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The unbearable lightness of the economics-made-fun genre

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Several commentators have argued that the economics-made-fun ('EMF') genre contains very little actual economics. As such, it would seem that criticisms of EMF do not apply to economics more broadly. In this paper I take a contrary view, arguing that, in fact, at a deep conceptual level, the engine of EMF analyses is *precisely* the engine of mainstream economics. Specifically, I argue that both EMF and mainstream economics rest on a conceptual foundation known as the principle of the substitution of similars ('PSS'). Understanding how PSS leads EMF practitioners to make claims well beyond what is warranted by their analysis also offers insight into how PSS can put economists in danger of overestimating the power and scope of their analyses. I explore the consequences of such problems through examples of economic analysis of the US housing market in the lead-up to the recent financial crisis.

Keywords: methodology; popular economics; William Stanley Jevons; ontology; anthropology of finance

JEL Codes: B13; B41; Z13

1 Introduction

In a famous scene from Monty Python's *Holy Grail*, a village wise man named Bedevere is called upon to adjudicate his fellow villagers' claims that a woman they have brought before him is a witch. After some thought and a bit of assistance from King Arthur, Bedevere devises a test and soon the villagers are gathered before an enormous balancing scale containing the woman and a duck, certain that they are about to receive definitive verification or refutation of their conjecture.

For fans of Monty Python's brand of humor, this scene bears the hallmark of what made Monty Python great: the juxtaposition of normalcy and absurdity. It is funny, in part, because the characters are completely committed to the logically sound and patently absurd elements of the situation in equal measure. This both highlights the absurdity and pokes fun at the human tendency to take our favored norms and beliefs, no matter how strange, as self-evidently right and true. When we laugh at Bedevere and the villagers, we are also laughing at our own all-too-human nature.

Lately, there has been a version of this comedic trope propagating through the popular non-fiction bookshelves in the guise of 'economics-made-fun' (EMF) writing. Like Bedevere, its authors claim to be revealing hidden aspects of the universe by interpreting superficial similarities between apparently unlike objects within the framework of a set of absurd assumptions to produce startling conclusions. For Bedevere and the villagers, the fact of the similar weights of a duck and woman means a witch roams among them.

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For Levitt and Dubner (2005), the fact that some sumo wrestlers and some teachers sometimes cheat points to a deep underlying connection between the two.

EMF analyses, though, are marketed not as absurd comedy but rather as mostly straight-faced social analysis. Its practitioners do sometimes present their work as winking or tongue-in-cheek, but not in a sense that is meant to undermine the impression that their conclusions represent genuine and profound insights. For the most part, the work is meant to be a version of real economics and is put forth by unquestionably real economists, some of whom occupy prestigious chairs at elite institutions and regularly publish in the discipline's top journals.

Like many other critics of the EMF genre, I find the characterization of EMF work as even semi-serious social science to be misleading at best. Unlike other critics, however, I will not be taking issue here with the ways in which EMF work falls short of (or completely outside) the standards of contemporary economics.¹ Rather, I will be exploring the possibility that the central analytic principle of EMF work fits quite comfortably within the norms and standards of contemporary mainstream economics, and that by recognizing this we can better understand some of the serious methodological problems within mainstream economics that have led to its current state of turmoil. The analytic principle I have in mind is a particularly extreme (and logically flawed) version of metaphorical explanation called 'the principle of the substitution of similars' (PSS), which was first introduced into economics by the late nineteenth-century economist William Stanley Jevons. Put briefly, the PSS holds that if two objects are even superficially similar, they can be presumed to be identical with respect to certain (arbitrarily selected) essential features. I will argue below that the PSS is an important generator of many of the surprising (and unsupported) conclusions of EMF work, but that it is also a widely used generator of inferences in mainstream economics, with results similar to that which we encounter in EMF analyses. Worryingly, however, the version of PSS used in mainstream economics is generally more sophisticated than that used in EMF work, and so the faultiness of its procedure is more difficult to detect – not least because the currently accepted standards of model assessment in economics are not built to detect it. The result is that we have a strain of rot at the core of our methodology that has been doing damage for a long time, and continues to do so largely unabated. The solution to this problem must begin with identifying the exact nature of the problem to devise a targeted response. Since EMF work uses PSS in an exaggerated manner, it is an ideal laboratory for this exploration.

The paper will proceed as follows. In Section 2, I will discuss the standard analytic trope of EMF analysis and show how it produces ostensibly legitimate but actually unsupported inferences. In Section 3, I will argue that Jevons' PSS bears a strong resemblance to the standard trope of EMF analysis and can be helpful in understanding the relationship between EMF analysis and more rigorous academic economics. In Section 4, I present the more sophisticated version of the PSS used in current mainstream economics and argue – using examples from the literature on the housing market in the lead-up to the financial crisis – that its use can lead to problems similar to those encountered in EMF work. I conclude by suggesting that judicious use of interpretive methods can be used to protect economics against the nefarious influence of the PSS.

2 The standard trope of the economics-made-fun genre

Although the EMF genre has expanded prodigiously since the publication of *Freakonomics* (Levitt and Dubner 2005), the subtitle of that seminal work – *A Rogue*

Economist Explores the Hidden Side of Everything – still captures the genre's ethos quite well. The uncovering of previously hidden truths using economics in an unexpected way is the essence of the genre's self-understanding. I will argue in this section, however, that close inspection of EMF analyses reveals that the conclusions reached are generally not hidden truths uncovered by economic reasoning, but rather rational reconstructions of the phenomena under study that *assume* a deep structure (a.k.a. hidden order) binding the phenomena together. Put another way, EMF authors are not discovering hidden truths, but rather 'discovering' their own assumptions and reporting them as surprising insights.

The *Freakonomics* chapter 'What Do Schoolteachers and Sumo Wrestlers Have in Common?' provides a good example. The chapter is ostensibly about cheating. In it, the authors present a very broad picture of cheating – including everything from steroid use by athletes to a third grader copying from another student – and then recount several stories that are intended to be understood as instances of cheating. The story of the cheating teachers centers around the Chicago public school system's experience with 'high-stakes' testing – i.e., standardized tests on which students must achieve a passing score to advance to the next grade level, and for which teachers are rewarded for good performance in their classrooms. In a study of several years of student test scores (and of a re-test of a smaller sample of classrooms), Steven Levitt and his co-author Brian Jacob found evidence that some teachers had altered students' test responses after the fact to raise their scores (Jacob and Levitt 2003a,b). The story of the sumo wrestlers centers around some seemingly suspicious trends in match performance. Specifically, Levitt and his co-author Mark Duggan examined matches in which a win was extremely important for one wrestler but not for the other, and found that the wrestlers in need of a win performed much better in those matches than they did under similar circumstances in non-crucial matches (Duggan and Levitt 2002).

Levitt and Dubner suggest that the facts reviewed above are evidence that teachers and sumo wrestlers have something in common.² Before examining this claim in detail, we need first to make it more explicit. To dispense with a minimal reading of the title, we can simply note that there is no question that sumo wrestlers and teachers have *something* in common. Any two things have *something* in common.³ But the authors' suggestion is clearly something more specific and contentious than this. As the stated intent of their book is to 'explore[] the hidden side of everything,' it seems appropriate to interpret the title of the chapter as a claim that there is a hidden connection between sumo wrestlers and teachers that the material covered in the chapter will expose.

To understand what would be necessary to make such a claim plausible, it is helpful to consider a literal reading of the stories and then trace the moves required to transform this reading into evidence of a hidden connection. What the stories indicate, literally, is that some members of a group of Chicago public school teachers and some sumo wrestlers, under a specific set of conditions, engaged in behavior that the authors believe was a violation of a set of rules the agents recognized as applying to them. If we give the authors the benefit of the doubt regarding their claims of cheating, then we could say that some Chicago teachers and some sumo wrestlers cheated under the specific circumstances reviewed in the stories.⁴ The connection, so far, is a superficial similarity between an action taken by some of the wrestlers and some of the teachers.

The authors' transformation of this superficial connection between actions into a deep, hidden connection between people unfolds in two stages. First, they recast cheating (within whatever context it is encountered) as a uniform phenomenon: namely, as an outcome of incentive processing. The substance of the recasting involves first pointing out how important incentives are to our decision making, next describing various examples of

behavior that would be commonly understood as cheating using the vocabulary of incentives, and finally concluding that '[c]heating is a primordial economic act: getting more for less' (Levitt and Dubner 2005, p. 25). This immediately changes the superficial similarity between the sumo wrestlers' and teachers' cheating into a deeper one: they are not merely engaging in activities that share some attributes, but, rather, are doing *the same thing*. To differentiate this meaning of cheating from the colloquial meaning (which is more capacious and nuanced), I will call the former 'freakcheating.'

The second stage in the transformation involves recasting freakcheating as the result of 'modular incentive processing activities' (this is my term, not Levitt and Dubner's). What I mean by this will be most easily explained by tracing out what Levitt and Dubner do to connect the uniform act of cheating to a kind of uniformity among cheaters. The first thing to note is that the uniformity of the act of freakcheating is immediately tied to a uniformity of internal process: it involves the processing of incentives by the individuals generating the activities. So the fact that wrestlers and teachers freakcheat tells us not merely that they have done the same thing, but that they have both done so as a result of the same type of action: processing incentives. But this is still not enough to support the claim of a deep, hidden similarity between the wrestlers and the teachers. For that we need the incentive processing activity undertaken by the two groups to be deeply similar as well – otherwise the appearance of freakcheating in both contexts could just have been coincidence. The authors do not explicitly argue for this last piece of the puzzle, leaving the reader to make the connection him/herself (or to reject it). But for those who are familiar with economic reasoning, the implication is fairly clear: if one holds all other differences equal, one can see that the offending teachers and offending wrestlers identically process similar circumstances to produce freakcheating. And this is where the notion of modularity is important. One way to understand this final step (and, I would suggest, the way that best fits Levitt and Dubner's meaning) is to imagine the wrestlers and teachers as bundles of incentive processing modules, where the actions of each module are analytically separable from all others. As such, although the teachers and wrestlers differ in many ways, and although their contexts differ in many ways, they share a particular element whose functioning manifests itself empirically if only we understand how to recognize it. This would allow us to interpret their freakcheating as evidence that each contains the module that processes similar antecedent freakcheating factors (i.e., high-stakes testing and crucial matches, respectively) identically. And this would, indeed, indicate that we had uncovered a previously hidden substantial connection.

In the foregoing description, for ease of exposition, I began with freakcheating and ended with the ontological position regarding action as the resultant of modular incentive processors. This is the order in which the elements of Levitt and Dubner's analytic process unfold to the reader. But from the point of view of the practitioner, the process unfolds in the opposite direction. Levitt and Dubner began with an ontological presumption, and it was within that ontology that their observations of the Chicago teachers and Sumo wrestlers acquired the meanings proposed in the chapter. This process can be stated in general form as follows:

- (1) Begin with an unconventional ontological framework (e.g., the world is populated by a complex of separable incentives, incentive processing modules and resultants of this processing called 'actions').
- (2) Reinterpret observed phenomena within the interpretive framework of that ontology (e.g., superficially similar actions are identical types that are the result of identical incentive processing modules acting in different contexts).

- (3) Claim that this reinterpretation is evidence of the sort of deep connection envisioned in one's presumed ontology (e.g., the observation of cheating among teachers and sumo wrestlers is evidence of a deep similarity between them).

This explanatory strategy is an essential and pervasive trope of EMF work. Some of the other examples of the employment of this strategy are obvious, such as the *Freakonomics* chapter 'How is the KKK Like a Group of Real Estate Agents?' Using the same ontological framework as in the teacher–sumo wrestler chapter, the authors interpret their observation of superficially similar use of private information by Real Estate agents and the Ku Klux Klan as evidence of a deep connection between them. But it is not only these straightforward comparisons of types of people that follow the trope delineated above. In fact, virtually all EMF work employs this trope.

Take, for example, Tyler Cowen's (2008) *Discover Your Inner Economist: Use Incentives to Fall in Love, Survive Your Next Meeting, and Motivate Your Dentist*.⁵ Although the book's chapters deal with a wide range of contexts and questions, the central message of the book is that there is something essentially identical about one's actions across all these contexts – namely, that one is processing incentives and that if one wants to do so properly, it helps to recognize that a generic processing module lies within the panoply of apparent diversity across all these contexts. The central metaphor that Cowen chooses for the book – one's 'inner economist' – is especially telling in this regard. The inner economist is the generic incentive processing module operating within each of us. And it is important to note that this metaphor must carry its home world along with it – i.e., that, as in the *Freakonomics* examples cited above, the deep similarities ostensibly discovered will only appear if one projects the imagined world of modular incentive processing units onto the world we actually experience. The idea of a generic incentive processing module within each of us is only intelligible within such an imagined world.

This explanatory trope has been a smashing success for EMF authors, judging not only by book sales, but also by the extent to which EMF thinking has become a 'meme' in popular culture and even an academic teaching tool.⁶ But there are significant problems with the EMF trope, and to the extent that EMF authors want their work to be understood as semi-serious economics it is important to make these problems clear. DiNardo (2007) and Rubinstein (2006) have provided ample evidence of some of the methodological shortcomings of EMF work. DiNardo (2007), in particular, casts enough doubt on the claims of *Freakonomics* to place the burden of proof squarely on the shoulders of anyone who would claim that the claims should be understood as scientific results.

But there is an additional reason to question the legitimacy of EMF work that would remain even if the authors addressed the methodological issues identified by DiNardo, namely, that the central explanatory trope of EMF work is inherently question-begging. As discussed above, the observations that EMF authors present as evidence of deep connections between disparate phenomena could actually only count as such evidence *if we already presume the deep connections to exist*. As such, the evidence they adduce is incapable of answering the larger question of whether or not their claims are valid. For example, with respect to the teacher–sumo wrestler chapter in *Freakonomics*, the authors' empirical analyses that were meant to test whether or not the teachers and wrestlers were cheating could only be considered evidence of a deep connection between the two groups if we had already accepted the ontological presumption that turns these instances of cheating into freakcheating. This is why what the authors are actually doing is *not* discovering a previously hidden connection, but rather 'discovering' their initial assumptions and reporting them as such a discovery. And this is a general feature of EMF

work: the surprising conclusions the authors reach are already contained in their assumptions, and the evidence they adduce to support the conclusions are only evidence of hidden connections if the assumptions are correct.

In this sense, EMF work bears a disturbingly strong resemblance to Bedevere's witch test. Bedevere claims that his proposed test – balancing the woman the villagers have brought to him against a duck – will reveal whether or not the woman is a witch. But seeing the results of the test as evidence of witchness (or lack thereof) only makes sense against the background of a set of ontological assumptions that include, *inter alia*, the existence of witches for whom floating in water is a constitutive property. We moderns can see that the villagers' focus on the balancing scales is misplaced. The ultimate answer to their question lies elsewhere – i.e., in an exploration of the question of the existence of witches – and that the testing methodology they have chosen is not equipped to produce the kind of information they need. They may come up with the right answer, but it will be for the wrong reason.

It is this latter problem of EMF analysis that has the most troubling implications. Unlike the problems identified by DiNardo and Rubinstein – the elucidation of which serves to separate contemporary economics from EMF – the problem I have raised is actually something that EMF *shares* with contemporary economics. Specifically, the philosophical justification for using the EMF trope as a means of (ostensibly) uncovering hidden patterns in the world is one that was originally also a part of the philosophical foundation of contemporary economics. This justification, the PSS, was the brainchild of William Stanley Jevons, and it is more than a mere historical curiosity. As I will argue in Section 4, it is an erroneous principle that not only allows EMF work to masquerade as semi-serious economics, but also provides cover for certain well-accepted methodological strategies within academic economics that actually possess the same essential flaw as EMF work. Before turning to this argument, however, it is necessary to give a brief review of Jevons' principle.

3 The principle of the substitution of similars

William Stanley Jevons is known to most contemporary economists as a member of the triumvirate of the marginalist revolution (along with Léon Walras and Carl Menger), but by his own reckoning his greatest contribution to knowledge came in the field of logic. The contribution, specifically, was the PSS, and it provided the analytic core of all of Jevons' scientific endeavors, including his seminal *Theory of Political Economy* (1871). Significantly, for the purposes of this paper, its imprint is still discernible today as an important part of the logic underpinning both EMF work and certain well-established methodological strategies in mainstream academic economics.

The PSS arose out of Jevons' engagement with mathematics and logic – a passion that predated by many years his interest in political economy. Although his university training centered around natural science, it was Jevons' studies in mathematics and logic with Augustus De Morgan that had the most profound impact on his scientific practice. In the late 1850s, Jevons began producing his first independent academic work – studies of cloud formation. In addition to yielding his first publications, this work also allowed Jevons to reflect concretely on the nature of scientific discovery and its relationship to methodology. He became convinced that the key to all scientific discovery was the recognition of similarities in apparently disparate objects – specifically, that when a heretofore obscure phenomenon is recognized as similar to a well-understood phenomenon, we may project what is known of the latter onto the former. Jevons felt that there was something very deep

about this idea, and that although in some sense it was a commonplace its implications had not yet been fully explored. Further, he had a sense of how such exploration might be possible, and it hinged on an innovation in logic. On New Years' Eve, 1862, Jevons wrote in his journal, 'my logical speculations give me most confidence. I cannot disbelieve, yet I can hardly believe that in the principle of *sameness* I have found that which will reduce the whole theory of reasoning to one consistent lucid process' (Black and Könekamp 1972, p. 186).

Jevons' key logical innovation was to tighten the relationship between subject and predicate, specifically by replacing the standardly used copula – some form of the verb 'to be' – with the mathematical symbol ' $=$.' For Jevons, the standard copula was too ambiguous (1958, p. 16). It signaled only that the subject was included in a class of things denoted by the predicate. And this relationship could imply many different things, which made it a complicated matter to determine what precisely could be deduced from a series of statements expressed with the standard copula. Jevons believed (building on the innovations of George Bentham and George Boole) that by specifying precisely the part of the predicate to which the subject exactly agreed, one could express statements like 'A is B' as ' $A = [\text{some subset or aspect of B}]$.'⁷ The great advantage of this was that one could then use the rules of algebra to work out the implications of statements. All the complicated rules of syllogism in Aristotelian logic could thereby be done away with (Jevons 1869, p. 25).

As a practical matter, what Jevons was aiming for was a way to more accurately express his view regarding the power of deep similarity. Using the ' $=$ ' symbol was a way of formalizing Jevons' belief that if one could establish a sufficient degree of similarity between two objects, then one ought to be able to conclude that *everything* that was true of the one would be true of the other – or, as he put it in his definition of the PSS, that a 'capacity of mutual replacement exist[s] in any two objects which are like or equivalent to a sufficient degree' (1958, p. 17).

An example from an early exposition of the PSS demonstrates how Jevons imagined the PSS would work in practice. Taking Nassau Senior's definition of wealth as the subject to be explored, he writes:

Sometimes we may have two definitions of the same term, and we may then equate these to each other. Thus, according to Mr. Senior,

- (1) Wealth = whatever has exchangeable value.
- (2) Wealth = whatever is useful, transferable and limited in supply.

We can employ either of these to make a substitution in the other, obtaining the equation,

Whatever has exchangeable value = whatever is useful, transferable, and limited in supply.

(Jevons 1869, pp. 25–26)

The concluding statement follows from the preceding ones in precisely the same manner and for precisely the same reasons that ' $y = 3$ ' would follow from the statements ' $x = y$ ' and ' $x = 3$.'

The primary problem with the PSS, as many of Jevons' contemporaries noted, is that transferring the notion of mathematical equality to a non-mathematical context is no simple matter. The fact that it is possible – in a purely mechanical sense – to *write* a statement in which two non-mathematical entities are joined by the ' $=$ ' symbol does not entail that such a relationship is possible. At the very least, the intelligibility of such an expression would require the espousal of a radical social ontology – i.e., that either the social world is underlain with mathematical structure or that any non-mathematically structured elements are isolable from the mathematically structured elements. This would

not be necessary if the ‘=’ were understood more loosely, for instance by being interpreted as equivalent to ordinary language expressions like ‘is similar to’ or ‘is the same as.’ In this case, there would be no problem in interpreting sentences like the ones from Jevons’ PSS example above. But Jevons intended to preserve the mathematical sense of the ‘=’ symbol – indeed, this preservation of the mathematical sense was absolutely crucial to his goal of importing the algebraic operation of substitution into logic.⁸

Given the importance of providing some kind of philosophical grounding for the possibility of mathematical equivalence in non-mathematical settings, it is surprising that Jevons devoted nearly no space to the issue in *The Principles of Science*, focusing instead on the construction of a system to work out the logical implications of such similarity *assuming* that it was possible. To many of Jevons’ contemporaries, this was simply question-begging, as they pointed out in reviews of *The Principles of Science* and in letters to Jevons. The eminent English scientist George Herschel, for example, expressed this objection trenchantly in an 1869 letter to Jevons:

And then, after all, the difficulty of reasoning correctly lies not in the mechanical application of logical formulae ... but in the application of reason and observation to decide what things *are* similar: *so* similar as to admit of substitution for each other in the argument at hand; which is not a province of formal or Aristotelian logic, however largely supplemented by Dr Boole, Dr Thomson or yourself. (Cited in Maas 2005, p. 148)⁹

And Herschel was not alone in these concerns – John Stuart Mill and George Boole were also critical of this aspect of Jevons’ philosophy of science.¹⁰ And even Jevons’ teacher and mentor Augustus De Morgan remained unconvinced, declining to adopt the replacement of the standard copula with the ‘=’ symbol despite the fact that he, himself, was an early innovator in working out the connections between mathematics and logic.¹¹

The reason for Jevons’ relative neglect of this issue was not that he disagreed with these critics that it was important, but rather that he thought that it could be addressed quite straightforwardly. Specifically, Jevons held that all phenomena possess logical structure – which, because of Jevons’ conflation of mathematics and logic, was equivalent to the position that all phenomena capable of varying in degree possess mathematical structure. And significantly for the purposes of this paper, this position was not an empirical finding or a position which Jevons supported with argument, but rather simply an article of faith.

We can see this by looking closely at Jevons’ exposition of this position. He begins by proffering what he considers to be the fundamental laws of logic – i.e., ‘the laws which express the very nature and conditions of the discriminating and identifying powers of the mind’:

1. The Law of Identity. *Whatever is, is.*
2. The Law of Contradiction. *A thing cannot both be and not be.*
3. The Law of Duality. *A thing must either be or not be.* (Jevons 1958, p. 5)

Next, he makes the radical inference that these laws are not merely laws of thought, but further are an expression of the structure of all existence. Significantly, he does so not through rigorous argumentation, but through a dialog with an imagined interlocutor:

Are not the Laws of Identity and Difference the prior conditions of all consciousness and all existence? Must they not hold true, alike of things material and immaterial? and [*sic*] if so can we say that they are only subjectively true or objectively true? I am inclined, in short, to regard them as true both ‘in the nature of thought and things,’ ... and I hold that they belong to the common basis of all existence. (Jevons 1958, p. 8)

And this helps us to make sense of the following general description of phenomena that appears several pages earlier:

In the material framework of this world, substances and forces present themselves in definite and stable combinations . . . The constituents of the globe, indeed, appear in almost endless combinations; but each combination bears its fixed character, and when resolved is found to be the compound of definite substances. (Jevons 1958, p. 2)

From the previous quotation, we can see that the ‘framework’ of which Jevons writes above encompasses *all* things, material and immaterial. What Jevons is expressing in these passages is nothing less than a thoroughgoing rationalist ontology.

Jevons does not provide an account of *why* he holds this ontological position, but the fact that he does hold it takes us a long way in understanding why he would have considered the PSS to be well founded despite his critics’ misgivings. Herschel’s concern that Jevons had not paid sufficient attention to the question of how one determines *what* things are similar is addressed by Jevons’ ontological assumptions. For Jevons, *all* things are deeply similar structurally; specifically, they are modular complexes of definite substances. And this is precisely the kind of similarity we need to get the principle behind the PSS off the ground. Based solely on this, we need not worry if statements like ‘Wealth = whatever is useful, transferable and limited in supply’ are *intelligible*, but rather only whether or not – *given that they are of an appropriate form* – they are correct.

We are now prepared to see the deep parallels between the PSS and the standard trope of EMF work discussed above. As with the EMF trope, the PSS involves beginning with an unconventional ontological position and reinterpreting observed phenomena within the interpretive framework of that ontology, and, once one successfully ‘tests’ the description against a relevant set of data, claiming that the reinterpretation is evidence of the sort of deep connection envisioned by the presumed ontology. But just as with the EMF trope, the test that ostensibly vindicates the proposition of a deep connection between the phenomena under study will be question-begging – specifically, it will be a test conducted entirely within the presumed ontology, will never touch the question of the plausibility of the ontology and will end only in ‘discovering’ one’s initial assumptions.

We can see this by turning to another example - Jevons’ application of his logic to the following statement made by Augustus De Morgan: ‘He must have been rich, and if not absolutely mad was weakness itself, subjected either to bad advice or to most unfavourable circumstances.’¹² Jevons claims that the statement can be equivalently understood as follows (note that the symbol $\cdot | \cdot$ is, roughly, Jevons’ representation of ‘and/or’):

If we assign the letters of the alphabet in succession, thus,

A = he
B = rich
C = absolutely mad
D = weakness itself
E = subjected to bad advice
F = subjected to most unfavorable circumstances,

the proposition will take the form

$$A = AB\{C \cdot | \cdot D(E \cdot | \cdot F)\} \text{ (1958, p. 76)}$$

This is a decomposition of the subject of De Morgan’s sentence into a modular complex of attributes. To the extent that such attributes are also observed in other subjects, the latter’s decomposition would include the same letters which, by Jevons’ account, would not only have to mean precisely the same thing, but would also have to be associated with the subjects in precisely the same way. Whatever else might be true of disparate subjects

containing some identical attribute, they are all deeply connected by being associated with that identical attribute in an identical manner. This is precisely what Levitt and Dubner implicitly claim by interpreting the cheating of teachers and sumo wrestlers as freakcheating generated by an identical incentive processing module, and it is logically unsound for precisely the same reason. The ‘discovery’ of that deep connection was not established through observation, but rather was something that was embedded in the analysis by assumption.

4 The PSS and contemporary academic economics

Unfortunately, the logic of the PSS underpins not only EMF analysis, but also some ostensibly legitimate analytic strategies that are pervasive in contemporary academic economics. In brief, the principle that the only valid test of a hypothesis is its empirical success (and, therefore, that the realisticness *per se* of the hypothesis is irrelevant) allows EMF-type methods to masquerade as legitimate methods by obscuring their illegitimacy through inadequate testing procedures. In this section, I will argue for this claim and provide as support the example of academic economic writing on the housing market in the lead-up to the recent financial crisis. I will proceed by first discussing (1) the relation between PSS and contemporary economic methodology, and (2) the safety net that is supposed to prevent the PSS from causing EMF- and Bedevere-type problems. I will then argue that the safety net is porous in that it is insensitive to certain kinds of important problems for the same reason that the standard EMF trope reviewed above is logically flawed.

The philosophical framework of the PSS is present in contemporary economics on two levels – first, with respect to ontological presuppositions and second, with respect to the manner in which these presuppositions are ostensibly rendered harmless. To what extent contemporary economics is committed to a rationalist ontology is a matter of controversy.¹³ The discipline’s methodological and epistemological commitment to mathematical modeling, however, is clear. Economic analysis of social phenomena today *means* creating a mathematical representation (implicitly or explicitly, though usually explicitly) of the phenomena as a means of proposing and testing possible explanations of the nature and dynamics of those phenomena. Economic knowledge is the product of proper application of this methodology. As such, a milder form of the Jevonian ontological position is implicitly espoused by contemporary economics, at least provisionally – namely, that mathematical representations of (any) social phenomena are a potentially useful explanatory mechanism for those phenomena and, further, that explaining social phenomena *only and always* through mathematical representation does not limit the potential scope of one’s social explorations. A rationalist ontology is a sufficient grounding for such a belief, though not a necessary one. Still, although a practicing economist can be ontologically agnostic to some extent, that agnosticism must be bounded to exclude the position that the phenomena of the social world are *definitely not* rationally structured. Such a position would be directly at odds with the standard practice of carrying only mathematical tools in one’s toolkit.

What is supposed to keep this ontological commitment from being a rationalist blemish on the economist’s pure empiricism is the principle that the only relevant thing about a hypothesis is its agreement with empirical observation (and not, therefore, its origin). Milton Friedman’s 1953 essay provided the canonical statement within economics of this principle, but Jevons had anticipated Friedman’s position in 1874, stating quite clearly in the first edition of *The Principles of Science* that ‘[a]greement with fact is the

one sole and sufficient test of a true hypothesis' (1874, p. 138, italics in original).¹⁴ This principle is meant to provide protection against the kinds of problems that the ontological elements of PSS thinking could otherwise cause. Unlike in much EMF work, in proper science (the thought goes) one cannot view deductions made from within one's ontological presumptions as results in themselves. Rather, they must be treated as hypotheses to be rigorously tested and to the extent they agree with empirical observation the presumed ontology can be treated merely as a creative source of theory. In effect, the ontological presumption is rendered invisible.

The central problem with this position is succinctly articulated by the two prongs of the Duhem-Quine thesis.¹⁵ First, since one can never test hypotheses in isolation, any hypothesis test is actually a test of the hypothesis along with all of its framing assumptions. Second, the test itself is never a comparison of the hypothesis with brute facts of nature, but, rather, a comparison of the former with empirical observations that have already been parsed into the categories of the theory. Put another way, the test must occur entirely *within* the presumed ontology and, as such, is merely a test of whether an internally consistent rational reconstruction of empirical observations is possible. What is left aside is the question of whether the presumed ontology itself is plausible, and this is the door through which EMF-type problems can enter into contemporary academic economic practice.

To explain precisely how this happens, I need to be a bit more precise about the current model assessment standards of contemporary economics. The basic tenets can be distilled into three principles:

- (1) *The empirical consequences principle (ECP)*: The only real kind of problem that an economic model can have is one that has empirical consequences – e.g., it makes bad predictions.
- (2) *The econometrics sufficiency principle (ESP)*: All modeling problems with empirical consequences – i.e., all *real* modeling problems – can in principle be detected econometrically.
- (3) *The Friedman Principle (FP)*: Economists need recognize no constraints in their model creation besides the ECP and ESP.

This statement of the model assessment standards is just an expansion of the Jevons–Friedman standard cited above to include the type of testing that is considered necessary and sufficient within contemporary economics – namely, econometric methods.

What is left out in these standards is a recognition of the role of ontological background assumptions not only as creative sources of hypotheses, but also as the scaffolding within which one's data is created and one's empirical testing occurs.¹⁶ Ignoring this role puts one in danger of mistakenly endorsing explanations that take superficial similarities to be evidence of deeper, more essential connections. The problem is that, based on the model assessment standards above, we have no way of verifying whether such deep connections actually exist, and if they do, whether the particular deep connection we've projected onto the data is the right one. In his essay 'Is a Science of Comparative Possible?' Alasdair MacIntyre put the point cogently with respect to positive political science work:

[I]f we identify behavior except in terms of the intentions and therefore of the beliefs of the agents we shall risk describing what they are doing as what we would be doing if we went through that series of movements or something like it rather than what they are actually doing. Nor do we avoid this difficulty merely by finding *some* description of the behavior in question which both the agents themselves and the political scientist would accept. (1978, p. 264)

As such, using the ECP–ESP–FP complex as one’s assessment standards casts a shadow over one’s conclusions, regardless of whether or not they have met those standards. By extension, if these are used as the assessment standards for an entire discipline, then that discipline will have an *a priori* credibility problem with respect to any of its results.¹⁷

This is not merely a theoretical problem. If one’s model assessment standards leave one blind to important misunderstandings, then empirical consequences that are missed by one’s empirical testing procedures may end up making their first appearance as live problems in actual reality that one has failed to anticipate. This could manifest itself, for example, as a realization of a particular event that is well outside of one’s predictions, or as the failure of a policy designed on the basis of one’s models. These problems can occur because of modeling problems that are within the province of the ECP–ESP–FP complex, but can just as easily occur for reasons outside it.

The recent global financial crisis and its relationship to the US housing market provides one concrete example of the types of things that can be missed by the ECP–ESP–FP complex and the consequences that can result. As we now know, the central cause of the financial crisis was the concentration of investment in the US housing market, particularly through derivative securities (e.g., credit default swaps and various forms of collateralized debt obligations and mortgage backed securities). The massive inflow of investment during the late 1990s and throughout most of the 2000s inflated a bubble in US (and eventually non-US) housing and related assets that unraveled and burst when, in 2007, housing prices began to level off and mortgage defaults began to rise. When the bubble had burst and losses began to be realized, it became clear that the use of opaque derivatives on a massive scale to take and hedge positions in the mortgage-related asset market had intertwined the fate of a large portion of the international financial industry.

What has also become clear is that this was no simple speculative bubble. Rather, it was a bubble created and sustained in part by a politico-commercio-regulatory (hereinafter ‘PCR’) environment created by, *inter alia*, private financial institutions, legislators, the Federal Reserve and the Treasury department. Among the primary elements of this environment were regulatory decisions that allowed OTC derivatives markets such as those for credit default swaps to remain non-standardized and opaque; a problematic framework for relations between credit rating agencies and their clients that encouraged misleading ratings of complex mortgage derivative products; severe moral hazard problems on the part of mortgage originators caused by massive demand for securitization of mortgages; and the spread of accounting and compensation practices that encouraged short-term thinking and high degrees of leverage in the creation and marketing of highly complex structured finance products. Although presumably it was not the intent of the agents involved in creating this environment to cause a financial crisis, there *was* a clear intent to remove barriers to highly leveraged investment strategies using lightly regulated and unregulated derivatives.¹⁸ One result of this was that the US housing market in the late 1990s and 2000s did not conform to the standard economic picture of a housing market – i.e., it was not *simply* a market for an asset delivering a certain type of service with attendant financing issues analyzable through the standard model of financial assets. Rather, it was a part of a PCR gambit and, significantly, this characteristic was *constitutive* of the late 1990s–2000s US housing market in the sense that failing to understand this fact meant failing to understand the phenomenon.

The unprecedented rise in US housing prices in the 2000s was apparent to economists concerned with such issues, and not surprisingly many papers were written on the subject. What *is* surprising is the almost complete lack of connection in the academic economic literature between the dynamics of the housing market, the activity in the related securities

markets and the political and regulatory environment that was fueling the interplay between the two. A review of six of the discipline's most prestigious generalist journals and three specialist journals related to housing, banking and finance from 2003 to 2008 turns up no papers that explicitly sought to connect these dots.^{19,20} Faccio, Masulis and McConnell (2006) and Schneider and Tornell (2004), two papers that explore the link between political connections and bank bailouts, are possible exceptions, though their focus is largely or entirely non-US and they do not address the specific dynamics of the PCR environment (in the USA or elsewhere). It is significant that this relative silence on the matter from academic economists extends well into 2008 when the crisis had already begun to unfold and journalistic accounts of the connections between the housing market, derivatives market and deregulation were being minutely covered in the financial press.

How should we interpret this? Was this lack of focus on the PCR environment a failing? If so, what kind of failing? If not, is this for ontological, epistemological or methodological reasons? – i.e., was it acceptable to ignore these factors because they are not a part of the economic universe; because, although they are a part of this universe it is impossible to detect them *ex ante*; or because although they are a part of the universe and detectable *ex ante*, economists need not take account of them directly to achieve their (and the discipline's) illuminatory goals?

The basic premise of these questions – i.e., that the discipline may have been blind to factors crucial to the financial crisis – has been debated voluminously in various post-mortem panels, symposia, hearings, academic articles, journalistic articles and opinion pieces. Any kind of comprehensive account of these debates is well beyond the scope of this paper, and I will not attempt one here. Rather, in the remainder of this section, I will briefly put forth and defend one possible answer to these questions – namely, that the neglect of the PCR environment was indeed a failing, and, moreover, was an example of the kind of problem reviewed above (i.e., of blind spots resulting from the confluence of PSS theoretical foundations and ECP–ESP–FP assessment methodology).

In general, what we find in the mainstream literature in the lead-up to the crisis are analyses of the housing market and its attendant financial markets from within the perspective of existing economic models. As such, there is an implicit presumption that the particular markets and institutions under study are not substantially different in nature from other such markets and institutions that have been the subject of similar models. For example, with respect to the behavior of housing prices, we generally see papers examining the impact of specific individual factors in isolation from broader consideration of the broader PCR environment. Several papers addressed the narrow question of whether or not there was a bubble in housing prices by regressing prices against 'fundamental' factors and exploring the extent to which the residual contained a substantial amount of the variation (and also, in some cases, gauging the psychology of home buyer expectations through surveys).²¹ Other papers examined the role of land use and construction regulation on home prices.²² Many papers explored the question of house price dynamics in the abstract by developing general models. Some of these did integrate financial and policy factors – e.g., collateral requirements, borrowing constraints, the secondary mortgage market, monetary policy – but none made specific reference to the peculiar PCR environment of the 2000s market in particular.²³ Another group of papers examined the risk in the financial system in general, with several focusing on the role of liquidity in banking crises and others focusing on other issues in bank risk.²⁴ But, again, none of these incorporated wider PCR factors.²⁵ As PCR factors were not included in the models and econometric specifications, they also were not explicitly included in the data used to test the models. Consequently, the generation of hypotheses and the assessment of those

hypotheses were completely orthogonal to PCR considerations. The methodology, then, created a blind spot with respect to the effects of PCR factors: even if such factors had significant empirical consequences, these consequences would have been attributed to some other factor that existed within the ontology of the model.

In arguing that PCR considerations were absent from these analyses, I am not claiming that economists would have predicted the financial crisis if only they had taken the PCR into account. My claim is that a class of relevant information was systematically excluded from these analyses *not* by a conscious and informed choice, but *implicitly*, because it lay outside of the models' presumed ontology. This is not to say (because it would not be true) that the PCR environment was invisible to economists. (Consider, for example, Raghuram Rajan's (2005) explicit warning, at the 2005 Fed symposium at Jackson Hole, that the confluence of regulatory, technological and culture changes in the financial industry posed a threat to the economy.) The problem is not that economists *qua individuals* did not recognize these factors, but that economic methodology allowed such factors to remain invisible to them *qua economists*.

The way to address this kind of systematic exclusion is to reform the methodology to make it sensitive to features that are empirically important, but may be outside of what is currently considered economically relevant. This suggestion is in keeping with the general tenor of many of the reform proposals that have emerged from the numerous recent post-mortem analyses of economics' performance in the lead-up to the financial crisis – for example, those calling for more agent-based modeling or a deepening of the behavioral content of models to recognize currently excluded complexity; or those calling for changes in the institutional structure and/or culture of academic economics to encourage a more diverse discourse.²⁶ To address the kinds of concerns I have discussed above, however, the necessary methodological reform would be more substantial. To be able to assess the plausibility of one's ontological presumptions, it is necessary to have methods for discovering the nature of one's subject matter 'from the bottom up' – that is, to allow for the subject matter itself to reveal its nature, even if that nature is different from or even contrary to that presumed by one's existing models.²⁷

To accomplish this, economics would need to incorporate research methods that elicit a more fine-grained portrait of empirical reality. Whether or not this would require major methodological innovation is up for debate. We have examples both within and without the discipline of what such work might look like. Within the discipline, for example, there are calls for such reform along the lines of fine-grained institutional economics (Hodgson 2008, 2009), Post-Keynesian economics (Davidson 2009; Leijonhufvud 2009; Taylor 2010) and critical realist economics (Lawson 2009). And Bewley (1999) provides a concrete example of fine-grained economics specifically tailored to enhance our approach to macroeconomics. From outside the discipline, we have examples of what a more ethnographic style of economic inquiry might look like from recent work in the anthropology of finance.²⁸ While interpretive methods are not a panacea, and their integration into economics will require considerable effort, I would argue they have the potential to play an important role in covering the blind spots of current economic methodology.

5 Conclusion

The EMF genre of economic writing has been both commercially successful and influential in popular discourse. Careful examination of one of its central explanatory tropes, however, reveals that it is fundamentally misleading. Rather than revealing a

hidden structure of everyday life, it presumes that structure, interprets empirical reality from within the framework of that presumed structure and takes this reinterpretation as evidence of the existence of the presumed structures.

Many critics of EMF writing portray it as a bastardized form of economics, implying that its methodological problems are not relevant to mainstream academic economic work. I have argued, on the contrary, that EMF and ostensibly legitimate economic work share a common root in William Stanley Jevons' PSS, and that this common root has infected legitimate economics with some of the same problems bedeviling EMF. Specifically, the PSS allows ontological presuppositions to masquerade as empirically established economic facts. Although academic economics includes rigorous testing standards that ostensibly insulate it from this problem, I have argued that these standards are porous enough not only to let the problem persist, but also to render it invisible. The most direct way to address this problem is to introduce methods that can shine a light upon and directly assess the validity of these ontological presuppositions. Although such methods are not currently in use in mainstream economics, they are a standard tool in the anthropological toolkit, and have been put to good use in exploring, among other things, the nature of financial markets. Because of the seriousness of the lapses that PSS-type problems can cause, it would be beneficial for economists to explore the integration of such anthropological methods into standard economic practice.

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Notes

1. This point has been made most forcefully and comprehensively by DiNardo (2007). Rubinstein (2006) offers a brief critique in a similar vein.
2. This is suggested by the title of their chapter, though they do not explicitly state this claim anywhere in the chapter.
3. For example, sneezes and the Crimean War share the common trait that both can be the subjects of sentences.
4. This is not a trivial concession, and DiNardo (2007) offers compelling reasons against making it (with respect to the finding of teacher cheating as well as many of the other findings in *Freakonomics* ostensibly supported by rigorous statistical analysis). I make the concession only to indicate that the points I make here are not dependent upon a critique of the authors' empirical testing methods. If the concession is not made, then my argument in this section would hold even more strongly.
5. Numerous other examples could have been chosen as well – e.g., Harford (2009), Landsburg (1993) or Friedman (1997). Each of these employs the same basic logic as that discussed here.
6. Harper Collins, the publisher of *Freakonomics* and *Superfreakonomics* (Levitt and Dubner 2009), offers a range of companion teaching materials and study guides (see http://files.harpercollins.com/OMM/freakonomics_teaching_materials.html).
7. The specific innovation of Bentham and Boole referenced here is the 'quantification of the predicate.' See Jevons (1869, p. 4).
8. This is a somewhat contentious claim, as Jevons does seem to argue in *The Principles of Science* for a broader understanding of the '=' symbol, noting that 'the meaning of the sign has ... been gradually extended beyond that of common [i.e. mathematical] equality' (1958, p. 15). However, he goes on to state that there is 'some real analogy between its diverse meanings' (Jevons 1958, p. 16), and his subsequent discussion is most consistent with the view that it is the non-mathematical uses that conform with the mathematical rather than vice versa. In any event, the mathematical usage is the most restrictive of all the senses of equality he discusses,

- and so in order for that sense to be included among the many senses of equality expressed by ‘ = ’ it must be the case that if there is something extra-mathematical about his meaning of ‘ = ’ it could not obviate any of the mathematical meaning of the symbol.
9. The Boole and Thomson referred to in the quotation are the English logician and mathematician George Boole (referred to above in connection with the quantification of the predicate) and the Scottish mathematician and physicist William Thomson, Lord Kelvin.
 10. Mill expressed his objections to Jevons’ logic directly in a letter to John Elliott Cairnes in December 1871, criticizing Jevons for having ‘a mania for encumbering questions with useless complications, and with a notation implying the existence of greater precision in the data than the questions admit of’ (Mill 1963, XVII, pp. 1862–1863; cited in Maas 2005, p. 97). For Boole’s critique, see Grattan-Guinness (1991).
 11. For De Morgan’s review of Jevons’ logic, see Sánchez Valencia (2001).
 12. The statement is taken from De Morgan (1858).
 13. I have argued elsewhere (Spiegler 2005) that contemporary economic methodology is incoherent in the absence of such a commitment. See also Spiegler and Milberg (2009) for an application of this argument to a recent example taken from the institutional economics literature.
 14. The position has a much longer history that predates economics. Descartes was, famously, comfortable with unrealistic assumptions, holding the predictive and postdictive power of hypotheses as the only legitimate measure of their adequacy. Frustration with this position was the source of Newton’s quip *hypotheses non fingo*.
 15. See Duhem (1962, esp. p. 185) and Quine (1963, esp. chap. II, secs. 5–6).
 16. Suppes (1962) referred to this mediated version of the data as a ‘model of the data.’ For a discussion of the role of models of data in scientific explanation, see also Suppes (1967).
 17. This is not to say that no results coming from such a science could be legitimate and/or correct. Rather, it is to say that since the discipline’s standards are blind to the difference between legitimate and (certain types of) illegitimate results, the discipline’s stamp of approval alone cannot inspire much confidence.
 18. This story has been covered in many venues. See Johnson and Kwak (2010) for a good overview with historical perspective.
 19. The journals in question are *The American Economic Review*, *The Journal of Economic Literature*, *The Journal of Economic Perspectives*, *The Journal of Finance*, *The Journal of Housing Economics*, *The Journal of Money, Credit and Banking*, *The Journal of Political Economy*, *The Quarterly Journal of Economics* and *The Review of Economic Studies*.
 20. In the remainder of the paper, I will take these papers to be a proxy for mainstream writing on the housing and financial markets in the lead-up to the crisis.
 21. See, e.g., Case and Shiller (2003), Shiller (2007), Goodman and Thibodeau (2008), Himmelberg, Mayer and Sinai (2005) and Smith and Smith (2006). There are also several papers examining the phenomenon of bubbles in general, either through historical review (e.g., LeRoy 2004) or through the construction of a theoretical model of bubbles (e.g., Scheinkman and Xiong 2003).
 22. See, e.g., Glaeser, Gyourko and Saks (2005) and Quigley and Raphael (2005).
 23. See, e.g., Peek and Wilcox (2006), Iacoviello (2005) and Ortalo-Magné and Rady (2006).
 24. In the former group, see, e.g., Diamond and Rajan (2005), Ericsson and Renault (2006) and Gorton and Huang (2004). In the latter group, see, e.g., Morrison and White (2005), Dell’Ariccia and Marquez (2006), Krishnan, Ritchken and Thomson (2005) and Van Order (2006).
 25. For the sake of brevity, I do not discuss here the mainstream literature on the macroeconomy in general. The general tenor of this literature was quite upbeat during the 2000s (see, e.g., Chari and Kehoe 2006), though the excessive abstraction of dynamic stochastic general equilibrium modeling during this time has subsequently come in for significant criticism (see, e.g., Colander, Howitt, Kirman, Leijonhufvud, and Mehrling 2008 and Colander et al. 2009).
 26. For the first class of proposals, see, e.g., Colander et al. (2009), Shiller (2005) and Allied Social Sciences Association (2009, 2010); for the second class see, e.g., Besley and Hennessey (2009, 2010).
 27. See Piore (1979) for a particularly instructive example of this in an economic setting.
 28. See, e.g., MacKenzie’s (2006, 2009), Zaloom’s (2006), Lepinay’s (2011) and Ho’s (2009) works in the anthropology of finance, Riles’ (2011) work in the anthropology of legal

communities supporting financial regulation, and Holmes' (forthcoming) work on the anthropology of central banking.

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