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ECONOMICS IN PRACTICE

Model Building versus Theorizing: The Paucity of Theory in the *Journal of Economic Theory*

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ABSTRACT

explanandum. The thing to be explained.

-- *Oxford English Dictionary*, 2nd ed., 1989

FEW WOULD BE RASH ENOUGH TO SUGGEST THAT MATHEMATICAL modeling does not deserve a place in economics training and research. The concern, however, is that the mathematical arts tend to become too detached from topics and factors important in life. We assert some basic requirements of scientific theory, giving rise to three tests: *Theory of what?*, *Why should we care?*, and *What merit in your explanation?* The logic of the three necessary conditions and the results of our content analysis are shown in Figure 1.

In this paper, we show that mathematical economics as represented by articles in the eminent *Journal of Economic Theory* routinely fails the tests. *Journal of Economic Theory* (*JET*) is published by Elsevier, a huge publishing house headquartered in Amsterdam and specializing in academic products.¹ The company's *JET* website states: "*JET* is the leading journal in economic theory. It is also one of nine core journals in all of economics. Among these journals, the *Journal of Economic Theory* ranks fourth in impact-adjusted citations."² The latter claim is probably derived from the journal ranking of Kalaitzidakis et al (2003). There have been many rankings of economics journals, and *JET*

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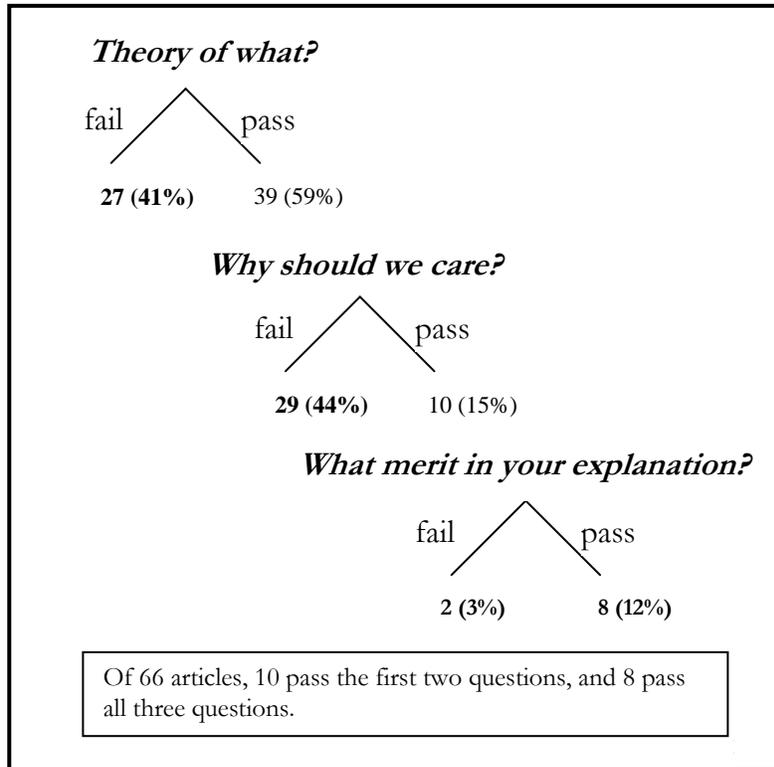
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¹ According to the Elsevier website, an institutional subscription to *JET* in hardcopy for one year is \$2,767. Usually, however, institutions subscribing to the journal purchase it as part of a package deal.

² [Link](#) to *JET* website, accessed 16 April 2007.

generally shows up in the top ten (Stigler et al 1995; Laband and Piette 1994; Liner 2002).

Figure 1:
Summary results of the content analysis of *JET* 2004



JET has exemplified “pure theory,” and those working in the genre and placing “pure theory” articles in top journals have generated many “high impact” citations to *JET*. The sample investigated here is confined to *JET*, but the points would apply greatly to numerous other journals and many articles in general economics journals.

Journal rankings then provide weights or thresholds in ranking economics departments. For example, Dusansky and Vernon (1998) rank U.S. departments based solely on publication in eight “blue ribbon journals,” including *JET*.

We examine the 66 regular articles appearing in the 2004 issues of *JET* and apply the three requirements of theory: *Theory of what?*, *Why should we care?*, and *What merit in your explanation?* We find that 27 articles fail the first test (*Theory of what?*) and 58 articles fail at least one of the three requirements. Thus, 88 percent of the articles do not qualify as theory. (The “pass” rates would be even lower if one were to exclude the special issue, and if one were to include

the short notes.) We relate our findings to those of other scholars who find that *JET* articles rarely graduate to theory.

We contend that the journal's claim to the status of science is doubtful, as well as the very title of the journal. A truer title would be, *Journal of Economic Model Building*. More generally, we challenge calling model building "theory."

We focus on theory as explanation, but that is not the only way to characterize theory. Scholars also have characterized theory as understanding and prediction. All such characterizations are related. Instead of asking, "what is to be explained?" we may ask, "what is to be understood?" or "what is to be predicted?" and accordingly "why should we care?" and "what merit in your explanation?" Explanation, understanding, and prediction all depend ultimately on the model's having empirical meaning and importance.

Surely, the strongest defenders of model building would admit that the practice *might* go too far, into unjustifiable realms of creative writing. Economists will disagree about the "marginal conditions," but every economist will affirm *some* marginal conditions, which imply the hazard of going too far. Every economist, therefore, should appreciate a watchdog effort, even if he objects to the watchdog's test and results.

All stakeholders should be concerned that scholarly prestige will be leveraged in a way that feeds mere scholasticism, rather than real contributions to science, learning, and culture. Even if scholastic arts did not distort thought and understanding, they certainly might divert them from the things that matter more. If *JET*—and many other outlets—consists mainly of crafts that lack integrity as explanation, it does not deserve much prestige within the enterprise we call economics. This article, then, speaks to all stakeholders — elected officials, taxpayers, tuition payers, donors, university administrators, faculty, students, and other citizens concerned about the character and content of economics.

THE INTEGRITY OF "THEORY"

By "model" we mean a system of functions and conditions that yield formal results, such as classes of equilibria within the model. The specific type of model building that has been central to 20th century economics is a mathematical system of "agents" who maximize explicit functions subject to constraints, yielding equilibria. As many have noted, it is a kind of story-telling.³ For example, payoffs and strategy spaces characterize the characters, the conditions and constraints form a setting, and the equilibria correspond to what happens, or the story's completion. Variations in conditions yield variation in story resolution, or a family of related stories. When the story has a moral, it

³ On model building as storytelling, see Gibbard and Varian 1978, Klamler 1992, Colander 1995, McCloskey 1990, Morgan 2001, and Cowen 2007. Some authors suggest that the model itself is a part of the story-telling, and some suggest the pure mathematical components depend on accompanying verbal narrative. But either way it makes a kind of story-telling.

usually corresponds to what we are to think of the equilibria. Nowadays, the term “model” is generally used by economists to mean a formal, explicit system using *mathematical* representation. That is how we use the term here.⁴

Axel Leijonhufvud (1997, 193) notes: “For many years now, ‘model’ and ‘theory’ have been widely used as interchangeable terms in the profession.” There is a tendency to treat the two sets as identical. Such a relation would have two implications:

“theory” \Leftrightarrow “model”

We dispute both “implies” arrows. A model is neither necessary nor sufficient for theory.

Model not necessary: The tendency to reserve the term *theory* for models is pervasive. An economist who develops math-free explanations will often not be credited as a theorist, no matter how original and persuasive the explanation and no matter how important the explanandum. In economics, “theorists” usually means model builders *and only* model builders. But to suggest that “theory” implies “model” is to suggest that Hume, Smith, Marx, Menger, Veblen, Keynes, Coase, Schelling, etc., etc., did not do theory. As Diana Strassmann (1994, 154) puts it, some ideas do not have “even the remotest potential for mathematical expression.” We shall pursue this side of the relation no further.

Model not sufficient: Our concern is to challenge the semantics that hold that every model is (or entails) theory. We maintain that scientific culture understands *theory* to entail requirements of importance and usefulness. Theory must serve real purposes of the science, thus, arguably meriting attention from the scientific community. Throughout this paper, we will often omit the “scientific” qualifier, because it is understood.

Barbara Wootton was a famous British sociologist, made Life Peer in 1958. Besides debating Hayek, she wrote a book *Lament for Economics* (1938), a heterodox critique. She provided an example proving that model does not imply theory:

The nursery poet, for example, who wrote:

If all the world were apple-pie
And all the sea were ink,
And all the trees were bread and cheese
What should we do for drink?

certainly poses a problem to which it is not easy to give a ready and satisfactory answer. Yet no University Chairs have been founded for the study of this particular group of problems, although these would

⁴ Compare with Gibbard and Varian (1978, 666): “As we are using the term, a *model* is involved whenever there is economic reasoning from exactly specified premises.”

unquestionably become of the gravest importance, should the conditions postulated in the first three lines of the poem be realized. And the simple reason is, first, that nobody has the slightest grounds for supposing that the world is likely to turn into apple-pie, the sea into ink, or trees into bread and cheese; and, second, that there is also no reason to suppose that this strange poetic fancy is linked with the prosaic world of common experience in any way which would make the study of the one likely to throw light upon the workings of the other (Wootton 1938, 31-32).

If an economist were to construct a model addressing what we would drink under the stated conditions, would we regard it as scientific theory? Surely not. Not every model entails a theory.

What, then, makes a model a theory? In preparing this article, we consulted the philosophical literature on scientific explanation, by figures like John Stuart Mill, Carl Hempel, Paul Oppenheim, Ernest Nagel, Karl Popper, Stephen Toulmin, L.J. Cohen, John Hospers, and David Kaplan. Such literature takes for granted most of the basics that separate model from theory. For example, in *The Encyclopedia of Philosophy*, Jeagwon Kim (1967) wrote the ten-column entry “Explanation in Science.” It dives forthwith into how explanations are structured, tested, confirmed, selected, and so on. The philosophy literature *takes for granted that there is an explanandum, and that it merits attention from the scientific community.*

It is different among economists. Many economists have criticized model building for its lack of relevance. For example, in *Truth Versus Precision in Economics*, Thomas Mayer (1993, 57f) diagnoses economics as fashioning single strong analytical links, but neglecting the remainder of the chain that would be necessary to really connect any of it to importance. Similarly, Deirdre McCloskey (1994, 131) criticizes model building for aping the math department, where proof and formal analysis are aesthetic crafts.

The Nobel laureate game theorist Robert Aumann (1985, 42) candidly writes: “If one thinks of mathematics as art, then one can think of pure mathematics as abstract art, like a Bach fugue or a Pollock canvas ... ; whereas game theory and mathematical economics would be expressive art, like a cubist painting or Tolstoy’s *War and Peace*. We strive to make statements that, while perhaps not falsifiable, do have some universality, do express some insight of a general nature; we discipline our minds through the medium of a mathematical model; and at their best, our disciplines do have beauty, simplicity, force and relevance.” The relevance Aumann here claims for equilibrium model building seems to be the kind of relevance one would ascribe to *War and Peace*.⁵

⁵ Although elsewhere in the same essay Aumann says that game theory solution concepts “should be judged by the quantity and quality of their applications” (p.65).

The outpouring of criticism of formalistic economics is well known. The most basic criticism of model building remains that of Barbara Wootton: What *in the world* are you talking about? Why should we care?

Leijonhufvud (1997, 196) characterized the problem this way: “Formalism in economics makes it possible to know precisely what someone is *saying*. But it often leaves us in doubt about what exactly he or she is *talking about*.”

Defenders of model building tend to brush off the criticisms as polemics. That treatment, however, ought not to be accepted. Wootton and myriad other skeptics raise a fundamental challenge, and place the burden of proof on those who feed on social resources. In as much as model building subsists on tax dollars, Adam Smith’s presumption of liberty, too, places the burden of proof on the feeders. Why should taxpayers pay for this particular art form?

The attitude of the model builders, however, is that the burden of proof lies with the critics. Ultimately, they fall back on their position in the existing academic power structure, and a faith in an invisible hand operating therein. That faith might be worthy, but, to our knowledge, it has never been seriously argued.⁶

We, too, feel that the burden of proof should be on the feeders. Nonetheless, we now assume the burden of proof. We provide a statement of at least some of what it takes for a model to qualify as theory. Then we see if *JET* meets those requirements.

**Theory of What?
Why Should We Care?
What Merit in Your Explanation?**

We specify three necessary conditions for a model to be a theory:

- I. *Theory of what?* The proponent of the model indicates some real-world phenomena X, and offers the model as an at-least-partial or potential description of the conditions and mechanisms giving rise to X. That is, the model helps explain X. It is a way to understand X.

- II. *Why should we care?* The proponent believes and tries to persuade us that X is of import and might be inadequately explained/understood, that it might merit some of the community’s attention. Thus, the proponent establishes X as an explanandum. Only if he genuinely believes in the need for better explanation and tells us why we

⁶ Incidentally, another faith that, as noted by Wootton (1938, 179), seems to be unargued is that of the professor working in an institution that he feels should not be tax-funded, such as a state university. The argument would be that his good works redeem his personal dependence on the existing system.

should feel likewise, might the explanation deserve to be called a *scientific* explanation.

III. *What merit in your explanation?* The proponent makes a case that his explanation merits attention and resources. Here, it is useful to distinguish two situations hinging on whether the explanandum is previously identified.

- a. If the explanandum is previously identified, then *What merit?* reduces to, *How's yours better?*—that is, better than alternative explanations, even just simple or naïve ones. Thus the proponent sets out alternative explanations and attempts to persuade us of comparative virtues of his explanation, virtues that warrant its holding a place instead of, alongside of, or in conjunction with other explanations.
- b. It is sometimes the case that a theorist organizes and identifies matters into an explanandum more or less for the first time, or, at least, in a novel and original way. That is, he not only runs with the football, but discovers the football that he runs with. As Gibbard and Varian (1978, 669) note, “Perhaps it is initially unclear what is to be explained, and a model provides a means of formulation.” Such formulation is found, for example, in much of the work of Thomas Schelling, who, by providing many empirically meaningful illustrations, freshly identified class of things to be explained, such as commitments, promises, threats, focal points, and tipping points. In such cases of freshly discovering the “football,” it is not fair to demand *How's yours better?*, since alternative explanations may not be available. No one has ever run with that football before, so it is inappropriate to demand better running. The demand of merit, therefore, needs to broaden the eligibility by allowing theory to be original both in the explanation and in the explanandum. *What merit?* allows for such complex originality. But it still demands some proof of merit—“proof” in the common-language sense. That proof will inevitably entail argument that the freshly formulated explanandum-explanation complex is important and useful.

Regardless of whether the explanandum is familiar or newly discovered, the demand of *What merit?* is not a demand for demonstrated dominance. Auditors may assent to “hear out” a new theory, even if *in some respects* it is manifestly weak (Booth 1974). But it also must claim to be strong *in some respects*. And, if we are interested in economic science, some of those respects

must go beyond mere equilibrium storytelling and aesthetics. Without claims to empirical import or relevance, the basic demand for merit is unmet. The explanandum-explanation complex must claim some merits in advancing our understanding of genuine real-world concerns. If the proponent's explanation is complicated, difficult, or bizarre, it must at least promise offsetting benefits (or advantages). Further, the demand is only for a *claim* of such benefits. The claim may be unpersuasive, but here we demand merely a *claim* (which, of course, need not be explicitly stated as such). Absent a claim of promised benefits, an explanation does not merit the title *theory*.

“[I]t is reasonable,” said Barbara Wootton (1938, 30), “to ask the economic theorists at least to show that they have some apparently probable ground for thinking that their present abstractions will eventually ripen into something of concrete and practical utility” (30). We say that the showing of “some apparently probable ground” is a requirement of theory.⁷ Our requirements also concord with Thomas Mayer's vision of scientific standards:

Imagine that academic economic research ... was sold in the market place... Those who want to understand how the economy functions would force suppliers of models to compete in terms of how well the model explains the observed characteristics of the economy. Each modeller would then try to show *that his or her model is superior to its rivals*. (Mayer 1993, 130, italics added)

“THE MARKET FOR ‘LEMONS’” AND ANOTHER EXAMPLE

Thus, a “take” or “story” works its way up. To demonstrate, let's apply the three questions to a familiar classic of economic theory, George Akerlof's “The Market for ‘Lemons’” (1970). The article passes all three hurdles:

- To the question, *Theory of what?*, one could say that Akerlof seeks to explain the non-existence of certain markets, large price spreads

⁷ Incidentally, notice that our requirements for theory have counterparts in *empirical evidence*. Like theory, scientific evidence is necessarily embedded in discussion worthy of some of the community's attention. A recent article in *Economic Inquiry* (Bradbury and Drinen 2007) engages in regression analysis to investigate why in baseball's American League, where pitchers do not bat, more batters are hit by pitch than in the National League, where pitchers do bat. The main reason seems to be that baseballs traveling at 85 mph hurt and might cause injury, so National League pitchers have to worry about direct retaliation by the opposing pitcher, and the hazard deters them from hitting batters. That makes sense. (An alternative explanation has to do with pitchers being weak batters and hence having lower probability of getting on-base if not hit by pitch.) Should we consider the investigation a work of economic science? The hit-batsman rate in baseball is unimportant, and the authors make no attempt to persuade us otherwise. The complex regressions are hardly necessary to get us to assent to the retaliation explanation. The findings might be of interest to economists-*qua*-baseball fans. But what about economists-*qua*-economists? Is it economic science? Does it merit any of the community's attention? Whether in statistics, model building, or some other mode of discourse, the economist must live up to the demand of *importance* if he claims the status of science.

(for example, between interest rates in urban versus rural lending), and institutions and practices that assure quality.

- To the question, *Why should we care?*, Akerlof indicates the inadequacy of alternative explanations, for example, when discussing the uninsurability of the elderly, in asking “why doesn't the price rise to match the risk?” (492). Akerlof's discussion indicates an important question that “standard” thinking did not readily answer, thus indicating the need for better explanation. Throughout the paper, Akerlof brings up real-world affairs and indicates a need for better explanation.
- To the question, *What merit?*, one may say that Akerlof indicates alternative explanations, such as discrimination based on prejudice and the standard theory of price reflecting quality, and, relative to those, he indicates virtues of his “lemons” theory. Alternatively, one might be more inclined to see Akerlof as freshly identifying the set of potential ‘lemons’ problems, and, by discussing real-world institutions, providing explanations for when they are resolved. Either way, the author clearly uses insights imparted by the model to explain real-world affairs of import.

Thus, we are comfortable referring to the “Lemons” article as theory of the non-existence of markets, price spreads, and certain assurance institutions. We might feel it inadequate and unsatisfactory in some respects. Different people will have different judgments about its value or soundness. But everyone will want to know: *Theory of what? Why should we care? What merit?* Familiar models in economics—supply and demand, the prisoner's dilemma, coordination games, the public goods model, the monopoly model, the signaling model of schooling, etc.—all qualify as theory when offered as worthwhile explanations of phenomena with import. Bad or wrong, perhaps, but theory.

Now, consider the *JET* article “An Evolutionary Approach to Learning in a Changing Environment,” which is the second on our list of 2004 *JET* articles. The article opens as follows:

We use the framework of evolutionary game theory to address the question whether the ability to learn (at a cost) has an evolutionary advantage in a strategic environment that is changing over time. If so, under what conditions on the environment can we expect learning to survive and possibly prosper? ¶ We identify the ability to learn with the ability of a player to play the best response to his individual opponent in the current state of the environment. The premise that learning is costly implies that these more sophisticated types must perform strictly better than simple modes of behavior ‘enough of the time’ in order to compensate for the costs and survive.

After reviewing the results found in “a constant environment,” the Introduction continues:

We consider an environment, composed of a set of games, where the underlying game changes stochastically over time. We identify a sufficient condition on a set of games that leads to asymptotic dominance by the learners. It requires that the learners are strictly better, when averaging over different regimes, than each simple type against any possible opponent. . . .

Other than using the terminology of “learning” in the model, the article is devoid of illustration, either factual or fictitious, of what is to be explained. There is no empirical content. It does not engage a previously identified explanandum, nor does it freshly identify a set of real-world affairs as explanandum. There is no explanandum. To the question, *Theory of what?*, it can give no real answer. Failing *Theory of what?*, it naturally also fails *Why should we care?* and *What merit in your explanation?*

HAUSMAN, GIBBARD AND VARIAN, AND SUGDEN

We are saying that the custom of calling model building “theory” is wrongheaded, and we purport to show that very little theory is published in “the leading journal in economic theory” (to quote the *JET* website). Because some readers might regard the claims as outlandish, we here relate our three requirements to discussions by thinkers with strong mainstream reputations.

Daniel Hausman (1992)

In his respected investigation of economic methodology, *The Inexact and Separate Science of Economics*, Daniel Hausman strictly separates model and theory: “Models are definitions of kinds of systems. The assumptions of models are clauses in definitions and not true or false assertions about the world. . . . When one offers a general theoretical hypothesis asserting that a model is true of some realm of reality, then one is offering a theory” (Hausman 1992, 273; see also 78).

Hausman emphasizes that model building need not have any connection to theory: “Such model building and theorem proving does not presuppose that one believes that the particular model is of any use in understanding the world” (79). He does not mean to denounce model building:

[S]cientists may nevertheless wish partly to *separate* questions concerning their conceptual apparatus from questions concerning the extent to which that apparatus applies to the world. That is, they may sometimes wish to investigate the properties of models without

worrying about whether those models depict or apply to any aspect of reality. ... Empirical assessment is out of order simply because there is nothing to assess: no empirical claims have been made. Insofar as one is only working with a model, one's efforts are purely conceptual or mathematical. One is only developing a complicated concept or definition. (79)

To be part of science, however, such explorations must somehow advance the goal of theorizing: "Concepts or terms are important to empirical scientists only insofar as they may enable them to say informative things about the phenomena under study" (79). In the chapter titled "Models and Theories in Economics," Hausman presents the distinctions between model and theory shown in Figure 2:

Figure 2:
Daniel Hausman's distinctions between models and theories
Source: Hausman (1992, 77)

Models	Theories (Descriptions, explanations, predictions)
definitions of predicates, concepts, or systems	sets of lawlike assertions
trivially true or neither true nor false	true or false
point is conceptual exploration	point is to make claims about the world
assess mathematically or Conceptually, untestable	assess empirically Testable
Consist of assumptions	consist of assertions

Hausman does not say whether he thinks economists devote too much time to mere model building. Perhaps he feels that the current levels of resources and prestige given to conceptual exploration seem justifiable. But

Hausman is clearly saying mere conceptual exploration is not theory.⁸ He would agree that a theory would have to be able to answer the question, *Theory of what?* As for *Why should we care?* and *What merit in your explanation?*, Hausman would presumably concur that to qualify as scientific theory an explanation has to claim to merit attention from the scientific community. Hausman's work clearly implies the institutional issue of whether too much resources and prestige are given to model building.

Gibbard and Varian (1978)

In their oft-noted article "Economic Models" in *The Journal of Philosophy*, Allan Gibbard and Hal Varian do not focus on a distinction between model and theory. But they discuss the ways models are used in theorizing. One way they call "models as approximations" of real-world situations. Another is "models as caricatures." Although they note that the model may help a theorist to formulate what is to be described or explained, Gibbard and Varian make clear that theorizing uses models, as approximation or as caricature, to represent "what is to be explained" (669), which might only be aspects of situations. Thus, implicit in their article we see a distinction between models and theories that would seem to conform roughly to Hausman's, and that would authorize the three requirements used throughout this paper.

Robert Sugden (2002)

In his essay "Credible Worlds: The Status of Theoretical Models in Economics," Robert Sugden explores the thoughts of Hausman, Gibbard and Varian, McCloskey, and others, and makes extended use of Thomas Schelling's "checkerboard city" theory of segregation and Akerlof's "Lemons" article (which is where we got the idea). Sugden drives toward the following suggestion

On this view, the model is not so much an abstraction from reality as a *parallel reality*. The model world is not constructed by starting with the real world and stripping out complicating factors: although the model world is *simpler* than the real world, the one is not a *simplification* of the other. The model is realistic in the same sense that a novel can be called realistic. In a realistic novel, the characters and locations are imaginary, but the author has to convince us that they are *credible*—that there could be people and places like those in the novel. ... We judge the author to have failed if we find a person acting out of character, or if we find an anachronism in an historical novel: these

⁸ Further and in parallel fashion, one might defend mere model building as training for economists who will elsewhere produce valuable theory. Yes, but it is training, like obstacle courses for soldiers. Obstacle courses are not battle and, again, mere model building is not theory.

are things that *couldn't* have happened. But we do not demand that the events of the novel *did* happen, or even that they are simplified representations of what really happened. (Sugden 2002, 131; on novels as models, see Cowen 2007)

Sugden's idea of model as a constructed reality seems to imply a distinction between model and theory. In the conclusion he writes, "Nevertheless, the gap between model and the real world has to be bridged. If a model is genuinely to tell us something, however limited, about the real world, it cannot just be a description of a self-contained imaginary world" (133). To qualify as theory, again, it seems only natural that Sugden would concur with our three requirements of theory. Indeed, in the first paragraph of the essay (107), Sugden offers the following intimations:

I have no fellow-feeling with those economic theorists who, off the record at seminars and conferences, admit that they are only playing a game with other theorists. If their models are not intended seriously, I want to say (and do say when I feel sufficiently combative), why do they expect me to spend my time listening to their expositions?

To sum up this section: It is not only "troublemakers" who distinguish model and theory, but also serious methodologists and thoughtful modelers, including Daniel Hausman, Allan Gibbard, Hal Varian, and Robert Sugden. The scheme used in the present investigation accords with what people with strong mainstream reputations say on the matter.

SCHEMATIZING THE INTEGRITY OF EXPLANATION

We read every regular article in the 2004 issues⁹ of *JET* to test whether it met our three demands of theory, *Theory of what?*, *Why should we care?*, and *What merit?* For each question, it is sufficient for our purposes that the article *purport* to answer it. We did not attempt to evaluate the reasonableness or persuasiveness of the motivation nor theoretical soundness. We simply are examining whether the article exhibits those three requirements of theory. We are testing for the existence of certain trappings of theory, not the genuineness or soundness of theories. It is therefore entirely possible that an article passes all three of our tests and yet is quite nonsensical and worthless for understanding the explanandum.

The challenge facing us was to make our testing transparent, accountable, and credible. To meet those challenges, we broke down the analysis into a series of sub-tests. The results of the sub-tests include our judgments and details drawn from the papers, including pertinent quotations. All the sub-tests, quotations, and

⁹ When we commenced the project in late 2005, the most recent complete year published and available online for download was 2004.

judgments are presented in an Excel file linked from Appendix 1 at the end of this paper. One can “spot-check” our analysis by scrutinizing an article and deciding whether we have applied the tests unfairly. Incidentally, in addition to analyzing the 66 *JET* articles, we also analyzed Akerlof’s “Lemons” article, located at the bottom of the Excel worksheet.

The Excel file first provides a quotation indicating the purported subject matter. We strove to select the passage that best indicates the purported subject matter. The next column of the Excel sheet contains the first three sub-tests:

- Sub-test 1:** Does the article illustrate an explanandum in any factual way, including by historical cases or even just by anecdote?
- Sub-test 2:** Does the article illustrate an explanandum by any fictitious example or thought experiment (other than the model itself)?
- Sub-test 3:** Does the model use language of an economic context/scenario? What terms are used in telling the model?

The next Excel column arrives at the first major question:

- Major question 1:** Theory of what? Does the article delimit an explanandum with reasonable clarity?

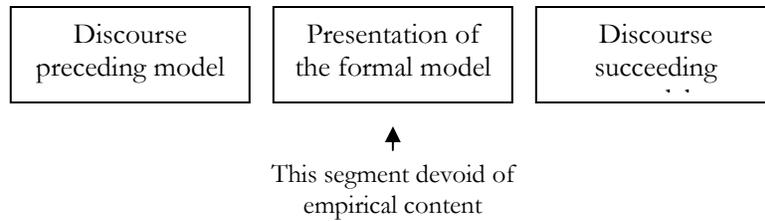
The assessment of *Theory of what?* draws on the prior sub-tests. Sufficient for passing is that the article provides any kind of illustration of the explanandum, either factual or fictitious. But that condition is not necessary. The article might be scored “yes” here by virtue of the economic context and language of the model itself. But whether economic language in the model will save a model is a judgment call, and a “yes” there will not always make a “yes” to *Theory of what?* An example will illustrate.

The 10th paper in the list is entitled, “Local Coordination and Market Equilibria.” The article states its accomplishment as follows: “We reformulate the stability analysis of competitive equilibria as a coordination problem in a market game whose non-cooperative equilibria coincide with competitive equilibria” (276). It provides neither factual nor fictitious characterization of an explanandum. As for sub-test 3, yes, the model uses economic terms including *traders*, *sellers*, *buyers*, *commodity bundles*, and *endowments*. But the storytelling of the model does not map intelligibly to anything we might imagine in our natural knowledge of worldly phenomena to be explained. If the article is supposed to be an explanation, it never fills us in as to what the explanandum is supposed to be. Thus, it fails *Theory of what?*

Only papers that pass *Theory of what?* could potentially pass the remaining major questions, *Why should we care?* and *What merit?* To approach those two questions, we first pose another sub-test:

- Sub-test 4:** Does the article refer to an alternative explanation, including even just a naïve one?

The next sub-tests help to break down the article's empirical referents. In articles, the segment that begins and ends with the formal presentation of the model almost never contains any empirical content. That structure is depicted as follows:



We found that only one article introduces empirical content in the midst of the presentation of the model.¹⁰ In the cases where the model presentation contains no empirical content, we may then distinguish two locations for empirical content: preceding the model and succeeding the model. In asking whether the article goes beyond illustration to greater utilization of empirical learning, we make two separate sub-tests.

Sub-test 5: In the paragraphs preceding the model, does the article refer to any empirical learning that goes beyond mere factual illustration (anecdote or individual incidents)?

Sub-test 6: In the paragraphs succeeding the model, does the article refer to any discussion of empirical knowledge cited as evidence for one explanation or another?

Those sub-tests along with the previous and our general reading of the article, lead to:

Major question 2: Why should we care? Does the article say why any economist should expend attention on better explaining the explanandum?

To pass *Why should we care?*, the article must either: (1) indicate *some inadequacy* in how alternative explanations (perhaps even just naïve ones) explain the explanandum, or (2) suggest that it is freshly identifying the explanandum and indicate how such identification might be useful. Either way, the indication of prior inadequacy helps to provide the research's *scientific motivation*. For this scoring, sub-tests 4 and 5 are particularly useful, but yeses there are neither necessary nor sufficient for passing *Why should we care?*

¹⁰ That article was Article #16 as listed in the Excel worksheet.

The next column contains the last major question:

Major question 3: What merit in your explanation? In the case of addressing a previously identified explanandum, we may ask more specifically: Does the article say how its explanation has advantages relative to or in conjunction with an alternative explanation? Otherwise, does the article say how its explanandum-explanation complex promises benefits?

To pass *What merit?*, the article must either allude to an alternative explanation, even a simple or naïve one, and say why its model explains features that the alternatives do not explain (or not as nicely), or it must claim promised benefits of a freshly formulated explanandum-explanation complex. Passing *What merit?* does not hinge mechanically on the sub-tests 4, 5, and 6.

A recapitulation of the six sub-tests and three major questions, *in abbreviated forms*, follows:

Sub-test 1: Does the article illustrate the explanandum in any factual way?

Sub-test 2: Does the article illustrate the explanandum by any fictitious example?

Sub-test 3: Does the model use language of an economic context/scenario?

Major question 1: Theory of what?

Sub-test 4: Does the article refer to an alternative explanation?

Sub-test 5: In the paragraphs preceding the model, does the article refer to any empirical learning that goes beyond mere factual illustration?

Sub-test 6: In the paragraphs succeeding the model, does the article refer to any discussion of empirical knowledge cited as evidence for one explanation or another?

Major question 2: Why should we care?

Major question 3: What merit in your explanation?

RESULTS OF THE CONTENT ANALYSIS OF *JET* 2004

Articles included in the analysis: Our investigation includes all of the regular articles published by JET in the 2004 issues (vols. 114-119). Two clarifications are in order:

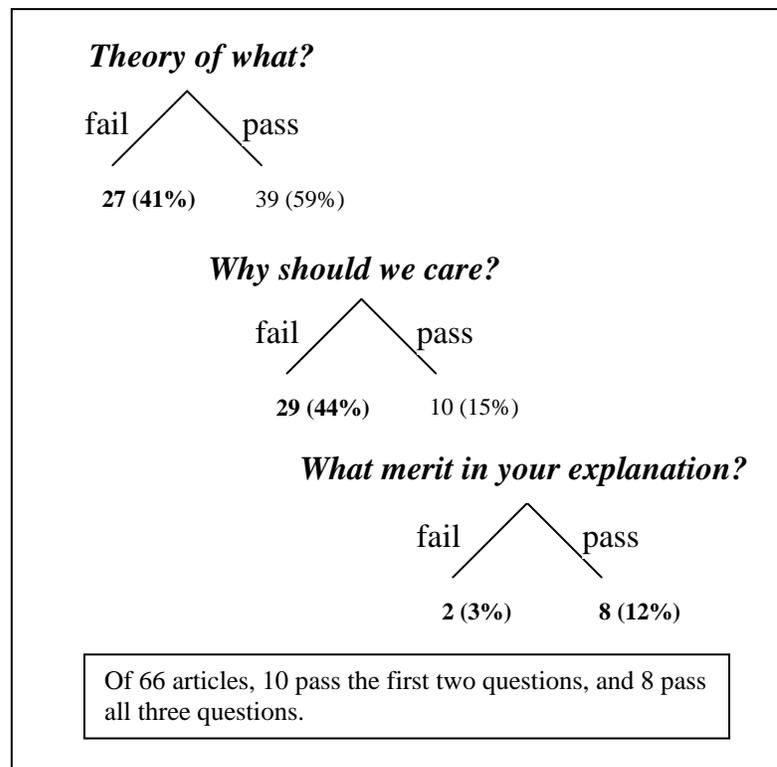
1. We included the special issue (Vol. 119, no. 1, Nov. 2004) on “Macroeconomics of Global Capital Market Imperfections,” based principally on papers presented at a conference held at

Duke University in 2001. The inclusion is significant: Of the eight papers that pass our three requirements of theory, four (or 50%) are contained in that issue.

2. We excluded all articles that the journal categorized as Notes or Comments—22 in all. The exclusion is significant: cursory examination of the Notes and Comments leads us to believe that very few, perhaps none, would pass the three requirements of theory.

Thus we examined the 66 regular articles of the 2004 issues. The basic results are summarized in Figure 3 (which is identical to Figure 1):

**Figure 3 (same as Figure 1):
Summary results of the content analysis of *JET* 2004**



41% Fail *Theory of What?*

Our analysis finds that 27 of the 66 articles cannot satisfy *Theory of what?* They stumble at the first hurdle, and do not qualify as theory on that basis alone. Here we list some examples drawn from only *the first half of sample*:

Art. #3: "Social Optimality and Cooperation in Nonatomic Congestion Games." *From abstract*: "Congestion externalities may result in nonoptimal equilibria. For these to occur, it suffices that facilities differ in their fixed utilities or costs. As this paper shows, the only case in which equilibria are always socially optimal, regardless of the fixed components, is that in which the costs increase logarithmically with the size of the set of users."

Art. #4: "A Solution to the Hold-up Problem Involving Gradual Investment." *From abstract*: "We consider a setting in which the buyer's ability to hold up a seller's investment is so severe that there is no investment in equilibrium of the static game typically analyzed. We show that there exists an equilibrium of a related dynamic game generating positive investment. The seller makes a sequence of gradually smaller investments ..."

Art. #5: "Unmediated Communication in Games with Complete and Incomplete Information." *From abstract*: "We study the effects of adding unmediated communication to static, finite games of complete and incomplete information. We characterize $SU(G)$, the set of outcomes of a game G , that are induced by sequential equilibria of cheap talk extensions."

Art. #12: "The Evolution of Exchange." *The abstract*: "Stochastic stability is applied to the problem of exchange. We analyze the stochastic stability of two dynamic trading processes in a simple housing market. In both models, traders meet in pairs at random and exchange their houses when trade is mutually beneficial, but occasionally they make mistakes. The models differ in the probability of mistakes. When all mistakes are equally likely, the set of stochastically stable allocations contains the set of efficient allocations. When more serious mistakes are less likely, the stochastically stable states are those allocations, always efficient, with the lowest envy level."

Art. #15: "Bargaining and Competition Revisited." *From abstract*: "We show the robustness of the Walrasian result obtained in models of bargaining in pairwise meetings."

- Art. #17: “Expected Utility Theory without the Completeness Axiom.”
From abstract: “We study the problem of obtaining an expected utility representation for a potentially incomplete preference relation over lotteries by means of a set of von Neumann–Morgenstern utility functions.”
- Art. #18: “Non-existence of Recursive Equilibria on Compact State Spaces when Markets Are Incomplete.” *From abstract:* “This paper analyzes one-good exchange economies with two infinitely lived agents and incomplete markets.”
- Art. #24: “Macroeconomic Fluctuations and Bargaining.” *From abstract:* “I study the limit rule for bilateral bargaining when agents recognize that the aggregate economy (and thus the match surplus) follows a finite-state Poisson process.”
- Art. #25: “Concave Utility on Finite Sets.” *The abstract:* “When does a preference relation on a finite set have a concave or a strictly concave utility function? We provide a complete answer. Our proof is an application of the Theorem of the Alternative, and constructs a concave utility if one exists.”
- Art. #26: “Core Many-to-one Matchings by Fixed-point Methods.” *The abstract:* “We characterize the core many-to-one matchings as fixed points of a map. Our characterization gives an algorithm for finding core allocations; the algorithm is efficient and simple to implement. Our characterization does not require substitutable preferences, so it is separate from the structure needed for the non-emptiness of the core. When preferences are substitutable, our characterization gives a simple proof of the lattice structure of core matchings, and it gives a method for computing the join and meet of two core matchings.”
- Art. #29: “A Necessary and Sufficient Condition for Non-emptiness of the Core of a Non-transferable Utility Game.” *The abstract:* “It is well-known that a transferable utility game has a non-empty core if and only if it is balanced. In the class of non-transferable utility games balancedness or the more general π -balancedness due to ... is a sufficient, but not a necessary condition for the core to be non-empty. This paper gives a natural extension of the π -balancedness condition that is both necessary and sufficient for non-emptiness of the core.”
- Art. #32: “Sorting Equilibrium in a Multi-jurisdiction Model.” *The abstract:* “This paper analyzes a general model of an economy with heterogeneous individuals choosing among two jurisdictions, such as towns or political parties. Each jurisdiction is described by its constitution, where a

constitution is defined as a mapping from all possible population partitions into the (possibly multidimensional) policy space. This study is the first to establish sufficient conditions for existence of sorting equilibria in a two-jurisdiction model for a policy space of an arbitrary dimension.”

Those are 12 of the 27 articles that failed *Theory of what?* Regarding Article #24, “Macroeconomic Fluctuations and Bargaining,” one might think that the article seeks to explain macroeconomic fluctuations or bargaining. In fact, the article does not refer to any real-world events or experience, and provides no living sense of what it might be explaining. The article is entirely about what happens within certain models.

For some of the articles failing *Theory of what?*, one may question whether they really even fashion themselves as explanations. While using some economic terminology (“congestion,” “utility,” “strategy,” etc.), some are essentially mathematical (e.g., #3, 17, 18, 25, 26, 29 listed above). Also, one article (#20) reports the results of a classroom experiment that tries to recreate a pre-existing model; another (#54) designs an allocation mechanism as a kind of operations research problem. These endeavors do not qualify as explanations, but, in fairness, they do not pretend otherwise. Still, such works will generally be termed “theory” within the academic culture.

If one were to ask an author of one of any of the 27 articles, “What *in the world* are you talking about?” the only responsible answer would be: “Nothing.” Again, 41 percent fall into that category.

44 percent Stumble at *Why Should We Care?*

Of the 39 articles that passed the first hurdle, 29 stumble at *Why should we care?*, so 44% of the articles have the profile (Pass, Fail, Fail). Here we briefly examine four of those 29 articles.

Art. #6: “Informed Manipulation,” offers a model in which inside traders have incentives to “trade in the wrong direction.” We failed it on *Why should we care?* because the paper does not provide any factual or fictitious illustrations relevant to the topic, nor does it say why “information manipulation” stands in need of better explanation. The authors present the model without any effort to connect the model to an empirical issue. There are six pages dedicated to technical appendices.

Art. #30: “Unequal Uncertainties and Uncertain Inequalities: An Axiomatic Approach,” provides “an axiomatic characterization of social welfare functions for uncertain incomes” (from the abstract). The authors introduce the topic with a numerical example of international trade and wages in two sectors across two countries. Then, they establish their model’s axioms and deduce theorems. We failed it on

Why should we care? because it provides no factual-based illustration or indication of the model mechanisms, nor any defense of the relevance of the model to international trade policy. The paper contains eleven pages of technical appendices.

Art. # 36: “Softening Competition through Forward Trading,” models a duopoly in forward trading. The authors write in the abstract: “We show that forward trading results in producers buying forward their own production, so that equilibrium prices are increased compared to the case without forward trading. This result contrasts with the social desirability of forward markets emphasized by the academic literature.” At the outset they mention the conventional view of forward trading as beneficial. The authors proceed to show that within the model forward trading can be practiced in strategic ways. We failed it on *Why should we care?* because the paper contains no argument or evidence of the importance of the model mechanisms to real world issues or controversies.

Art. #52: “Differentiating Ambiguity and Ambiguity Attitude,” is a case that might have been failed on *Theory of what?*, but we decided to say it passes, as being a theory of the role of ambiguity in decision making. The authors write in the abstract: “The objective of this paper is to show how ambiguity, and a decision maker (DM)'s response to it, can be modelled formally in the context of a general decision model.” We failed it on *Why should we care?* because the article does not provide a single real world issue or problem that the formulation might help us understand or explain. Then, the paper goes axiom by axiom to build propositions and lemmas using terminology of real analysis and topological concepts. Sixteen pages are given to technical appendices.

Three percent Stumble at *What Merit in Your Explanation?*

Of the 10 articles that passed the first two hurdles, two stumble at *What merit in your explanation?*, so 3 percent of the total fall in to the category (Pass, Pass, Fail). Here we remark on those two articles.

Art. #56, “Government Guarantees and Self-fulfilling Speculative Attacks,” states: “We develop a model in which government guarantees to banks’ foreign creditors are a root cause of self-fulfilling twin banking-currency crises” (from the abstract). The article uses economic terminology in modeling financial crises, a real matter of obvious import. But the article fails on *What merit?* because it simply works through mechanisms within the context of the model and never suggests that the results correspond to or illuminate any facts or history. Once the model gets started, empirical referents never enter into the

article. Nor does the article claim to show virtues of its model over other models or simpler explanations based on government guarantees. Thus, the article provides no apparent answer to *What merit in your explanation?*

Art. #58, “Smoothing Sudden Stops,” provides a model in which international financial flows might come to a sudden stop at the first signs of crisis. The authors identify the heart of the trouble as structural domestic limitations that inhibit “external insurance.” The authors write: “we show that if *domestic* agents are able to write complete insurance contracts with each other, the external underinsurance problem disappears” (106). Again, the purported topic is of import. But the authors fail to connect the model mechanisms and results to empirical or historical referents. Nor does the article claim to show virtues of its model over other models or simpler explanations based on ideas of such domestic structural limitations. On p. 122 the authors claim to have made two contributions, but the purported contributions are essentially that they have modeled certain assumed factors and mechanisms.

12 percent Clear All Three Hurdles and Qualify as Theory

Only eight articles—or 12% of the 66—pass *Theory of what?*, *Why should we care?*, and *What merit?* It is noteworthy that four of the eight passing articles appeared in the special issue (Vol. 119, no. 1, Nov. 2004) on “Macroeconomics of Global Capital Market Imperfections.” Thus, exclusion of the special issue would have meant that only four of 58 regular articles passed, or seven percent.

Here we briefly examine each of the eight articles and note the surmounting of the hurdles.

Art. #8: “Optimal Fiscal and Monetary Policy under Sticky Prices” presents a model to answer the following (199): “Should the central bank pursue policies that imply high or low inflation volatility?” Four sections describe the quantitative properties of their model. Data of the last forty years from the US is used to calibrate the model and compute parameters. The analysis based on the calibrated model is shown so to fit the historical data, and a policy conclusion that central banks should favor price stability over any other policy goal.

Art. #16: “Fiscal Shocks and Their Consequences” deals with the following issue: “The basic question that we address is whether standard neoclassical models can account for the response of hours worked and real wages to a fiscal policy shock” (90). The authors analyze data from the post World War II era in the United States. Two sections describe

their empirical strategy and the stylized facts extracted from the data. They then develop a model based on those characteristics. They also discuss alternative explanations and, using both calibrated and econometric evidence, argue that their explanation is better supported by the data. (Incidentally, this is the only paper in the entire set that includes empirical content during the presentation of the model.)

Art. #31: “Endogenous Lifetime and Economic Growth” presents an economic growth model with human capital, with threshold effects in life expectancy, human capital investment, and economic growth such that a country may get trapped rather than converge toward developed countries. At the outset the paper motivates the topic with empirical material about health and GDP growth. In an overlapping generations model the author studies the mortality rates and life expectancy to understand the relation between health and economic growth. He constructs an empirical test using data from 95 countries to test the model. He suggests that public investment in health can help poor countries escape poverty traps.

Art. #53: “Optimal Monetary Policy in a Phillips-curve World” states: “The goal of this paper is to study the optimal monetary policy in a model in which there is a direct link between these policies and employment” (175). The model is motivated with empirical claims and two sections present empirical evidence based on data from the United States and a set of countries to make cross-section analysis. In subsection 5.1 and their conclusion they talk about alternative explanations and how the data better support their model.

Art. #55: “A Corporate Balance-sheet Approach to Currency Crises,” says in the abstract: “This paper presents a general equilibrium currency crisis model of the ‘third generation’, in which the possibility of currency crises is driven by the interplay between private firms’ credit-constraints and nominal price rigidities.” We passed the paper on the first two major questions because it is about currency crises and appropriate policy, and the topic holds obvious import. We passed it on *What merit?* because, as noted by the authors (see abstract and pp. 24-25), its central analysis can be represented graphically, which is a merit.

Art. #57: “Monetary Policy in a Financial Crisis,” presents a model of monetary policy that treats interest rates as the instrument or control variable. We passed the paper on the first two major questions because it is about currency crises and appropriate policy, and the topic holds obvious import. After presenting the model the authors carry out a calibration with data from Korea and Thailand to compute parameter

values. Using those parameters they explore what would happen in the aftermath of a financial crisis when interest rates are cut, and when interests are hiked. The authors write: “We conclude that resolving the debate over the effects of an interest rate cut in the aftermath of a financial crisis requires understanding how much short-run flexibility there is in the economy. We suspect that there is relatively little such flexibility, at least in the short run, so that the contraction scenario may be the most plausible one” (102). By working through policy variations in a calibrated model of important phenomena, the article offers comparative results, making a reasonable claim of merit for the investigation.

Art. #60: “Contagion of Self-fulfilling Financial Crises due to Diversification of Investment Portfolios,” develops a model for contagion during financial runs between neighboring countries. Again, the topic holds obvious import. After presenting the model, the authors review empirical evidence that better fits their model than identified alternative models, making for a claimed merit. In fact, the section providing that discussion is called: “Applicability of the model to real world phenomena” (170).

Art. #62: “Financial Globalization and Real Regionalization” presents a model that is aimed at capturing the following historical evolution: “Over the period 1972–1986, the US business cycle was strongly correlated with the business cycle in the rest of the industrialized world. Over the period 1986–2000, international co-movement was much weaker (real regionalization). At the same time, US international asset trade has increased significantly (financial globalization)” (207). At the outset the authors illustrate their case with factual claims based on US and OECD country data. The whole section two is devoted to describing relevant data for the model they build afterwards. A subsequent section discusses related empirical papers to support their model’s results. Another section they calibrate their model using historical data. The analysis based on the calibrated model is shown so to fit the historical data. Although they do not present alternative explanations, their analysis is empirically oriented and justified.

Some Incidentals

Seventeen articles acknowledge support from the National Science Foundation, and many others acknowledge support from other government agencies (including non-US agencies). Five articles indicate that they are based on dissertation research. Several articles make policy remarks. Details are found in the Excel file columns R and S.

Are *JET* Models Subsequently Tested?

It is possible that other economists take published models and subsequently supply the commitment to empirical relevance necessary to graduate the models to theory (Hausman 1992, 273). Whether such graduation occurs is a question calling for further research, but investigations by Philip Coelho and James McClure (2005; 2007) suggest that few models graduate to theory. In one investigation, Coelho and McClure identify the *JET* articles published in 1980 and containing at least five lemmas. They find that there were 12 such articles. They then investigate the articles that cite those 12 papers. As of June 2006, there were 237 articles that cite the 12 *JET* articles. They report that of 237, only nine utilize data. Of the nine, only two articles attempt a direct empirical assessment of the model's results, and zero render a judgment of "accept" or "reject." Coelho and McClure conclude: "the originating articles have to date defined no operational propositions" (2007, 13).

Do Economists Think that Model Building Is Over-valued?

Our finding that much "theory" is really mere model building conforms to findings indicating that a large portion of economists, when answering in the privacy of a confidential survey, indicate they think that the journals overdo mathematical economics. Grubel and Boland (1986, 434) found that 64 percent of economists responded "too much" to "The proportion of journal space devoted to mathematical economics," while only one percent said "too little." William Davis (1997, 164) finds that 40 percent of economists "generally disagree" that "Theoretical models used in economic research are generally reflective of the state of the world they are meant to portray" (see also Davis 2007). Davis (2004) interprets the situation as one exhibiting what Timur Kuran (1995) calls preference falsification, where many individuals play along with a situation they do not really believe in.

Meanwhile, there are some signs that the prestige of mere model building might be flagging of late. Sutter and Pjesky (2007, 237) provide evidence that space in the *American Economic Review* and in *Economic Journal* given to articles with model-building and without regression tables had declined in 2003-04 relative to previous decades. In a recent study of changing impact-weight journal rankings, Kim, Morse, and Zingales (2006, 12) write: "In the meantime the *Journal of Economic Theory (JET)* declined from the thirteenth position in the 1970s, to fifteenth in the 1980s and twenty-ninth in the 1990s," although they note that partly the decline is general to economics journals as compared to finance journals they include in the analysis. To check whether citations to *JET* have been sagging of late, however, we consulted Thomson ISI's *Journal Citation Reports*, and did *not* find declines (even in percentage terms) of the citations made by *American Economic Review*, *Economic Journal*, *Journal of Political Economy*, and *Quarterly Journal of Economics* to *JET* over

the period 1999 to 2005. So, as far as we know, the evidence that model building has been losing prestige and “market share” remains spotty.

**SUGGESTION FOR FUTURE RESEARCH:
THE MARKET FOR LEMMAS AND ESCAPISM**

Without vigilant concern for relevance, importance, and usefulness, model building may degenerate into a genre of creative writing, as noted by one of the founders of game theory:

As a mathematical discipline travels far from its empirical source, or still more, if it is a second or third generation only indirectly inspired by ideas coming from ‘reality’, it is beset with very grave dangers. It becomes more and more purely aestheticizing, more and more purely *l’art pour l’art*. This need not be bad, if the field is surrounded by correlated subjects, which still have closer empirical connections, or if the discipline is under the influence of men with an exceptionally well-developed taste. But there is a grave danger that the subject will develop along the line of least resistance, that the stream, so far from its source, will separate into a multitude of insignificant branches, and that the discipline will become a disorganized mass of details and complexities. In other words, at a great distance from its empirical source, or after much ‘abstract’ inbreeding, a mathematical subject is in danger of degeneration. (John von Neumann quoted in Dore et al 1989, xiv)

Only 12 percent of the articles in *JET* 2004 pass *Theory of what?*, *Why should we care?*, and *What merit in your explanation?* The vast majority of *JET* articles do not deserve the name “theory.” And yet such work is routinely called “theory” and the journal has great prestige.

“Economics at the end of the twentieth century,” writes E. Roy Weintraub (2002, 7), “is a discipline that concerns itself with models, not theories, so how did this happen and what does it mean?”

Many explanatory factors come to mind, including entry restrictions (Grubel and Boland 1986) and the romance of mathematics (Gibson 2005). Here we wish to make a suggestion toward an historical account of the market for lemmas.

We suggest the importance of *escapism* in the evolution of academic culture. That culture evolves in ways that accommodate and rationalize various urges to *escape*—

- from “the turmoil and mud of politics” (Macvane 1895, 184) and “a disillusioning contemplation of the march of events” (Graham 1942,

MODEL BUILDING VERSUS THEORIZING

- xvii-xx; see also Cannan 1933, 378; Knight 1951, 5; Leijonhufvud 1973, 337);
- from the fickleness and foolishness of public opinion and a sense of impotence (Newcomb 1893; Hutt 1936, 34f);
 - from the unpopularity and marginalization of going against the tide (Dicey 1905, 448; Hutt 1936, 34f; Philbrook 1953);
 - from moral and cultural factors of the problem (Graham 1942; Sen 1987, 7) and the responsibility of exercising individual judgment (Myrdal 1969, 41; Yeager 1997, 162f);
 - from real-world complexities and situational peculiarities that frustrate the will to know and embarrass the pretense of knowledge (Keynes 1936, 298; Gordon 1955, 161; Buchanan 1979, 280; Hayek 1989, 7);
 - from realities that challenge one's own fancies (Smith 1776, 772; Wootton 1938; Boettke 1997);
 - from the workplace acrimony and career hazards of ideological dissonance (Tullock 1989, 246).

The flight from relevance then tends to lead to a worsening of judgment, further degradations in public policy and opinion, and more intense urges to escape. There is a tendency for relevance and good judgment to fall and rise together (Hutt 1936, 34f, 207f). We suggest that any historical account of the market for lemmas try to incorporate that relationship.

APPENDIX

Excel file detailing the scorings of the 66 articles of *JET* 2004, including the six sub-tests and the three major questions: *Theory of what?*, *Why should we care?*, and *What merit in your explanation?* [Link](#).

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