but remained the basis of many financial contracts) over my academic career. I like to think I've been responsible for a 60% real loss in office market capital value by my efforts: make capitalism more efficient and it will fall apart under the weight of its own contradictions! However, if you show this to most practitioners, they simply do not believe it, even though it is their own data.

These sit hand in hand with situations where institutional positions and beliefs lead to positions which, from any rational model, are inconsistent or "irrational" – I am cautious of using that word since it presumes the existence of a rational or economic equilibrium position which may not hold in practice. Again aged examples from my working experience include the under-valuation of over-rented UK offices properties after the 1990 downturn (where valuers considered that cashflows from tenants paying above market rent due to upward only rent review clauses implied high risk so raised cap rates substantially: eventually arbitrage removed that) or the yield adjustments applied for break clauses when they arrived (a conference paper, sadly never published, showed that valuer's own assumptions on the exercise of breaks was inconsistent with the yield adjustments applied). We could look at herd valuations of sub-sectors in the same light, ignoring the inevitable supply adjustments.

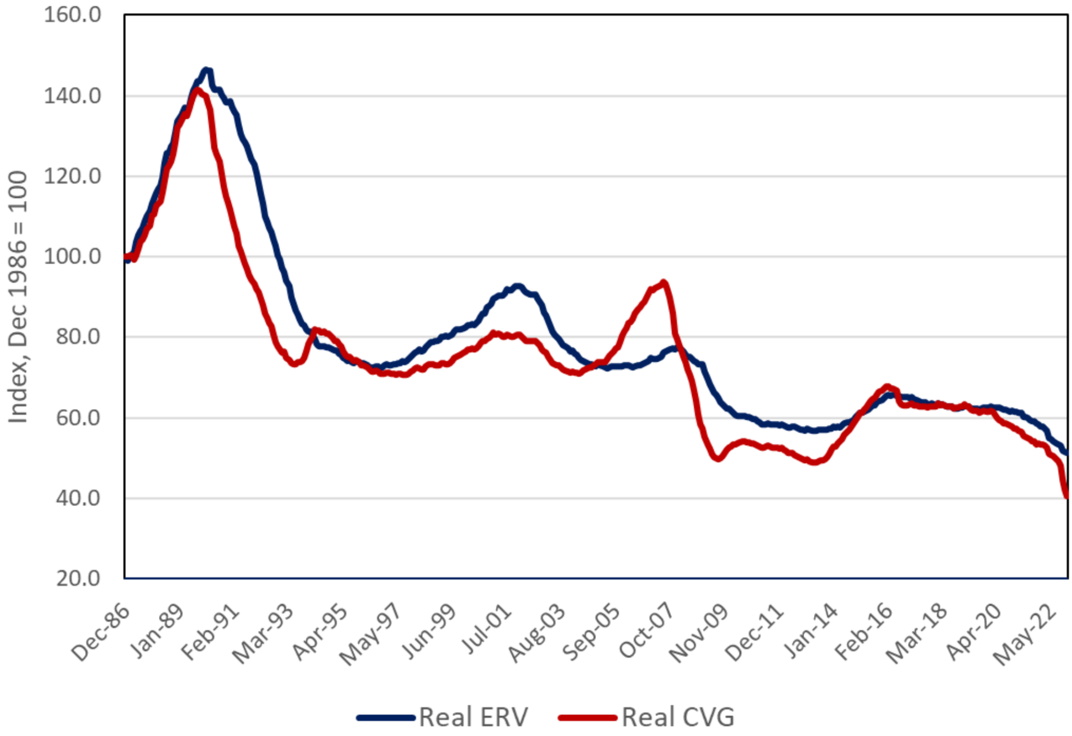
The key question you should be asking is how can this happen in a professional, informed market? From formal financial economics, we need to acknowledge the limits on arbitrage (it is harder to short the private real estate market than even in a Shleifer and Vishny (1997) world) but these myths and inconsistencies might *occur* but they should not persist.

So what mechanisms are there to preserve them? Well, one aspect, for me, is the role that key individuals play in the market place: not least because of that "major but infrequent" activity pattern. What evidence we have (and it is neither easy to do the research nor to publish it) shows the capacity for strong powerful individuals to influence outcomes. For example, the Investment Property Forum Hurdle Rate project (Hutchinson *et al.,* 2017), as well as showing typical heuristics and models far from consistent with corporate finance[14], indicated that in investment committees, senior individuals frequently over-rode model-based decisions. With no tradition of back-testing (and the problems of heterogeneity) we cannot test the impact of that, we can only guess at it. Client influence and valuation process studies show a similar story, as does the prevalence of rules-of-thumb in decision-making, notably in real estate development and in private equity.

[13] Bond & Mitchell (2010)

[14] To be fair to financial economics, this echoes the US findings in Graham & Harvey, 2001).

Figure 1: UK Offices, Real Rental and Capital "Growth" 1986–2022



Notes: The figure shows estimated rental value and capital value indices for UK standing commercial office investments, deflated by the retail price index: source, MSCI and ONS, author's calculations.

There are many reinforcement mechanisms here: first the belief that there *are* market gurus with particular market insights and perspectives. Second, the deal driven, entrepreneurial reward structure encourages such decision making – and those making the decisions act as role models for rising workers who probably soon forget what we have taught (or, if they don't, don't rise up). It is possible: it's a suggestion, not a fact, that recruitment reinforce this by selecting candidates who fit that mould too – what is being sought in the filtering, assessment and interview process? And, additionally, where do people gain the information and ideas that form their views and perspectives: it's a "people business" so internal networking plus the nature of professional and trade press and media which seek and publish the views of those opinion formers and the consultants that hold a mirror up to the sector. I'm over-stating this, but it's a powerful echo chamber[15].

Which brings me back to Frank Ramsey. First, I need to acknowledge the stream of work

[15]I'm grateful to Carolin Hoeltken for drawing my attention to a forthcoming publication by Holmén et al. which examines the personality traits of financial professionals and finds them to be "*less risk-averse, less trustworthy, show higher levels of psychopathy, and are more competitive than participants from the general population*". That is the headline from the abstract: the findings are more nuanced but does point to the possibility that particular behavioural traits might influence outcomes, particularly in heterogenous private markets.

in behavioural finance applied to real estate markets. While much of this is in consumer, retail, markets, looking at residential decisions and pricing errors, some does look at commercial and professional practice. I wish there were more, but it probably isn't going to be from prospect theory that we make advances in understanding the deep market practices that affect outcomes in commercial real estate. The puzzle, if you accept my gurus and mythologies premise, is why myths, entrenched beliefs and ad hoc practices inconsistent with a rational economic utility-maximizing model are persistent and systematic in an informed professional market: Ramsey's work on assigning probability according to utility should move the market to a more rational position. But it is in the truth and logic element of his work, particularly that on subjective probability, that there might be useful insights from a century ago. If you think about a model with Bayesian updating, then we absolutely need to know what is forming the priors: Ramsey's formulation of this (and it is relatively early work) was:

*The degree of belief in p & q = degree of belief in p * degree of belief in q given p.*

For my purpose (and Ramsey's) the important bit there is the "belief" under conditions of uncertainty. And there is no guarantee that this is driven by rational utility maximisation. Ramsey was a pragmatist, influenced by utilitarian thought, but he also had a much wider and rounded view on beliefs that also included the insights he had gained from Freudian psychoanalytic theory.

From this, with strong embedded assumptions and beliefs, it is quite possible that priors are not adjusted in a way that (in a rational expectations way) accounts for the arrival of new information[16]. This then opens the door to behavioural biases in decision-making from confirmation bias, filtering of sources and, hence, to herd-behaviour, over-confidence, money illusion, groupthink and other suspects. If that *is* the case, what is the validity of a model that assumes all of that away?

Where might this take us? It's a potential research agenda. First, we need to accommodate agency explicitly into our existing research models and modes, from whatever tradition. Agency matters, difference matters, outcomes may be persistent and systematic. We need to challenge the core assumptions of existing models: as an example: REIT studies use the concept of target leverage, looking at the impact of deviation from target, which is a function of aggregate market leverage and firm characteristics. Now suppose the market is *wrong*. What happens to those models now?

Second, we need to be more open in seeking ideas from outside our fields, to break down silos and be more open to external ideas and concepts. I know that is hard from a career per-

[16] As an aside, the original Quan & Quigley (1991) smoothing paper touches on this in noting that it might be irrational for an appraiser to fully adjust for new but noisy transaction data.

spective. I think we should try to build more interdisciplinarity and methodological formality into our research training. I have lost the battle to remind colleagues that methodology is *not* the same as method but it's a distinction we should try to preserve and to think through what it implies. I suppose the key question, though, is how could we conduct research into it that goes beyond the conceptual, that tests these ideas? With considerable difficulty, but in principle it is feasible – even if that may mean experimental research with professionals! Assessing impacts and tracing causation is much more complex – but then that is a deep philosophical issue in its own right, if neglected in much of our research practice.

Let me finish by returning to Keynes and Ramsey. While Keynes noted the inevitability of death, Ramsey painted a more optimistic picture: "*In time the world will cool and everything will die; but that is a long time off still, and its present value at compound discount is almost nothing. Nor is the present less valuable because the future will be blank ... it is pleasanter to be thrilled than to be depressed, and not merely pleasanter but better for all one's activities*" (Ramsey, 1925, cited in Misak, 2020, p218). I am still fascinated by the forces that shape our cities and the processes that move our markets. I want to know more. And I think, I hope, that the insights will come from those liminal spaces in between disciplines and the acknowledgement of the importance of individuals and agencies in an uncertain world. Thank you.

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