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"Musings on the Cass Trick"

by

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Abstract

This is a leisurely introduction, in the vein of a piece in the history of science, to belated publication of my well-known paper on incomplete markets, "Competitive Equilibrium with Incomplete Financial Markets

I was very pleased when Bernard Cornet told me that he'd like to publish my first (in fact, more accurately, second) substantial paper [3] concerning financial equilibrium (hereafter simply FE). I've always liked the paper a lot, and thought it was a significant and influential contribution. In my view, together with a handful of other pathbreaking papers – most notably Geanakoplos and Polemarchakis's (hereafter simply John and Herakles's)[1] beautiful piece [9] – it resurrected interest in a vitally important area of equilibrium theory (in fact, brought equilibrium theory itself back from near death). The focus of these papers was on incomplete financial markets. More generally conceived as the melding of commodity markets

[1] I will follow this practice, switching from family to given names, throughout. After all, the first participants in the flowering of GEI are (for the most part) now friends as well as colleagues. More generally, the whole tone will be pretty informal. It will also be consistent with my mature view of life, pretty frank and opinionated. Finally, I must emphasize that the note is largely an exercise in the history of science. This aspect will be much more apparent if one skips the remaining footnotes – intended to be just that, adjuncts to the main flow (or niceties of technical detail) – at least on first reading. Also, since it is history from my personal perspective, I have more or less given free rein to my ego. Just count the relative number of references to my own work!

with financial markets in the broad framework of general equilibrium, the area remains vibrant: There are still many, mostly (deeply) unexplored problems. One major class of such problems concerns analysis of the implications of institutional restrictions on financial activity. Unlike very partial equilibrium, very microtheoretic explanations for the emergence of specific types of institutional arrangements, this involves abstraction of the essential structural features of information-based market failures in a much wider setting, one where their overall effects can be studied and understood.

Another major class of problems concerns seriously accounting for the indisputable fact (at least to anyone with a modicum of common sense) that a multitude of inside instruments are the "means of payment" in the 21st century.[2] M (or M1, or M2, or ... whatever) has simply become irrelevant, and we should get on with the extremely important task of fully developing the one truly general model which is most suitable, indeed indispensable for studying and understanding, in the large, how modern financial institutions actually facilitate economic activity.

Obviously these two classes of problems are closely interconnected. But enough of this proselytizing – back to the main story.

My paper and John and Herakles's paper are very nice complements, establishing existence for two seemingly polar models of asset structure (with nominal v. numeraire [a very useful, special case of real] assets), and then exploring optimality from two actually opposite perspectives (constrained optimality v. constrained suboptimality). There now seems to be general consensus that their approach to analyzing optimality is by far more interesting – and I fully agree. So, my focus here will be on the problem of existence, more specifically, on the device I introduced for establishing existence with nominal assets, what John has labeled (p. 21 in [7]) the Cass trick.[3] Later on I will outline why my device can be employed to the same end with any indexed assets (a natural generalization of both nominal and numeraire assets), even beyond. Before doing so, however, I want to briefly digress in two directions: First, I will explain why my paper was never published (it was never even submitted for publication). Second, I will describe some aspects of how I would (and wouldn't) have rewritten it for publication, given some further insight into the essential features of GEI. The substance of the paper you can read for yourself.

How the paper came about – but was then never published

The genesis of the paper was my interest in constructing a simple, tractable example of sunspot equilibria, one where it was absolutely clear this phenomenon had nothing at all to do with multiplicity of certainty equilibria.[4] Looking at one commodity, two household examples with incomplete markets for numeraire assets – an obvious candidate for market failure leading to sunspot outcomes – revealed (as John and Herakles later proved generally) that any equilibria would typically be (as in Karl's and my original example) finite and isolated. This meant more of the messy sort of construction in the appendix to [5], so I turned from numeraire to nominal assets. And, voila, for the simplest example (with just two future spots but only one asset), a continuum of distinct sunspot equilibria appeared (provided there were incentives to trade in the absence of sunspots), even when there was only a single certainty equilibrium, the result detailed in [2]. This, together with Martin Hellwig's perceptive observation that this GEI example had nothing intrinsically to do with sunspots, piqued my interest, and led first to this paper, and later to my joint work with Yves Balasko [1] (as well as Geanakoplos and Mas-Colell's parallel work [8]) extending the analysis of generic real indeterminacy with nominal assets to the standard, two period model.[5]

But why didn't I (at least try to) publish the paper? In retrospect I think the main reason was simply that I absolutely hate to revise and rewrite.[6] For me, a well-written paper is like a fine (sometimes even elegant) piece of furniture, where all the pieces are carefully crafted to fit neatly together as a whole. But this means that – just as in general equilibrium – a change here requires a change there requires . . . . Very soon after I finished the version published here I began to realize that there were some basic shortcomings, things that really needed redoing (more about this shortly). I would have probably revised and submitted the paper in due course, however, but for two circumstances. The first, and to be honest, probably the less important, was that I learned of Jan Werner's alternative proof of existence [12] even before I had finished writing this up, when I gave a seminar at Bonn during the winter of 1983-4. And when I finally saw his paper – I believe that this was after I had finished writing but could be wrong – I liked it. So one of my first reactions (my Grandmother would have called it my lazyman's reaction [but I must confess to having had other less admirable reactions too]) was something like this: "You know, this is Werner's first major piece of research (it was a chapter in his thesis), he needs publication and publicity much more than you do." This ready-made excuse was then reinforced a year or so later when I became aware of Duffie's second alternative proof [6], which I also liked (and which was also part of his thesis), about the time he gave a seminar at Penn during the fall of 1985-6. In the end this all turned out perfectly fine, as far as I'm concerned. While our three papers contain quite different, clearly independent analyses, both Jan's and Darrell's as well as mine are nice representatives of the proof of existence with incomplete markets for nominal assets under assumptions comparable in generality to those developed in GE over thirty years before.

The second circumstance, much more compelling for me than the first, was that I had already submitted [2], which received truly awful treatment[7], and was pretty down on the peer-review-filtered-through-an-editor process. To be fair, in my experience, the vast majority of editors (and others in a similar position) make a real, serious effort. For instance, [1] received truly even-handed treatment by a person well-known to hold equilibrium theory in extremely low regard. But there is the occasional very bad experience, which leaves a very bad taste.

With the passage of more time I eventually came to view the paper as just another "existence exercise," and rationalized doing nothing further by thinking to myself "Who needs that?" And finally, even later, I came to kind of like having a famous unpublished paper – knowing full well that anybody who was really interested in GEI would find out about it, possibly even get ahold of and read it.

How the paper would have been rewritten

As I saw it soon soon after being written, there was one major problem with the paper, my very awkward treatment of no-arbitrage, a central theme in GEI. (I still like the sound of "no fast bucks," though!) Whereas there I used several pages of tedious algebra, nowadays I toss off the "hard" part of the proof of equivalence with the state price characterization as a two or three line implication of Farkas lemma, as we all do. In connection with this it had also become very clear to me that, in my analysis, the only property of the overall return matrix

$$R = \begin{bmatrix} -q \\ r \end{bmatrix} \in \mathbb{R}^{S+1,I},$$

(where, basically as in [3], q are the prices of I bond-like financial instruments [which will be known here as A nominal assets] and, for $r = [r(s), s > 0]$, $r(s)$ are their returns at $S > I$ future spots) that mattered was no-arbitrage. In particular, signs of the returns themselves were immaterial, and, of course, no-redundancy (i.e., rank $r = I$) was for free.

As regards the argument itself, except for some quaint notation and terminology, it still reads pretty well. At some point I had recognized, however, fully in the spirit Ken Arrow had taught me GE at Stanford in the early sixties, that, spot-by-spot local nonsatiation (rather than weak monotonicity) sufficed for both no-arbitrage and my particular proof – given the convention that prices are always nonnegative, and the (implicit) assumption of free disposal.

There are also a few minor ambiguities and inaccuracies here and there (in particular, I could have made it clearer at the outset that "purely financial phenomenon" meant precisely that returns were specified in units of account, which in turn are spot-dependent). Finally, while most of what I had to say in the last section, entitled "Extensions," also still reads pretty well, the closing comment concerning drop in the rank of r (when it depends on spot goods prices) is dead wrong – though I didn't really appreciate this completely until the following fall. As everybody – including me – now well knows, for dealing with this kind of irregularity the relevant approximation involves the A-dimensional subspaces of $\mathbb{R}^S$ containing the columns of r, not the particular mappings generating them. The rest – especially concerning restricted participation – rings as true for me today as it did then (though it could now use some fairly obvious updating.)

The Cass trick itself (pp. 12-14 in [3])

This section is necessarily somewhat technical. But I want to avoid introducing all the apparatus necessary for absolute precision. So I presume that you are fairly familiar with contemporary equilibrium theory, especially that you understand

- Why I concentrate attention on the overall return matrix – in particular, the natural form which spot market budget constraints take in FE (with the value of a household's portfolio at unit-of-account overall returns appearing alone on the right-hand side), and
- What it means when I move from general results (concerning existence) to generic results (concerning determinacy) – in particular, the additional regularity in the economy's primitives which this requires (for instance, that the typical household's utility function be [at least] C^2, differentiably strictly increasing, and differentiably strictly quasi-concave, and satisfy a boundary condition, for strictly positive consumption) in order to be able to describe FE in terms of a system of smooth equations.

The essence of the Cass trick – actually a very simple idea – is to solve the "classical-like" problem of irregularity of demand at the boundary of the spot goods-asset price space by replacing some household's[8] demand for goods and assets (when he has access to only spot goods and asset markets) with his Walrasian demand for goods (as if, instead, he has access to an overall contingent claims market at spot goods prices). In short, one just replaces Mr. 1's spot goods cum assets budget set with his Walrasian budget set at the same spot goods prices.[9] John has written a very clear, concise explanation of how this maneuver works (pp. 21-22 in [7]). What I aim to do here is to elaborate a bit on the underlying structure of asset markets required.[10]

For this purpose I will (implicitly) assume the weakest form of a monotonicity-like assumption on utility functions consistent with the "classical-like" proof of existence I developed in [3] (for specificity, you can simply assume that utility functions are weakly increasing spot-by-spot, as I did in there). I will also generalize and modernize notation a bit. For instance, spots will be expressed as arguments rather than superscripts ($r(s)$ rather than r^s), and the set and number of future spots more congenially ($s \in \mathcal{S} = \{1, 2, \ldots, S\}$ with $S < \infty$). Also, let

$$\begin{aligned} P &= P(1)^{S+1} = (\mathbb{R}^C_+ \setminus \{0\})^{S+1} \\ &= \text{ the set of potential spot goods prices} \end{aligned}$$

(here $P(s) = P(1) = \mathbb{R}^C_+ \setminus \{0\}$, all s, i.e., the set of potential commodity prices is the same at each spot), and

$$\begin{aligned} Q &= \mathbb{R}^A \\ &= \text{ the set of potential asset prices} \end{aligned}$$

(here assets are labeled by the superscripts $a \in \mathcal{A} = \{1, 2, \ldots, A\}$ with $A < \infty$).[11] Then, without specifying the date-event tree in detail, we can think of the overall return matrix as simply a C^0 mapping

$$R : P \times Q \rightarrow \mathbb{R}^{S+1,A}.$$

Successfully employing the Cass trick now involves two assumptions, the first innocuous, the second not all.

R1 *No-arbitrage.* There is $\lambda \in \mathbb{R}^{S+1}_{++}$ s.t., for $(p, q) \in P \times Q, \lambda R(p, q) = 0$.
R2 *No-redundancy* (one possible version). For $(p, q) \in (\text{int } P) \times Q$, rank $R(p, q) = A$.

The gist of R1 is that column a of the overall return matrix R is associated with only asset a, whose return at some spot s^a is linear in its price q^a. Otherwise, the return at spots $s \neq s^a$ will depend only on $p(s)$ (but neither specific assumption is absolutely necessary). So, in effect, R1 defines q in terms of p. Obviously (by choosing appropriate units of account, namely, so that one old unit of account becomes $\lambda(s)$ new units of account at each spot s), in R1 there is no loss of generality in taking $\lambda = (1, 1, \ldots, 1)$, while from both R1 and R2, it must be the case that $A \leqq S$. $A = S$ is the situation with complete markets – in which, by virtue of Arrow's Equivalency Theorem, FE reduces to the familiar Walrasian equilibrium of GE – $A < S$ the situation with incomplete markets – the domain of GEI.

Now focus on the leading case of pure distribution over two periods, where assets are traded at prices q today, at spot $s = 0$, and have uncertain returns $r(s)$ tomorrow, at spots (or, equivalently, in states) $s \in \mathcal{S}$, together with a particularly convenient type of asset structure, what I will refer to as *indexed assets*, where

$$r(s) = r(p(s)) = i(p(s))y(s), s \in \mathcal{S}.$$

Thus, $i : P(1) \to \mathbb{R}_+$ is the index[12], and $Y = [y(s), s \in \mathcal{S}] \in \mathbb{R}^{S,A}$ is the (fixed) specification of asset yields in terms of the index. The leading examples of indexed assets are

- $i(p) = 1, p \in P(1)$, nominal assets,
- for some (fixed) market basket of commodities $n \in \Delta(1)$ (the unit simplex in $\mathbb{R}^C$), $i(p) = pn, p \in P(1)$, numeraire assets, and
- for some (fixed) welfare function $w : \mathbb{R}^C_+ \to \mathbb{R}$ (having standard properties)

$$\begin{aligned} i(p) &= \inf_{w(x) \geqq \underline{w} \text{ and } x \geqq 0} px, p \in P(1) \\ &= \text{the minimum expenditure (in units of account) needed} \\ &\quad \text{to achieve at least the level of welfare } \underline{w} > \min_{x \geqq 0} w(x), \end{aligned}$$

  say, *ideal assets.*

Note that, for this leading case, the mapping R satisfies R1 iff $q = \sum_{s \in \mathcal{S}} r(s)$. In the GEI literature this fact is often taken into account by replacing Mr. h's spot 0 budget constraint with his Walrasian budget constraint at spot goods prices (as I will below).

Indexed assets seem to me to be the most interesting (from an economist's viewpoint) general structure for which R2 obtains naturally (since, without loss of generality, yields can be taken to be linearly independent). For nominal assets, the index is just the unit of account ("money") itself. Numeraire assets represent the abstraction of the CPI (John and Herakles's being the extreme case where the reference market basket is $(1, 0, \ldots, 0)$), while what I have dubbed ideal assets represent what Milton Friedman (must, maybe should have) had in mind in his famous Newsweek column urging the US Treasury to issue an indexed bond. Here, of course, I avoid many conceptual issues by assuming a fictitious invariant representative household with the utility function, or level of welfare w. Notice too that ideal assets imply that a so-called safe asset – the bond paying one unit (of the ideal index) – is truly safe – at least for the representative household.

The key difference between nominal assets on the one side, and numeraire or ideal assets on the other is that, for the latter, the index is linearly homogeneous in spot goods prices. This has the following interpretation, and implications. Ignoring the subtleties involved when $p(s) \not>> 0$, some $s \in \mathcal{S}$, consider rescaling future units of account by $1/i(p(s))$, so that future spot goods prices become $p'(s) = T(p(s)) = p(s)/i(p(s))$, and future asset returns $r'(s) = r(p(s))/i(p(s)) = y(s), s \in \mathcal{S}$. This maneuver, in effect, converts the indexed asset model into a canonical form, the nominal asset model – but with a very crucial qualification. For numeraire or ideal assets, after such a transformation spot goods prices $p'(s)$ are restricted to lie in the lower-dimensional subset

$$T(P(1)) = \{p \in P(1) : i(p) = 1\}$$

for each future spot $s \in \mathcal{S}$. But after having learned of the Cass trick, and accounting for this restriction, we know that the smooth equations describing FE for the two cases share two basic properties: (i) They have a solution for every economy (parameterizing by just endowments, given [fixed] asset yields); and (ii) as with the Walrasian model, they permit only one overall price normalization,[13] but also contain only one redundant equation (using Walras' law together with having replaced Mr. h's spot 0 budget constraint with his Walrasian budget constraint at spot goods prices). This strongly suggests that, in the case of nominal assets, there should typically be significant real indeterminacy. And this is exactly what, generalizing my example in [2], Yves and I in [1] and John and Andreu in [8] demonstrated.[14] Essentially for the same reason we also both clearly demonstrated the likelihood of substantial real indeterminacy – a very significant departure of GEI from traditional GE – whenever future asset returns are not linearly homogeneous in spot prices. Similarly, it now seems plausible to me that, for any indexed assets s.t. $T(P(1))$ contains an open set, there will be an open set of economies exhibiting the same property – but this is just a conjecture. (The trivial generalization is when $T(P(1)) = P(1)$, the model with indexed assets is structurally indistinguishable from the model with nominal assets.)

Finally I want to add two comments: First, concerning extension of my existence argument to several periods,[15] and second, concerning my strongly held view that – in terms of focusing on a particular polar model – the nominal asset (sometimes the numeraire , better yet, the ideal asset) model is, at the very least, equally valuable as the (general) real asset model.

Again concentrating on indexed assets, in the case of nominal assets – as I observed in [3] and Darrell later made explicit in [6] – the generalization to several periods is immediate. This is because the interpretation of spots (or, equivalently, nodes [of a finite date-event tree]) is simply immaterial. That is, it makes no difference whether the entries in the overall return matrix, here effectively reduced to $R(q; Y)$, are asset prices or their yields – they can be any fixed combination satisfying R1 and R2 (ignoring redundant assets as required). On the contrary, in the case of numeraire or ideal assets this extension is more problematic, since if $R(p, q)$ satisfies R1, then rank $R(p, q)$ may vary with spot goods and asset prices (given asset yields).[16] Obviously, there are many other specifications of indexed assets for which, for $(p, q) \in (\text{int } P) \times Q$, rank $R(p, q) = A$. (Again the trivial case is that for which $T(P(1)) = P(1)$).

Regarding indexed asset models: I'm still very fond of the (admittedly very atypical) nominal asset model. As Yves and I wrote in [1], "The yields from many financial instruments [read: assets] are, in fact, best conceived as being determined in their own right [read: specified in units of account]. This is obvious, for example, in the case of debentures and certain forms of insurance. We think it is also a plausible view in the case of common stocks and other assets whose yields depend largely on beliefs about their future values (in particular, beyond the horizon being explicitly considered). In any case, for the purposes of choosing a single, canonical model for representing financial markets in general equilibrium theory, we find Arrow's vision [read: nominal assets] equally as compelling as Debreu's [read: real assets]." (If memory serves well, the editor whom I praised earlier really hated this paragraph!) And it, maybe even more it's truly indexed cousin, the numeraire asset model, is so much more tractable for investigating interesting economic problems![17]

I hope these tidbits have whetted your appetite. Now for the meat and potatoes . . . .

Acknowledgements

I want to thank Bernard Cornet (who is the current Editor of the *JME*) for affording me this unusual opportunity to publish a paper written twenty some-odd years ago, and Paolo Siconolfi (who is my personal guru on matters of existence

[of equilibrium]) for approving the logic of my elaboration on the structure and application of the Cass trick (aka, between us still, the Mr. 1 trick). Stern suggestions by Hervé Cres and Anna Pavlova were also very helpful in forcing me to stay more focused (even, be more kindly). Now all bear at least a bit of responsibility for the outcome.

In the course of preparing this note I asked many of the early contributors to the reincarnation of GE as GEI (I have a suggestion about this [itself incomplete] acronym: Why not, at this stage, better GFE, "general financial equilibrium," better still [as I have frequently done] simply FE?) for their recollections, and received lots of help. Here, of course, none bears any responsibility for how I processed the information they provided. For me, only one real surprise emerged from this process – Michael and Martine's curious treatment of the Cass trick (pp. 82-89 and 135 in [10]). But this is their prerogative, for sure.

Footnotes

2. My wallet contains, at this moment, my Societé Générale debit card, my Citibank Visa card, and the various contact numbers and codes for my Citizens Bank accounts (including one covering so-called overdrafts) – as well as about 40 Euros in paper currency. Why the last? I've never claimed to be completely modern (I also carry a France Telecom pay phone card) – and do find small change handy for buying the IHT, taking taxis, and the like when in Paris.

3. John certainly did me a PR favor – but at a cost. Now, whenever I have a novel take on some problem, it's often referred to (sometimes [without good cause] dismissively) as (just) another Cass trick. Fortunately I think this has not yet put me in an equivalence class with the late President Nixon (who was often [with good cause] referred to as "Tricky Dick").

4. Karl Shell's and my original work [5] was intended to establish the plausibility of sunspot phenomena with minimum technical complication. At that time we believed that the most transparent example involved overlapping cohorts, but cohorts finite in number; we were well aware that our seminal example with overlapping generations (reported by Karl in [11]) introduced an unnecessary element, an open-ended future. The primary reason for this particular example's appeal was that it permitted constructing sunspot equilibria by simply randomizing over certainty equilibria – which, unfortunately, suggested a close connection of our basic idea with multiplicity of certainty equilibria. This despite the further lengthy (likely unread) construction of a counterexample in the appendix to that paper. Incidentally, I should add that that particular construction needs patching up,

there is a reversal of a crucial inequality deep in its workings – my fault, not Karl's! The error can be remedied. But with so many other clear-cut counterexamples – including my own GEI one – appearing soon after we've never seen the need or use for formal correction.

5. Interestingly, John and Herakles's numeraire assets apparently provide essentially the only structure for which, with incomplete financial markets, there are clearly both general existence and generic determinacy. So, if I had myself pursued the question of determinacy further in [3], our papers would have also provided a sharp contrast as well as a nice complementarity. I will have more to say about this important feature of GEI below.

6. Just ask various of my co-authors of the several papers which were finally published ten some-odd years after first being written. Unfortunately, Bernard has now given me yet another excuse for procrastinating – just wait long enough and you don't have to revise!

7. Without my going into great detail, let me just say the following. That paper was written in a very *JPE*-like style because, as I mentioned earlier, it was intended to follow up Karl's and my fundamental contribution. But an especially astute co-editor of that journal flatly rejected it on the thoughtful, nonideological grounds that "We don't believe in sunspots." Incredible! So – as you can plainly see from the reference itself – out of pure disgust, I let the paper just sit, and then several years later simply dumped it off in an obscure outlet I chanced to have available. (Feiwel's collection is apparently so difficult to come by that even John's diligent RA in preparing [7] couldn't get the citation correct, and I know he tried very hard.) The paper deserved much, much better.

8. Without loss of generality this household can be taken to be $h = 1$. For this reason, among those involved with FE hanging around CARESS during the mid-eighties, the Cass trick was known as the "Mr. 1 trick."

9. At the same time, one also simply drops the asset market clearing conditions, which are for all practical purposes redundant anyway (since originally each household faces a budget constraint at each spot [yielding this as well as other useful GEI versions of Walras' law]).

10. I will focus exclusively on the problem of establishing existence where the only demand irregularities occur at the boundary. However, I should mention that, for the same reason (avoiding boundary problems), the Cass trick has also proven extremely valuable where, because of drop in the rank of R, demand irregularities also occur in the interior.

11. For simplicity, I assume that each asset has a single price. Thus, for instance, a bond has a par value, and in equilibrium will have a market price which can be above or below par. But in general an asset can have several "prices" associated with it. Thus, for instance, a call option on commodity 1 will have a market price together with its exercize or strike price. Nothing in principal depends on this simplification (though in practice it's very useful).

12. The index could also be spot-dependent, but with little gain in economic insight. In general, i is required to be at least C^0, and to satisfy at least $i(p) > 0$ for $p \in \text{int}P(1)$.

13. It is easily verified that with this single price normalization the additional equations imposed by having numeraire or ideal assets become

$$i(p(s)) = i(p(1)), s \in \mathcal{S}, s \neq 1,$$

which number only $S - 1$. This plays a role in the analysis described below.

14. In fact, we both determine precise degrees of real indeterminacy, measured by the dimension of subsets of the allocations associated with the FE for a generic economy, under alternative assumptions about variability of asset prices and their yields. Explaining all this is a whole nother (long) story.

15. In this connection, it may be useful to be reminded that, for example, if a T-period bond originally marketed at spot s^t at price $q(s^t)$ can be bought and sold in a subsequent period $t < t' < t + T$, then this is equivalent to having $(T - (t' - t))$-period bonds marketed at spots $s^{t'}$ (the relevant successors to spot s^t) at prices $q(s^{t'})$ with returns identical to those of the original bond at even later relevant successor spots.

16. This potential variability in the rank of R may or may not be generic in Y. For low-dimensional cases (three or four periods and binomial or trinomial trees) it is pretty easy to show that, with just resale of a few long-term assets, there are robust examples for which rank is invariant. This is true, for example, for the same structure (but opposite purpose) Magill and Quinzii employ to exhibit a counterexample to dynamic completeness (pp. 235-238 in [10]).

17. For instance, these very manageable models have been the basis for almost all of the serious analysis of various optimality issues in GEI (as far as I know). I should add that my fascination with atypical systems of equations in FE is unabated, as recent work with Anna Pavlova [4] demonstrates. What Anna and I have dubbed "trees and logs" is just about as nongeneric a real asset (you read right, real asset) model as one could possibly imagine!

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