

THEORIZING IN SOCIAL SCIENCE

The Context of Discovery

Edited by Richard Swedberg

Stanford Social Sciences
An Imprint of Stanford University Press
Stanford, California

2014

5 Three Frank Questions to Discipline Your Theorizing

Daniel B. Klein

explanandum: The thing to be explained.

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

RICHARD SWEDBERG'S FLAGSHIP PAPER, "THEORIZING IN SOCIOLOGY and the Social Sciences: Turning to the Context of Discovery," invites us to try to teach students to theorize, and it guides us on how to do so. Also, the paper enhances our appreciation of real theorists. Michael Polanyi (1963: 96) suggested that "we need reverence to perceive greatness, even as we need a telescope to observe spiral nebulae." Swedberg's essay shows the considered reverence proper to the edifying of young scholars and the inspiring of their abductivity.

When Swedberg asked me to participate in the present project, he suggested that I further explore ideas that Pedro Romero and I raised in a paper titled "Model Building versus Theorizing: The Paucity of Theory in the *Journal of Economic Theory*."¹ The paper criticizes certain cultural norms in professional economics, norms that allow so-called theorists to pass off mere models as theory. The *Journal of Economic Theory* (*JET*) is one of the worst offenders, and one of the most prestigious. Romero and I maintain that a model qualifies as theory only if the article offering the model answers three questions:

1. Theory of what?
2. Why should we care?
3. What merit in your explanation?

We examined all of the regular articles in the 2004 issues of the journal and concluded that only 12 percent answered all three questions, and thus that

only 12 percent of the articles in the *Journal of Economic Theory* qualify as theory.

The problem Romero and I addressed does not afflict other social science fields to the extent it does economics, and especially not the audience for this book. So the aspiration in the present essay is first to recap the Klein-Romero critique and then to use the three questions in the service of teaching and improving the kind of theorizing Swedberg has in mind. Whether the essay helps with what Swedberg is trying to get at—the context of discovery, the creative experience, abduction—is for the reader to decide. Facing the three questions is a kind of training, a training that builds discipline. Such training might have played a role in making the abductive theorist what he or she is. Swedberg says that “[r]ules are typically helpful for the beginner” (2012: 16). Perhaps the three questions help one to discipline his or her theorizing; they might help one to build out the theory.

The Critique of the *Journal of Economic Theory*: Motivation and Method

Axel Leijonhufvud (1997: 193) notes about the economic profession, “For many years now, ‘model’ and ‘theory’ have been widely used as interchangeable terms in the profession.” Treating the two sets as identical would have two implications: “theory” \Leftrightarrow “model.” We dispute both “implies” arrows. A formal model, either mathematical or diagrammatic, with quite explicit equilibrium conditions, is neither necessary nor sufficient for theory.

Model not necessary: 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, Weber, Durkheim, Veblen, Keynes, Coase, and others did not do theory. As Diana Strassmann (1994: 154) puts it, some ideas do not have “even the remotest potential for mathematical expression.”

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.

In her book *Lament for Economics* (1938), Barbara Wootton wrote a kind of proto-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 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 those conditions, would we regard it as theory? Surely not. Not every model entails a theory.

The outpouring of criticism of formalistic economics is well known. The most basic criticism of model-building remains Wootton's: 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*."

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

- I *Theory of what?* The proponent of the model indicates some set of real-world phenomena X, and offers the model as at least a 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 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.

2. If the explanandum is previously identified, then *What merit?* reduces to, *How's yours better?*—that is, how is it 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.

3. 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 also discovers or invents the football that he runs with. As Allan Gibbard and Hal 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 the economist Thomas Schelling who, by providing many empirically meaningful illustrations, freshly identified classes 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 is 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 allow 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 modeling craftsmanship. 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, our 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 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." We say that the showing of "some apparently probable ground" is a requirement of theory. Our requirements also conform quite nicely 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, emphasis added)

In our article, Romero and I consider George Akerlof's "The Market for 'Lemons': Quality Uncertainty and the Market Mechanism" (1970) and explain why we say it passes all three hurdles. We then turn to another example, from the 2004 *JET*, and explain why we say it fails them. We then relate our approach to discussions by four thinkers with strong mainstream reputations—Daniel Hausman (1992), Allan Gibbard and Hal Varian (1978), and Robert Sugden (2002)—and conclude that our distinction between models and theories, and our bases for distinguishing the two, are highly congruent with their thinking. We then go on to explain the scheme of our investigation in greater detail.

We read every regular article in the 2004 issues of the *Journal of Economic Theory* to test whether it met our three demands of theory: *Theory of what?*, *Why should we care?*, and *What merit?*² For each question, it is sufficient for our purposes that the article *purport* to answer it. We are testing for the existence of certain requisites of theory, not the soundness of the theory. An article might pass all three of our tests and yet be 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 subtests. The results of the subtests include our judgments and details about the papers, including pertinent quotations. All of the subtests, quotations, and judgments are presented in a spreadsheet that was appended to our article.³ One can spot-check our analysis by scrutinizing an article and deciding whether we have applied the tests fairly.

The spreadsheet first provides a quotation indicating the purported subject matter. We strove to select the passage that best describes the purported subject matter. The next columns of the spreadsheet contain the first three subtests:

Subtest 1: Does the article illustrate an explanandum in a factual way, including by historical cases or even just anecdotes?

Subtest 2: Does the article illustrate an explanandum with any fictitious examples or thought experiments (other than the model itself)?

Subtest 3: In telling the model, does the author use language suggesting an economic context or scenario?

The next 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 subtests. 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” on *Theory of what?* 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—a “yes” at Subtest 3 will not always make a “yes” to *Theory of what?* An example will illustrate.

The tenth paper in the list is titled “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” (Chatterji and Ghosal 2004: 276). It provides neither factual nor fictitious characterization of an explanandum. As for Subtest 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?* have the potential to pass the remaining major questions, *Why should we care?* and *What merit?* To approach those two questions, we pose another subtest:

Subtest 4: Does the article refer to an alternative explanation, including even just a naïve one?

The subtests that come next 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 in Figure 5.1.

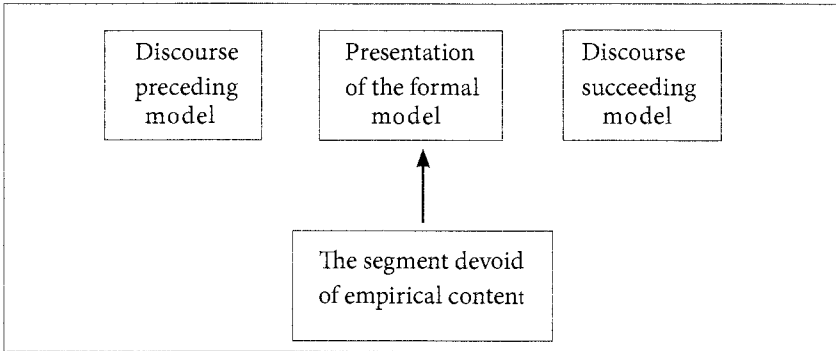
Only one 2004 *JET* article introduces empirical content in the midst of the presentation of the model.⁴ In the cases where the model presentation contains no empirical content, we may then distinguish two locations for empirical content: preceding the model and succeeding the model. In asking whether the article goes beyond illustration to greater utilization of empirical learning, we apply two separate subtests:

Subtest 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)?

Subtest 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 subtests, along with the previous ones 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 energy on better explaining the explanandum?

FIGURE 5.1 Model-building articles almost always have three parts.

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, subtests 4 and 5 are particularly useful, but passing those tests is neither necessary nor sufficient for passing *Why should we care?*

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 promise benefits of a freshly formulated explanandum-explanation complex. Passing *What merit?* does not hinge mechanically on Subtests 4, 5, and 6.

Here follows a recapitulation of the six subtests and three major questions, in abbreviated form:

Subtest 1: Does the article illustrate the explanandum in any factual way?

Subtest 2: Does the article illustrate the explanandum with any fictitious example?

Subtest 3: Does the model use language of an economic context or scenario?

MAJOR QUESTION 1: Theory of what?

Subtest 4: Does the article refer to an alternative explanation?

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

Subtest 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

Our investigation includes all sixty-six of the regular articles published by *JET* in the 2004 issues (vols. 114–19). The results are summarized in Figure 5.2.

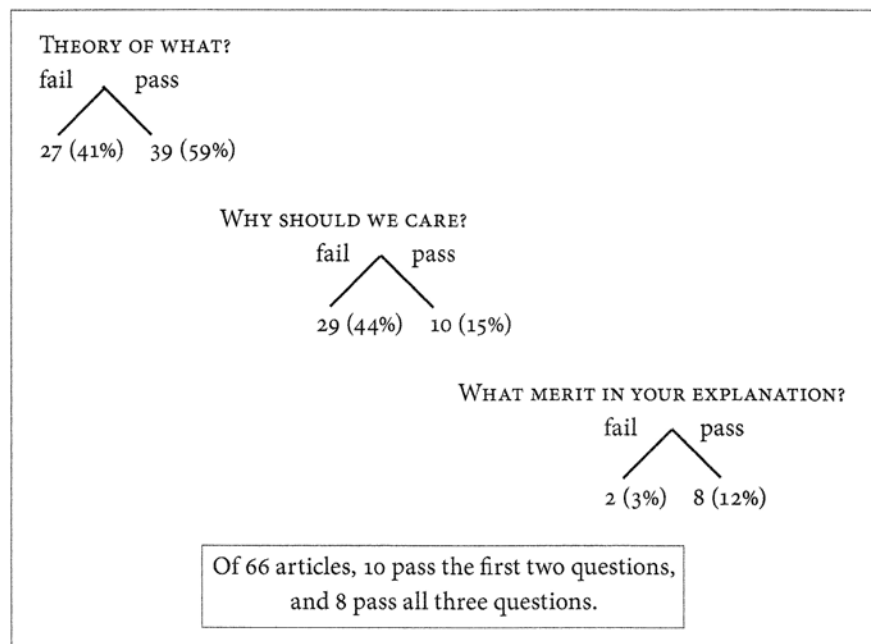
Our analysis finds that twenty-seven of the sixty-six articles do not satisfy *Theory of what?* They stumble at the first hurdle and do not qualify as theory on that basis alone. In our original article, Romero and I provided illustrative quotations, usually from the papers' abstracts, of twelve of the twenty-seven articles, to show some of the ways in which they failed to satisfy *Theory of what?*⁵ If one were to ask an author of one of any of the twenty-seven articles, "What in the world are you talking about?" the only responsible answer would be: "Nothing."

Of the thirty-nine articles that passed the first hurdle, twenty-nine stumbled at *Why should we care?*, so 44 percent of the articles have the profile (Pass, Fail, Fail). Of the ten articles that passed the first two hurdles, two stumbled at *What merit in your explanation?*, so 3 percent of the total fall into the category (Pass, Pass, Fail). Only eight articles—or 12 percent of the sixty-six—passed *Theory of what?*, *Why should we care?*, and *What merit?* In our original article, we briefly examine samples at each of first two stages and then all eight articles at the last.

Another economist with an impeccable mainstream reputation, E. Roy Weintraub, wrote, "Economics at the end of the twentieth century is a discipline that concerns itself with models, not theories, so how did this happen and what does it mean?" (Weintraub 2002: 7). Romero and I conclude by offering some ideas to consider in addressing Weintraub's questions.

In 2008 another article about *JET* was published that complements ours. Research like that which appears in *JET* is often defended as laying the groundwork for subsequent work that hammers out applied theory fitting the

FIGURE 5.2 Summary results of the content analysis of JET 2004



real world, and that tests such applied theory with empirical data. In this view, abstract work is like higher-order investment in knowledge, generating lower-order applied knowledge. Philip Coelho and James McClure (2008) examined the 1980 issues of *JET* for articles in which the word “lemma” appears at least five times. There were twelve such articles. They then looked at all the articles that cite those articles and investigated whether the twelve *JET* articles laid the groundwork for empirical application and testing. They concluded as follows:

The 12 articles with five or more lemma generated 237 citations to them in the following (approximately) quarter century. Nine of the 237 citing articles contained empirical data, two had empirical data that had something to do with the propositions of the original article, and none had a definitive test leading to an acceptance or rejection of a proposition of the original article. In short, the 12 originating articles have to date defined no operational propositions. (Coelho and McClure 2008: 82–83)

Epilogue to Klein and Romero (2007): The editors of the *Journal of Economic Theory* were invited to reply to the article, but did not. The author

of one of the *JET* 2004 articles, John Quiggin, wrote to us about our article and its treatment of his *JET* article (Quiggin 2004). The email exchange is reproduced in a subsequent issue of *Econ Journal Watch* as journal correspondence.⁶ We had scored Quiggin's article as passing *Theory of what?* but failing *Why should we care?* and *What merit?* Quiggin protested the latter scores and reproduced text from his article to meet those questions; that part of the exchange is easily accessible at the link provided in note 6. I replied as follows:

Pedro and I recognize that we might have mis-scored the paper. We looked again. It still seems to us that the paper does not say much to answer *Why should we care?* Can you elaborate on a real-world problem that your formulation helps to resolve? Do you maintain that your formulation is a better way to get at the results of traditional investment models? If so, better in what way? Is the betterness mainly a matter of economic insight, or mathematical sophistication? We still find that the motivation is obscure.

We understand that asset price standard deviation may not really get at risk meaningful to investors. Is overcoming that failing the paper's motivation? Does your formulation get at how investors think of risk? If so, we still have trouble seeing that motivation. (Klein, as part of Quiggin 2007: 360)

John Quiggin concluded the exchange with the following reply:

I suppose our disagreement comes down to tastes in how you write articles. I don't think it's necessary to convince an audience of economists that price determination in asset markets is an important problem, and having suggested a potential improvement interesting enough to satisfy the referees, I'll leave it to the profession to judge whether or not it's worthwhile. You obviously feel these points should be spelt out more in the article.

As a result my interpretation of the message of your article is "*JET* articles aren't written the way we (Klein and Romero) would like". This is fair enough, but fails, for me, to answer your own question, "Why should we (the profession in general) care?" (Quiggin 2007: 360)

To my mind, Quiggin's reply is unsatisfying. The exchange provided him an easy and open opportunity to answer the further questions put to him in my reply, and thus the two questions on which we had failed his article. Instead he invoked the authority of the *Journal of Economic Theory*—the very thing that our article was testing.⁷

The Three Questions Turned toward Swedberg's Theorizing Project

In interrogating the *JET* articles, the three questions—*Theory of what?*, *Why should we care?*, and *What merit in your explanation?*—were used to check whether the article provided *some* answer, *any* answer, to each of the questions. In turning now to Swedberg's theorizing project, the "we" of that circle is mostly free of any penchant for irrelevant and useless models. For Swedberg's theorizers—or, let us say, "our theorizers"—the questions may be useful in getting them to learn to give for each question not just some answer, but a *good* answer.

Now I speak to our theorizers as coach or counselor—one appointed by Swedberg. We turn the questions to our theorizers in a genial manner. There is no need to carefully separate the questions and hold anyone's feet to the fire. For our theorizers, the questions work as a set and may be managed as a set.

Running like a spine through the questions is the explanandum-explanation complex. In the complex, the explanandum and the explanation are counterparts. That means that whenever you speak of one, you should be able to speak of the other, as of parent and child.

Swedberg's article gives a prominent place to the naming of central concepts. He notes that neologisms often horrify (2012: 20), yet I propose a term for "the explanandum-explanation complex," viz., "explexus." The first four letters—"expl"—correspond to those of *explanation* (as well as the Latin *explanare*), and "plexus" connotes a complex of some mystery, like the complex of nerves that runs under each clavicle, the brachial plexus.

Our terminology differs from that used by Carl G. Hempel and Paul Oppenheim in their famous article "Studies in the Logic of Explanation." They write:

We divide an explanation into two major constituents, the explanandum and the explanans. By the explanandum, we understand the sentence describing the phenomenon to be explained (not the phenomenon itself); by the explanans, the class of those sentences which are adduced to account for the phenomenon. (Hempel and Oppenheim 1948: 136–37)

For the thing to be explained, all of us use "explanandum." But for "those sentences which are adduced to account for the phenomenon," they use

“explanans.” But the ordinary word “explanation” suffices for what they call “explanans.” They, however, use “explanation” for what I am calling “explexus.” It seems to me that it is the larger complex consisting of the “two major constituents” that calls for an exotic term (my “explexus”).

One might ask: Rather than introduce a neologism, why not just call the complex “theory”? Indeed, Swedberg writes of “developing a theory, including an explanation,” and of “completing the tentative theory through an explanation.” He says, “a fully built out theory includes an explanation” and “the theorizing process is not over till an explanation has been produced” (Swedberg 2012: 14, 16, 25).

But Swedberg also says, “Theorizing . . . means an attempt *to understand and explain something . . .*” (14, emphasis added). The word “theory” holds several facets aside from explanation, including understanding, prediction, and control. The facets are distinct but they relate to one another. When one sees, appreciates, or knows a good explexus, one better understands. In providing a good explexus, then, one advances understanding. Also, in providing an explexus one may aid prediction or control: If you believe a theory that smoke is caused by fire, then when you see fire, or someone fixing a fire, you might predict smoke. Also, you can, to some extent, control smoke by controlling fire. Explanation, understanding, prediction, and control are distinct but they tend to move together. Perhaps one could focus on understanding, and set up “the thing to be understood,” and so on. But explanation seems most worthy. Explanation connotes statements and discourse, whereas understanding may be only tacit. As for prediction, it is often too ambitious, and control sounds menacing. If a theory can be said to have a spine, that spine is the explexus.

By the way, while Swedberg’s essay provides wonderful and copious connections to thinkers who have treated of theorizing, there is no mention of David Hume, who said that “philosophical decisions are nothing but the reflections of common life, methodized and corrected” (1748: 170). Hume’s ideas about causation and human understanding (in the first book of *A Treatise of Human Nature* and in *Enquiry Concerning Human Understanding*) are highly pragmatist. He says our minds form groupings of things based on resemblance and such, and learn to develop and focus on associations based on temporal priority and contiguity, *in whatever ways prove useful to us*. All this is the nature of causation. He places great emphasis on causation being an ordering in human minds, and, thus, in a sense, portrays all of us as theorizers. Hume associates causation with

necessity, but it is clear that the decision as to *which* necessities we shall *focus on* is made pragmatically; it is not useful to say that *the absence of heavy rainfall*, a necessary condition for a fire, caused the smoke. Likewise, Hume suggests that we formulate the thing to be explained, the explanandum, creatively and pragmatically, as when he writes:

These instances [of things that we collect mentally as a set based on their resemblance] are in themselves totally distinct from each other, and have no union but in the mind, which observes them, and collects their ideas. Necessity, then, is the effect of this observation, and is nothing but an internal impression of the mind, or a determination to carry our thoughts from one object to another. Without considering it [that is, necessity] in this view, we can never arrive at the most distant notion of it, or be able to attribute it either to external or internal objects, to spirit or body, to causes or effects. (Hume 1739: 165)

Causation, for Hume, was a matter of focal or useful associations. A “corollary” (1739: 172) was skepticism: How can one invoke an associative regularity between one set of things (explanations) and another set of things (explananda) for an event as singular and unlike all others as that suggested by the statement “God created the universe”?

So, one counsel to our theorizers is to consider putting Hume on their reading lists.

Now I reposition them from third-person plural to second-person singular. That is, now I address *you*, young theorizer! Consider putting Hume on your reading list.

Think about the three questions—*Theory of what?*, *Why should we care?*, and *What merit in your explanation?*—in tandem, or as a set. When you find problems in answering one of the questions, a remedy may be found in adjusting your answer to one of the other questions. Likewise, shape your explanandum and your explanation in tandem. Your explanandum has to dovetail with your explanation. You might improve this dovetailing by adjusting the explanandum, not the explanation. Swedberg (2012: 32) cites Everett C. Hughes (1984: 503) on looking for “likeness inside the shell of variety.” The likeness, or resemblance, is something that your theorizing may clarify and help to establish. If you think of yourself as a creator, even an artist, your creation, your work of art, is the entire explexus.

But it also pays to zoom in on one question at a time. In answering *Theory of what?*, you are disciplining yourself to make clear what it is that you attempt to explain—that is, to identify the explanandum. I trust you are not building equilibrium models like those in *JET*, but, still, for many a social science author, Leijonhufvud might nonetheless say that the work “leaves us in doubt about what exactly he or she is *talking about*.”

And giving *some* answer to *Theory of what?* is not good enough. You have to give a *good* answer. Give clear illustrations, whether historical, factual, anecdotal, or merely fictitious, as when Schelling (1960: 54) provides the parable of a husband and wife becoming separated in a department store, or when Adam Smith (1790: 181ff) gives the parable of the now-rich poor man's son who looks back on his putatively successful life. If the illustration is clear, people can relate to it. As Deirdre McCloskey (2000: 78) says, people find it much easier to move from the concrete to the general than from the general to concretes.

By multiplying and varying your illustrations of the explanandum, you may also find its boundaries. It can be helpful to bring up a concrete example that resembles your explanandum in some respects, but differs in other respects that keep it from being an illustration of your explanandum. By clarifying what you do *not* pretend to explain, you clarify what you *do* pretend to explain. You delimit the explanandum.

By giving numerous clear illustrations—and factual is a plus, all else equal—you start to build out the basis for testing your theory. Unlike the blowhard, you give the reader something with which she may respond at the end: “But you said you were going to explain *this*, and I don't see (or don't buy) your explanation.” Don't be afraid to find out that you, too, are a bit of a blowhard. What, after all, *is* your motivation to theorize? Better to find out now and begin correcting it.

Now, though an economist, I am quite permissive about the nature of explananda. What people say in such-and-such circumstances, what people think, what they feel are all viable explananda. Particular norms, practices, or moral or cultural patterns can be the explanandum.

Even what a particular person said or did on particular occasions might qualify. Why did Smith make an organon of moral sentiment being enshrouded in sympathy?⁸ To explain that would take quite a theory.

If some particular speech acts of Adam Smith may be a good answer to *Theory of what?*, that is because he is Adam Smith. Not just anyone's speech

acts will serve as well. If just anyone's served as a good answer, then all of the *JET* authors could have said that their explanandum is their saying what they say in the abstract of their article, even the articles that are purely model-building.

Here we see that theorizing is inevitably embedded in a discourse situation, entailing one or more circles of "we," and that our three questions depend on such embeddedness. Indeed, one of the questions is, "Why should we care?" One might reply: Who is "we"? That is how Quiggin replied in the exchange succeeding the critique of *JET*. He essentially said: My "we" is not your "we," and I abstain from entering further into your challenges of my article in *JET*.

Minding the three questions, then, impels you to mind your "we." Conflict is ineluctable among interpretations and judgments, and hence among circles of "we." They compete with each other. Along with fashioning the parts of your explexus, you are also choosing scholarly traditions and communities.

You can hope to innovate and alter those communities, but any such innovation proceeds as a development on them. Our third question—*What merit in your explanation?*—insists that new explanations of previously identified explananda show their merits in relation to old explanations. Innovations are new, but the standards by which they are judged are usually old and change only incrementally. Thomas Kuhn (1959) spoke of the relationship between scientific tradition and innovation as "the essential tension."

If a merit of your explanation is that it explains more reliably or more neatly than the rival explanation, mind the quantitative aspect. *More* reliably or *more* neatly can often be treated quantitatively. Quantification is not necessarily something apart from theorizing. It may inhere in theorizing. Make your quantifications as transparent, as accountable, as possible. Besides answering merit, quantification will help show why we should care and what you are theorizing about.

Rules May Be Loose, Vague, and Indeterminate

So to aid your appreciation of theoretical work and improve your own, think about explexus and the three questions. If we believe that explanation and understanding are related in a quite general way, we might try to discern

the explanatory aspect of most any work in the social sciences, since, even if empirical, historical, or discursive, it presumably proposes to aid understanding of something in some way.

Consider the present work. It would not be called theoretical or explanatory. Perhaps it would be called an essay. Yet the *explexus lens* can be applied. The thing to be explained, the *explanandum*, is some features of our sensibilities of good theory, our judgments about whether a theory is worthwhile. We cannot hope to render such sensibilities “precise and accurate,” as Smith would say; the aim is not to make them grammatical. Rather, they are “loose, vague, and indeterminate” (Smith 1790: 175–76; see also 327). But that is not to say that they are arbitrary. The Smith material drawn on here is a discussion of the rules of virtues. Even loose, vague, and indeterminate rules *are rules*. He likens them to “the rules which critics lay down for the attainment of what is sublime and elegant in composition.” Although “there are no rules whose observance will infallibly lead us to the attainment of elegance or sublimity,” “there are some which may help us, in some measure, to correct and ascertain the vague ideas which we might otherwise have entertained of those perfections.” The three questions (*Theory of what?*, *Why should we care?*, and *What merit in your explanation?*) are helpful disciplines in theorizing and in the assessment of theory. I thusly explain—albeit only partly—our judgments about theorizing: Works that we praise have good answers to the three questions, and those we blame often do not. How well the author answers the three questions partly explains our judgments about his or her theory. That is the spine of my nascent theory.

It is not possible here to provide many illustrations of the *explanandum*, or many demonstrations of the explanation. That impossibility is part of why this chapter would be called an essay and not a theoretical work. This essay hopes only to offer a number of illustrations, to begin to develop a theory and begin to test it.

Illustrations and demonstrations might be divided into two categories on either side of the benchmark of “mediocrity,” which Smith calls the point of “propriety”: Above propriety are things that are praiseworthy and below, blameworthy (Smith 1790: 26, 27, 80). The article that Romero and I wrote blames a majority of the articles in the 2004 volume of *JET* for failing to satisfy some of the basic proprieties of theorizing. Here, however, I do not examine any work deemed blameworthy. To do so would be difficult, given our

lack of common knowledge of any such work, and insufficient space to make criticism fair.

The Three Questions and Coleman and Hoffer (1987)

In their book *Public and Private High Schools: The Impact of Communities*, James S. Coleman and Thomas Hoffer (1987) integrate theory, evidence, and narrative. The integration is rich and persuasive. The messages are of great moment. Coleman and Hoffer work with different categories of school—public, Catholic, and other private categories—and investigate differences in how well the students do. The jacket of the book says: “the authors are able to point to the crucial role of school-community relationships in explaining these differences.” The explananda are patterns of differences in how well the students do, including the *growth* of academic learning over a two-year period; the explanations are differences in the features of communities to which the families belong.

The explexus, in simplified form, is the following: Students in Catholic school do better than those in public school and in other categories of private school, and “the first prominent explanation for the difference is the functional community that exists around a religious body to which the families adhere and of which the school is an outgrowth” (214). The mechanisms of functional community are conveyed throughout the book, as when the authors write: “The feedback that a parent receives from friends and associates, either unsolicited or in response to questions, provides extensive additional resources that aid the parent in monitoring the school and the child . . .” And: “[T]he norms that parents, as part of their everyday activity, are able to establish act as important aids in socializing children” (7). Such resources are discussed also in terms of social capital, networks, and intergenerational closure—the parents have relationships with one another. Coleman and Hoffer emphasize the intergenerational dimension, noting that religious life is “one of the few remaining strong bases of functional community in modern society which includes both adults and children” (215).

The book’s answers to *Theory of what?* and *Why should we care?* are clear. The interesting question is, *What merit in your explanation?*, which in this case becomes, *How’s yours better?* How is the functional-community explanation better than the other explanations?

One explanation might be that Catholic schools spend more money per pupil, but the opposite is true (35). Other explanations are separated out by virtue of control variables, on family income and structure, parental education, race, and so on. Issues of missing variables and selection effects always haunt such a study, but for the results on the students' academic *growth*, these concerns are greatly reduced, since a student's twelfth-grade performance on the test is effectively controlled for by *that student's* tenth-grade performance. For the evidence about what happens to students in their years immediately after high school, and so on, the selection issues may be more serious—and presumably have been taken up in the great deal of subsequent literature on the book.

A big point of the book is to highlight the importance of functional community in relation to “value community”—that is, same values among the families. Value community is strong when private families converge on some independent private school to their liking. But “[i]n the private sector, many schools are based on value communities that have little grounding in functional communities” (12). Catholic schools have functional community, and that is Coleman and Hoffer's explanation for why they generally do better.

Coleman and Hoffer engage *How's yours better?* candidly and scrupulously. They write: “We attempted to find an alternative explanation for the exceedingly strong Catholic school effect on the dropout rate . . . One possible explanation concerned Catholic religion *per se*: It might simply be that Catholic students were less likely to drop out of school than others, whether in a Catholic school or not, because of greater social integration in the Catholic religious community itself” (214). They summarize their argument for the relative merit of the functional-community explanation: “But this did not account for the difference, for while Catholics were slightly less likely to drop out of public school than were non-Catholics, they were no less likely to drop out of Catholic schools. Nor was it true that the degree of integration with the religious community, as measured by frequency of church attendance, was responsible for the special protection against middle-class dropout provided by Catholic schools” (214). But they are careful to qualify the argument, being fair to the competing explanation: “However, . . . those who regularly attended church, whether Catholic or not, [were] considerably less likely to drop out than those who never attended church” (214).

One might challenge the book on *How's yours better?* by probing whether the evidence can be interpreted within the kind of explanation we would

associate with Milton and Rose Friedman (1980: 140–78), that, especially for an excludable service like schooling, private enterprise usually outperforms government enterprise, because private owners have decisive authority over their resources, are residual claimants, and must satisfy their trading partners to win their support and participation. Such probing would need to look more carefully at the results for the non-Catholic private schools. Coleman and Hoffer acknowledge that small sample sizes become a problem if types of non-Catholic private schools are subdivided (e.g., 98n). Also, the reader finds that the non-Catholic schools are treated differently in different places in the book, sometimes with a “high performance private” category—for which a ceiling effect on the results of the student tests poses a serious data problem—and sometimes not. Also, families sometimes pony up for private schooling because their child is troubled (or *is* trouble)—a point the book might have given more attention (see 97–99, 112–113, 117, 234)—which raise concerns about selection effects (especially for the forms of evidence other than student academic growth). Coleman and Hoffer do not directly engage the Friedman explanation. The explanation of Coleman and Hoffer highlights “a set of resources which are not provided by schools themselves, but are provided by these social relations that exist among the parents of students in the school” (216). Is that explanation something apart from the Friedmans’ explanation, or is it an application and development of the Friedmans’ explanation?

In the book the authors richly develop the explexus. They are also praiseworthy for being willing to step above the explexus and ask: What important things does the explexus leave out of view? One dimension beyond their data is whether “a functional community can strengthen the advantages of the already-advantaged and block the opportunities of the disadvantaged” (232), and they refer to the “graphic portrayal” provided by August Hollingshead’s *Elmtown’s Youth* (1949). Thus they pause to recognize that certain families may wish to lighten the effect of the functional community in which they are embedded.

A Co-authored Work of Mine, on Urban Transit

There is a strong tradition of transportation researchers at the University of California, Irvine, and when in 1989, at age 27, I joined the faculty there, I fell in with them. During the ensuing years, Adrian T. Moore and Binyam Reja, then doctoral students, and I wrote a book called *Curb Rights: A Foundation for Free Enterprise in Urban Transit* (1997a). It is fair to say that the

book offers a theory. The reception has been mixed: The book does not get cited much, and yet many reviews in leading journals were quite positive.⁹ We also published an article that offers a condensation (Klein, Moore, and Reja 1997b).

I see value in thinking of transportation systems as consisting of three parts: vehicles, routes (or corridors), and terminal capacity. In maritime transportation we have boats, water routes, and ports. In air transportation we have airplanes, flight routes, and airports.

Looking back on my years in the transportation community at UC Irvine, the community was fairly conventional in the sense that its purposes were oriented toward status quo arrangements. Much of the research was funded by government agencies. Its theoretical bearings tended to be bound by a posture of looking to advise policymakers on how to tinker with the status quo. Such an outlook is characteristic of academic transportation research generally.

In the seminar discussions about urban transit, we would discuss such things as urban development patterns, commuting behavior, transit's competition from the private automobile, ridership rates, vehicle capacity utilization, economies of scale, network densities, and the optimization of routes and schedules. My background differed from that of others in that I had come up through libertarian intellectual communities. In actual practice, transportation is, of course, heavily governmentalized: Many of the key resources are government-owned, much of the expense is paid by the taxpayer, and there are many critical restrictions on voluntary private action. I questioned much of the status quo. Shouldn't transportation be more of a spontaneous order? The idea is that a tolerably good foundation of property rights and freedom of contract conduces to a system of activities that achieve coordination, including innovation, better than does a more governmentalized system. Confining our thinking to road-based transit (not rail), Moore, Reja, and I pondered how private ownership and entrepreneurship could function and studied different actual urban transit episodes. We came to focus on a part of the urban transit system that seemed to escape attention, namely, the capacity of terminals: bus stops, bus stations, curb zones, taxi stands, and staging and congregation areas. Terminal capacity had been rather invisible in urban transit research, but the more we pondered it the more we saw how critically important its role is. In pondering free-enterprise urban transit, it was not sufficient to consider private ownership of bus fleets or deregulation of fares, routes, and

schedules. Terminal capacity—“curb rights”—was an essential piece of the puzzle.¹⁰

One of the key issues in route-based urban transit is interloping. Suppose you invest in setting up a scheduled service, but opportunistic jitneys—small, unscheduled vehicles, such as vans, that ply a route—swoop in to offer rides on attractive terms to the passengers waiting at the stop, passengers who might not congregate there if they could not fall back on your scheduled service. If interloping is rampant, then it might be impossible to maintain scheduled service. And if the scheduled service then folds, maybe the passengers will no longer congregate and even the jitneying will unravel. The anchor of scheduled services gives rise to interlopers, but the interlopers dissolve the anchor.

In other cases the market might be so thick that jitneying can thrive even in the complete absence of scheduled service. So how should jitneying be handled? Our thought, in brief, is that the matter at each “terminal” should be decided by the entrepreneur who owns the property or holds a long-term lease from the local government to the curb zone along the street. On a foundation of property rights, interloping becomes a matter of trespass.

You might think: But in the United States we don’t have problems of interloping at bus stops. That is largely true: The bus stops are government property and people abide by laws against unauthorized use of them. But would it be possible to prevent interloping and yet enjoy the virtues of the invisible hand? Local governments could lease out curb zones to private parties, in a way to foster competition among them. Another key would be free entry—that is, freedom of entrepreneurs to develop terminal capacity on private property as they see fit; that would require the liberalization of restrictions that now prevent them from doing so.

There are variations in curb rights within the United States today. Moreover, different times and different places in different countries have exemplified variations in curb rights that make for different episodes or experiences in urban transit. (Here space does not permit enumeration of notable different experiences; see the book, pp. 33–79). The different historical and international experiences constitute an array of different types of experience. We schematize the array as a typology (pp. 91, 128). The typology is our explanandum. Our explanation of the different types of transit experience is the variation in curb rights (and other conditions, notably the thickness of the market). Our analysis also helps to explain how various problems in urban transit relate to

weaknesses in the underlying structure of property rights in terminal capacity (pp. 107–15). Our chief explanatory factor—“curb rights”—lends itself to policy reform suggestions, which are provided at the end of the book. Even if the reform suggestions are not possible politically, the theory helps us understand road-based transit.

Abduction and the Three Questions

I continue the memoir of Adrian Moore, Binyam Reja, and me doing *Curb Rights*, but now turn to the three questions and Swedberg’s topic of abduction. As for the first question, *Theory of what?*, we had somehow learned enough about different transit experiences to have a body of material to fashion into an explanandum. The impulse toward historical and comparative study was natural among us: Moore had a master’s degree in history, Reja came from Ethiopia, and I was researching the history of private toll roads. We read enough of the literature on different transit experiences to feel that we knew of all the different kinds of transit experience that had been given scholarly treatment. Swedberg (2012: 8) quotes Pasteur’s dictum, “chance only favors the prepared mind.”

As for the question, *Why should we care?*, real human troubles involve urban transit, giving rise to problems and issues. There is strong interest in mitigating the problems and addressing the issues. Transit policy is obviously a matter that many people care about, and for good reason.

Before addressing the third question, *What merit in your explanation?*, something should be said about coming up with the explanation. Did it involve a creative flash, an abductive leap?

I think it is important to say that, from the very start of our collaboration, Moore, Reja, and I shared a broadly libertarian outlook. In a sense, we had a broad theory from the beginning, namely, something to the effect that social affairs work better when resources are privately owned and people are free to compete within the bounds of property, consent, and contract. But such a theory is unoriginal. If we were to make some such theory worthwhile, we would have to sharpen it and move it beyond free-market commonplaces. We impelled ourselves to do so by addressing the question, *What merit in your explanation?*, and, in particular, *How’s yours better?* That is, how is our explanation better than the familiar—and briefer—rehearsals of free-market verities? By asking yourself the tough

questions, again and again, you feel impelled to come up with something that is better.

Something that is quite crucial to whatever success one might ascribe to *Curb Rights* is an equilibrium model presented in diagrammatic form (pp. 86–89). The model plots activity at a curb zone and involves two functions, passenger congregating and jitneying, and the domain of one is the range of the other—that is, congregating is a function of the number of jitneys that come by per hour, and the number of jitneys that come by is a function of the number of people who congregate at that curb zone. A diagram of this kind can accommodate both stable and unstable equilibria and illustrate tipping-point dynamics.

Somehow I came up with the diagram. I had had training in game theory and had written game-theoretic papers with tipping points. Also, I was an avid reader of Thomas Schelling, and the diagram is very similar to ones in *Micromotives and Macrobehavior* (Schelling 1978a). If there was an abductive moment in our project, it was when the diagram was first drawn: Different variants of the model, or different equilibria (or disequilibrium dynamics) within the models, correspond to different types of transit episodes, which are distinguished principally by differences in curb rights. I think I first drew the diagram alone in my home. But I do not recall anything more than that.

Our model is what makes the connection between explanandum and explanation as elegant as it is. Such elegance is one answer to the question, *What merit in your explanation?* The model helps to show how just one or two differences in conditions give rise to different types of outcomes, with the outcomes bearing resemblance to different types of transit experience.

Another merit of our explanation is that it helps us understand how defects in transit systems relate to features of the rights regime. In particular, the theory helps us see that many transit services in the United States are inert and unresponsive because there are critical restrictions on private action, because so much of the system is owned by governments, and because there are pervasive subsidies from taxpayers (and those subsidies are rendered on the producer, rather than user, side of the market). In this respect, our theory helps to incorporate the topic of urban transit into a broader understanding of how social coordination is related to the rights regime.

Although I do not recall much about the abductive moments of my work with Moore and Reja, these recollections might be helpful in understanding the process of successful abduction (if we may suppose the work a success).

It is hard to say how you come up with a theoretical insight. But once you have an insight, you can explore it. The better prepared you are, the better you can assess its merit, promise, and potential. Ask yourself the three questions, *Theory of what?*, *Why should we care?*, and *What merit in your explanation?* Your job as a theorizer is to formulate a complete explexus, consisting of both an explanandum and an explanation. Ask the questions again and again. That will help to direct your reading and thinking. In your knot of scholarly companions and peers, make a custom of asking each other the three questions. Challenge each other: *Theory of what?*, *Why should we care?*, *What merit in your explanation?* It will make you and your colleagues ever better prepared. Ask yourself the three questions to test your theorizing as it develops. The test results will help you refine and direct your theorizing.

An important ingredient is hope. Challenge yourself and your peers in a way that offers hope. Ask the questions as an aid to finding the way forward, not as a way to stifle theorizing and silence yourself or your peers.

Then we hope that abductive breakthroughs will come.

Chapter 5

I thank Jason Briggeman and Hannah Mead for valuable copyediting and Deirdre McCloskey, Thomas Mayer, Niclas Berggren, Richard Swedberg, and two referees for helpful comments.

1. The paper is available at <http://econjwatch.org/articles/model-building-versus-theorizing-the-paucity-of-theory-in-the-journal-of-economic-theory>. It was published in 2007 in *Econ Journal Watch*, of which I am the chief editor and four others are co-editors. The journal's policy is that when an editor submits something to the journal it must be reviewed and approved by two other editors, as was the article in question.

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

3. The spreadsheet is available online at <http://www.econjournalwatch.org/pdf/KleinRomeroAppendixMay2007.xls>.

4. That article was Article #16 as listed in the spreadsheet.

5. 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). 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 usually be termed "theory" within the academic culture.

6. The correspondence with Quiggin is available at <http://econjwatch.org/articles/why-should-we-care-what-klein-and-romero-say-about-the-journal-of-economic-theory>.

7. Another reaction to Klein and Romero (2007) came from Robert Goldfarb and Jon Ratner (2008). Although they share our concerns, they explore a diversity of understandings of the terms "model" and "theory" and criticize us by suggesting that we used definitions of those terms that are not definitive, and that, with other definitions, an investigation may well have deemed more of the *JET* articles to qualify as theory.

8. The most notable occasions of his doing so are Smith (1790: 17, fn * on 46, 163–65, 193, 306); and Smith (1977: 49).

9. Find review excerpts at <http://econfaculty.gmu.edu/klein/Assets/curbRtsPraise.html>.

10. The idea of private ownership could be extended also to the streets, an idea discussed in Klein (2012: Chap. 11).

REFERENCES

- Akerlof, George A. 1970. The Market for 'Lemons': Quality Uncertainty and the Market Mechanism. *Quarterly Journal of Economics* 84(3): 488-500.
- Booth, Wayne C. 1974. *Modern Dogma and the Rhetoric of Assent*. Chicago: University of Chicago Press.
- Chatterji, Shurojit and Sayantan Ghosal. 2004. Local Coordination and Market Equilibria. *Journal of Economic Theory* 114(2): 255-279.
- Coleman, James S. and Thomas Hoffer. 1987. *Public and Private High Schools: The Impact of Community*. New York: Basic Books.
- Coelho, Philip R. P. and James E. McClure. 2008. The Market for Lemmas: Evidence that Complex Models Rarely Operate in Our World. *Econ Journal Watch* 5(1): 78-90. [Link](#)
- Friedman, Milton and Rose Friedman. 1980. *Free to Choose: A Personal Statement*. New York: Avon Books.
- Gibbard, Allan and Hal R. Varian. 1978. Economic Models. *The Journal of Philosophy* 75: 664-677.
- Goldfarb, Robert S. and Jon Ratner. 2008. "Theory" and "Models": Terminology through the Looking Glass. *Econ Journal Watch* 5(1): 91-108. [Link](#)
- Hausman, Daniel M. 1992. *The Inexact and Separate Science of Economics*. Cambridge: Cambridge University Press.
- Hempel, Carl G. and Paul Oppenheim. 1948. Studies in the Logic of Explanation. *Philosophy of Science* 15(2), April: 135-175.
- Hollingshead, August B. 1949. *Elmtown's Youth*. New York: John Wiley.
- Hughes, Everett C. 1984. *The Sociological Eye: Selected Papers*. New Brunswick: Transaction Press.
- Hume, David. 1748. *An Inquiry Concerning Human Understanding*. Edited by C.W. Hendel. New York: Bobbs-Merrill, 1955.
- Hume, David. 1739/1740. *A Treatise of Human Nature*. Edited by L.A. Selby-Bigge and revised by P.H. Nidditch, 2nd edition. Oxford: Clarendon Press, 1978.
- Klein, Daniel B. 2012. *Knowledge and Coordination: A Liberal Interpretation*. New York: Oxford University Press.

- Klein, Daniel B., Adrian T. Moore, and Binyam Reja. 1997a. *Curb Rights: A Foundation for Free Enterprise in Urban Transit*. Washington, DC: Brookings Institution Press.
- Klein, Daniel B., Adrian T. Moore, and Binyam Reja. 1997b. Curb Rights: Eliciting Competition and Entrepreneurship in Urban Transit. *The Independent Review* 2(1), Summer: 29-54. Online: <http://econfaculty.gmu.edu/klein/PdfPapers/CURB.PDF>
- Klein, Daniel B., and Pedro P. Romero. 2007. Model Building versus Theorizing: The Paucity of Theory in the *Journal of Economic Theory*. *Econ Journal Watch* 4(2): 241-271. [Link](#)
- Kuhn, Thomas S. 1959. "The Essential Tension: Tradition and Innovation in Scientific Research." Originally published in 1959, reprinted in *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977, 225-39).
- Leijonhufvud, Axel. 1997. Models and Theories, *Journal of Economic Methodology* 4(2): 193-98.
- Mayer, Thomas. 1993. *Truth versus Precision in Economics*. Hants. Edward Elgar.
- McCloskey, Deirdre N. 2000. *Economical Writing*. 2nd edition. Waveland Press.
- Polanyi, Michael. 1963. *The Study of Man*. Chicago: University of Chicago Press.
- Quiggin, John. 2004. Invariant Risk Attitudes. *Journal of Economic Theory* 117(1):96-118
- Quiggin, John. 2007. Why Should We Care What Klein and Romero Say About the *Journal of Economic Theory*? *Econ Journal Watch* 4(3): 359-360. [Link](#)
- Schelling, Thomas C. 1960. *The Strategy of Conflict*. Cambridge: Harvard University Press.
- Schelling, Thomas C. 1978. *Micromotives and Macrobehavior*. New York: Norton.
- Smith, Adam. 1976 [1776]. *An Inquiry into the Nature and Causes of the Wealth of Nations*. Oxford: Oxford University Press. [Link](#)
- Smith, Adam. 1790. *The Theory of Moral Sentiments*. Indianapolis: Liberty Fund, 1982. [Link](#)
- Smith, Adam. 1977. *The Correspondence of Adam Smith*. Eds. E.C. Mossner and I.S. Ross. Oxford: Oxford University Press.

- Strassmann, Diana. 1994. Feminist Thought and Economics; Or, What Do the Visigoths Know? *American Economic Review*, Papers and Proceedings 84(2), May: 153-158.
- Sugden, Robert. 2002. Credible Worlds: The Status of Theoretical Models in Economics. Pp 107-136 in Uskali Mäki, ed., *Fact and Fiction in Economics: Models, Realism and Social Construction*. West Nyack, NY: Cambridge University Press.
- Swedberg, Richard. 2012. Theorizing in Sociology and the Social Sciences: Turning to the Context of Discovery. *Theoretical Sociology* 41: 1-40. [Link](#)
- Weintraub, E. Roy. 2002. *How Economics Became a Mathematical Science*. Durham: Duke University Press.
- Wootton, Barbara. 1938. *Lament for Economics*. London: George Allen & Unwin.