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SYMPOSIUM: TRAILBLAZERS TOO LIGHTLY MENTIONED?

Trailblazers Symposium: Introductory Remarks

In this issue, in lieu of the Comments section, the journal features the symposium: *Trailblazers Too Lightly Mentioned?* The symposium consists of three articles, but others might extend the symposium in an upcoming issue.

The articles speak of eminent economists advancing lines of thinking without due recognition of classical-liberal trailblazers. The pattern suggests a syndrome, which would raise the question, Why such a syndrome?

The three articles criticize a number of authors for neglecting the trailblazers. As is the policy at this journal, the commented-on authors have been invited to reply. They have declined as yet, but the invitation remains open.

—Daniel Klein
May 2007

SYMPOSIUM: TRAILBLAZERS TOO LIGHTLY MENTIONED?

The Economic Analysis of Constitutions: Fatalism Versus Vitalism^ε

CHARLES B. BLANKART AND GERRIT B. KOESTER*¹

[ABSTRACT](#)

SCIENTIFIC PROGRESS SOMETIMES OCCURS IN QUANTUM LEAPS, AS when seminal contributions challenge well-established convictions and trigger whole new research programs. Thomas Kuhn (1962) studied these quantum leaps in his theory of “the structure of scientific revolutions.”

According to Daron Acemoglu (2005), the field of political economy has witnessed such a revolution with Torsten Persson and Guido Tabellini’s *The Economic Effects of Constitutions*. Acemoglu writes in his review in the *Journal of Economic Literature*.²

Therefore, I believe that overall PT [Persson and Tabellini] have largely achieved their ambitious aim of revolutionizing comparative political economy, and this book is the most significant contribution to this field since Lipset’s work [Lipset 1959] almost fifty years ago. PT have not only pushed comparative political economy forward, but they have provided a set of findings that will challenge all

^ε Among those invited to reply to this article were Torsten Persson and Guido Tabellini, who answered in correspondence that they felt the article is very close to Blankart and Koester’s *Kyklos* article (2006), and that they refer readers to the reply in *Kyklos* by Alesina, Persson, and Tabellini (2006). *Ed.*

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1 Some of the material in the article appeared in our related criticism in *Kyklos*, Blankart and Koester (2006). That criticism drew a reply from Alberto Alesina, Torsten Persson, and Guido Tebellini (2006).

2 Large parts of the Acemoglu review deal with the econometric testing of central hypotheses of PT’s book. In this comment we focus on the general approach.

economists and social scientists, and likely pave the way for a large body of new work in this area. (Acemoglu 2005, 1043)

Acemoglu suggests that what PT have achieved amounts to more than the entire comparative politics literature of the past fifty years:

Only a very brave or uninformed scholar could attempt to write on comparative political economy without seriously studying this book, and only a very stubborn researcher would have his or her posteriors remain unchanged after studying it. PT have already achieved something very few scholars can: a body of work for not only the current generation of researchers, but also for the next generation. In fact, the impact of the book might even be greater than this discussion suggests. If the results indeed correspond to the causal effects of the form of government and electoral rules on policies and economic outcomes as PT claim, we have learned more with this book than from the entire comparative politics literature of the past fifty years. (Acemoglu 2005, 1033)

A BLINKERED VIEW

For not-so-brave and not-so-uninformed scholars, however, Acemoglu's evaluation of *The Economic Effects of Constitutions*, and the book itself, contain some surprises.

Acemoglu quotes from the introduction of the book, where Persson and Tabellini describe the objectives of their inquiries:

We would like to answer questions like the following: If the United Kingdom were to switch its electoral rule from majoritarian to proportional, how would this affect the size of its welfare state or its budget deficit? If Argentina were to abandon its presidential regime in favour of a parliamentary form of government, would this facilitate the adoption of sound policies toward economic development? (Persson and Tabellini 2003, 7, and quoted by Acemoglu 2005, 1027)

Questions of comparative constitutional analysis such as these have been studied for a long time by economists of the so-called public choice tradition³, e.g., in *The Calculus of Consent* by Buchanan and Tullock (1962) and *Constitutional Democracy* by Mueller (1996), and in the huge literature based on such works, including the journals *Public Choice* (established 1968) and *Constitutional Political Economy* (established 1990). But in the whole of PT's book there is barely a

³ See Grofman (2004) or Dennis Mueller's *Public Choice III* (2003) for a discussion of the foundation of the public choice approach. We focus on the so-called Virginia School of public choice (see Mitchell 2001).

footnote to public choice (by which we mean to include the public-choice oriented literature of constitutional political economy). PT reject this literature, or even deny its existence. Right in the beginning of their book they write:

Surprising as it may seem, social scientists have not, until very recently, really addressed the question of constitutional effects on economic policy and economic performance (Persson and Tabellini 2003).⁴

Without acknowledging the public-choice/constitutional literature, PT prefer to cite their own work and the literature of political science, which they consult extensively. Likewise, Acemoglu (2005, 1043) reviews the preceding literature but refers – with the exception of the classical contributions of Arrow, Black, Downs and Hotelling and Stigler’s work on regulation - solely to the work of the political scientist and the sociologist Seymour Martin Lipset (1959).

How can researchers like Persson, Tabellini, and Acemoglu completely ignore the public choice tradition? We would argue that the reason is a very skewed reception of the public choice approach. In the guest introduction of an earlier and highly related book by Persson and Tabellini, *Political Economics* (2000), Christian Schultz writes:

Political Economics has become one of the most active research areas in the last decades. Building on earlier work of the Public Choice school, rational expectations macroeconomics, and game theory, Political Economics *has taken the next step by including rational voters, parties and politicians in the models* (Schultz 2000, xv, emphasis added).

Public choice is alluded to as a mere stepping stone in the formation of political economics, the "new approach." Elsewhere, Persson, Roland, and Tabellini articulate a rationale for such light treatment:

Traditional neoclassical theory is entirely normative and assumes a benevolent planner with a well-defined social welfare function. This has been criticized as a caricature by the Public Choice school, which argues that politicians rationally follow their self-interest. Positive Public Choice theory, however, typically relies on an alternative caricature: the malevolent Leviathan policy maker that replaces the benevolent Pigouvian planner and is solely maximizing her own rents. *The voters’ interest and the possible conflicts among them are generally disregarded, and political institutions do not play any part in the analysis.* To put it more bluntly: both traditions lack micro-political foundations. Building a bridge between these two traditions – combining their main insights – is an important task for public finance. This requires addressing the above questions regarding how well democratic institutions align the interests of

⁴ We classify economics as a social science. See e.g. Frey (1999).

voters and the incentives of self-interested politicians. (Persson, Roland, and Tabellini 1998, 686-687, emphasis added)

As public choice – in the view of political economics – is restricted to the study of the ‘Leviathan’ and includes neither voters, nor conflicts between them, nor political institutions, it becomes clear how political economists can claim the introduction of models that include rational voters, politicians, and parties to be “the next step” in the economic analysis of politics.

For a public-choice scholar, however, this view is astonishing. How could one possibly overlook the public-choice research on representative democracies that dates at least back to Anthony Downs (1957), Gordon Tullock (1967), Riker and Ordeshook (1968, 1973), and Ashenfelter and Kelley (1975), and covers a huge variety of models (e.g., deterministic voting models, probabilistic voting models, and legislative bargaining models) that all include rational voters, political parties, and politicians? Indeed, even Anthony Down’s *Economic Theory of Democracy* (1957) – one of the early contributions to public choice – already builds on the assumptions of rational voters, political parties, and politicians. More pointedly: how can one criticize public choice for disregarding political institutions when James M. Buchanan received a Nobel Prize largely for his work on the economic analysis of those very institutions? Public choice work that integrates analyses of political institutions include for example Crain and Tollison (1979) on the executive, Kimenyi, Shughart and Tollison on the judiciary (1985), or Niskanen on bureaucracy (1975).

Hence, PT and Acemoglu keep their eyes shut to public choice. As noted, they want to compare the consequences of alternative constitutions found in the world. Persson and Tabellini (2000, 2003) focus on two constitutional polarities: (1) majoritarian versus proportional electoral systems, and (2) presidential versus parliamentary forms of government. Using as a benchmark the social optimum as defined by the Samuelson condition for public-good provision and zero rents to the politicians,⁵ they find every constitutional combination to exhibit shortcomings, especially in three dimensions: the amount of public goods provided, redistributive transfers to politically powerful minorities, and rents to politicians. Which of the three is more pronounced depends on the particular constitutional combination. More variables, e.g., total government spending, adjustments to shocks, deficits, and structural policies, are analyzed, especially in PT 2003, but we focus on the main variables discussed in PT 2000.

In a parliamentary regime the legislators of the majority coalition form the government and dictate the policy. To sustain their electoral support they promote the joint interests of their voters and therefore concentrate spending on relatively broad-based programs such as public goods and general transfers. So the level of public good provision is relatively close to the “ideal level” characterized by the Samuelson condition, although the ideal level is not actually

⁵ See Persson and Tabellini (2000, 254).

reached, as the majority coalition focuses on its voters, not the whole population. But the relatively satisfactory level of public goods comes at the expense of large special-interest rents and large rents to politicians, as the government is largely unconstrained in privileging special-interest groups that are part of the majority coalition, and few checks and balances prevent rent extraction by politicians.

In presidential systems, in contrast, there is no firm parliamentary majority. Therefore powerful officeholders such as the heads of the US congressional committees dictate the agenda and try to play off one minority against another. Politicians' promotion of minority interests and the absence of a parliamentary majority make for weaker incentives than in a parliamentary system to provide public goods, resulting in underprovision. But the presidential veto power allows for better prevention of special-interest rent extraction by rent-seeking politicians than in a parliamentary system.

**Figure 1: The Economic Effects of Constitutions
(Persson and Tabellini)**

		Electoral System	
		Majoritarian	Proportional
Form of Government	Parliamentary	<ul style="list-style-type: none"> • Large rents to politicians • Large special-interest rents • Strong underprovision of public goods 	<ul style="list-style-type: none"> • Very large rents to politicians • Very large special-interest rents • Underprovision of public goods
	Presidential	<ul style="list-style-type: none"> • Very small rents to politicians • Very small special-interest rents • Very severe underprovision of public goods 	<ul style="list-style-type: none"> • Small rents to politicians • Small special-interest rents • Severe underprovision of public goods

Note: Figure based on Persson and Tabellini (2003; 2003).

With respect to electoral rules, the authors educe similar effects. Compared to a proportional electoral system, a majoritarian system leads to increased competition between the political parties and helps therefore to restrict rent-seeking activities aiming at transfers to politically powerful minorities. But underprovision of public goods is more severe, as spending is targeted only at the marginal districts (especially

if the districts are small), while the safe districts are neglected. In proportional systems, on the other hand, more – and in particular more broad-based – spending can be expected (especially if districts are large), as all votes are equally important in the election. But competition and accountability are weaker, because representatives' efforts are internalized to a lesser extent and so rents to politicians tend to be larger.

Bringing the effects together, a trade-off between limiting politician rents and public good provision results for both the choice of electoral rule and the form of government. Combining different electoral rules and forms of government leads to four main regimes, which are summarized in a simplified form in Figure 1.

In an extensive cross-sectional empirical analysis, Persson and Tabellini find their derived trade-offs at least partly supported (see, for a summary, Persson and Tabellini 2003, 269f.). The influence of electoral rules on public-good provision, rents to politicians, and rents to interest groups comes out fairly clearly. Concerning the form of government, they state that their empirical results are largely inconclusive.

A PUBLIC-CHOICE PERSPECTIVE ON CONSTITUTIONAL POLITICAL ECONOMY

Does the discussed approach of political economics, by virtue of its advances in the current literature, allow us to dispense with public choice in constitutional political economy? To discuss this question we need not delve into a detailed evaluation of the theory of political economics, nor a critique of possible weaknesses in the argument, nor a detailed examination of the empirical claims. Instead, we focus on the central character and purpose of the research, by comparing the proclaimed ends of the research with its results.

Returning to a quotation provided above, we ask: Suppose that British or Argentinean citizens are confronted with the trade-offs identified by Persson and Tabellini and summarized in Figure 1. What should they do? What assistance does political economics give them?

Persson and Tabellini might say:

Look, you are in a situation of second best. Switching from parliamentary to presidential democracy or from a majoritarian to a proportional system may not improve your situation. You may discover that you got out of the frying pan but straight into the fire and will never reach the social optimum as defined by the Samuelson condition and zero rents to the politicians, as you cannot avoid the principal-agent problem. Your political agent works under an incomplete contract, and there is nothing you can do about that. In graphical terms corresponding to Figure 2a, you remain on an inner utility possibility frontier such as BB, connecting the welfare of voters V1 and V2, and the only choice we can offer you is a bundle of alternative political systems CC. But consider that when departing from the status quo Q, you will always be

confronted with a trade-off between securing public good provision and limiting rents to politicians.'

This approach is fatalistic. All of the analysis that political economics offers keeps within the narrow range of institutions that dominate the geopolitical status quo. Nothing is said about institutional innovations. This is wrongheaded, as economic analysis, especially constitutional analysis, calls for consideration of creative solutions. The men and women who developed the constitution in the French Revolution and the founding fathers of the American Constitution were imbued with such creativity. They could not have advanced the art and practice of constitutional analysis had they merely concluded that the world is second best and cannot move much beyond the status quo. As in science generally, and especially the social sciences, researchers and philosophers should assume a spirit of *vitalism*, not fatalism.

Figure 2a:

The Political Economics Approach

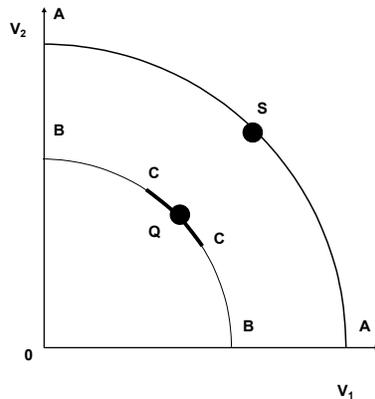
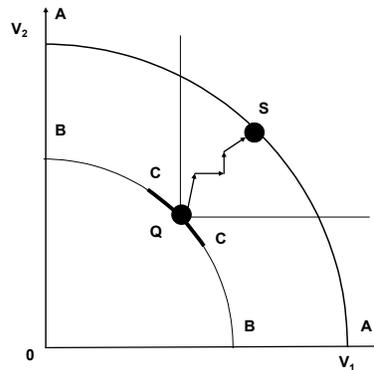


Figure 2b:

The Public Choice Approach



So why is the approach of political economics so conservative? It is typical that cultural elites hew to the status quo. We notice that political economics refers predominantly to the political science literature and largely neglects the public choice literature. Political science, according to the political scientist Hans J. Morgenthau, can be defined as follows:

Political science deals with the nature, the accumulation, the distribution, the exercise, and the control of power on all levels of social interaction, with special emphasis upon the power of the state (Morgenthau 1948).

According to this definition, political scientists ask: What are the institutions and constraints that allow the accumulation, distribution, exercise, and control of power here and now – and not under some alternative, not yet existing framework? And they focus on coercive capabilities of the state. Political economics fits into this approach, as political scientists analyze how politicians decide when confronted with a set of well-known institutions. Choice is limited by the trade-off in and around the status quo – “between the 45 yard lines,” as the analogy to American football goes.

In contrast, public choice scholars, working on the foundations laid especially by Buchanan, do not regard status quo trade-offs as inevitable constraints, but rather as *challenges*. If there is a better potential arrangement beyond the constraint CC – and Persson and Tabellini imply that there is – we may hope to find or create an institutional arrangement to achieve or approach it (see Figure 2b). Public-choice scholars characteristically ask the question: What can be done to go step by step from Q towards S? How can we improve institutions so that we come closer to S? For public choice the relevant question in constitutional analysis is not limited to the effects of existing institutions.

Hence public choice focuses on suggestions for institutional improvements based on constitutional analysis. Happily, the improvements educed by public choice thinking could indeed dissolve large parts of the basic trade-offs discussed so intensively by Persson and Tabellini, as the following examples may illustrate.

One example is public choice’s analysis and evaluation of direct democracy. Complementing representative democracy with a referendum will have the effect that the government spends less (imposes lower taxes) when the citizens want lower spending. Likewise, a popular initiative will cause it to spend more if the voters want to have higher expenditures. Using empirical evidence from different countries, public-choice scholars have shown how referenda and initiatives have decisive effects on spending, taxation, and government debt, as government prerogative decreases and accountability increases (for surveys see Kirchgässner, Feld, and Savioz 1999 and Matsusaka 2005).⁶

A second public-choice concept is decentralization. If labor and capital can migrate at low cost to other jurisdictions in a decentralized state, citizens have a larger say under decentralized than under centralized government. Public output becomes more adjusted to local preferences. Studies such as Kirchgässner (2002) show that at least on the local level, on average no cost increases occur in smaller as compared to larger jurisdictions, and scale effects are therefore often negligible.

Both direct democracy and decentralization can, therefore, be seen as important steps towards the social betterment implied by Persson and Tabellini, as their application helps to increase the supply of public goods to the level demanded by the citizens and to restrict the rents of politicians by giving a larger say to the citizens.

⁶ Indeed, the political-economics community seems to be not totally unaware of this. In a footnote, Persson and Tabellini (2003, 5) quote one older study by Pommerehne and Frey (1978), but come to the surprising statement that only very limited research has been conducted along these lines.

But public choice has not only educed complementary improvements for existing political systems, but has fundamentally challenged the view that constitutional choice is limited to majoritarian versus proportional electoral rules and presidential versus parliamentary forms of government. For example, Gordon Tullock offers the simple vote-transfer mechanism (1967). Every adult person is a member of the parliament (as in a popular assembly). Citizens can transfer their vote to anyone they so choose. The natural choice is a person whom they expect to have nearly the same preferences and to vote as they would. Those who go to parliament will vote with as many votes as they become the representative of. In this case, the advantages of personality vote (which, in the approach of political economics, is confined to a majoritarian system) can be combined with those of proportional representation. Accountability increases because representative shirking is easily observed, and voters select representatives in a contestable market. As representatives are linked more closely to their voters, the transmission of preferences into politics will be less distorted. Under such a regime, representatives would not necessarily join a fixed coalition, but rather aim at increasing their “re-election” probability by voting issue by issue as closely as possible to their voters’ preferences. And the government would no longer be either parliamentary or presidential (in the American sense). The parliament may rather appoint an executive board (like the Federal Council in Switzerland), or the citizens elect a president whose function is to arrange compromises and majorities in the parliament. To prevent free-riding, exploitation of minorities, and cycling, the parliament could decide by qualified majority rule or by one of the simple voting procedures such as voting by veto (Mueller 1978, 1984) or Hylland-Zeckhauser’s point voting procedure (Hylland-Zeckhauser 1979), so that the decisions come closer to those under unanimity rule without causing high transactions costs. Taken together, the vote-transfer mechanism, an executive board in the parliament, and a voting procedure such as voting by veto would lead to political outcomes closer to the “social optimum” than any of the political systems discussed by Persson and Tabellini (for further details see Blankart and Mueller 2002, 2004).

Hence we conclude that the contribution of political economics to the study of constitutional political economy is confined to the small “neighborhood” of the status quo; and therefore is far from displacing public choice. In particular, its almost complete disregard for public choice prevents political economics from facing the central and most important question in constitutional political economy: how to design a constitution that best aligns public policy with individual preferences? As innovation in government is clearly required, the question calls for criticism of the status quo.⁷

⁷ Much more creative than the works by Persson and Tabellini is the book *The Grabbing Hand* by Andrei Shleifer and Robert Vishny (1999), in which the authors are searching for superior institutional designs for privatization, prevention of corruption, and means of supporting market-oriented politicians in foreign-aid policy.

A SCIENTIFIC REVOLUTION?

If we translate the statements of Acemoglu – discussed in the introduction of this article - into the theory of Thomas Kuhn (Kuhn 1962), he states that Persson and Tabellini’s work on the economic effects of constitutions reflects a paradigm shift. In Kuhn’s view a paradigm shift occurs if a new approach emerges that is more plausible, is better able to explain empirical phenomena, and is in part or in whole incompatible with the existing paradigm (Kuhn 1962, Lakatos 1970). It is important to notice that this replacement occurs (as Lakatos pointed out) only if the new alternative theory contains “corroborated excess empirical content” over predecessors or rival theories – meaning that unless the new theory explains both what was explained before and new facts as well, there is no scientific reason to prefer it over the existing stock of literature (Lakatos 1970, 116 et sqq.). This concept implies as well that once a paradigm shift occurs, large parts of the preceding literature become dispensable.

In our view the achievements of Persson, Tabellini et al in constitutional political economy are not so outstanding. They focus almost exclusively on trade-offs within existing representative democracies and miss the central question of constitutional analysis: how to improve the alignment of public policy with individual preferences. Therefore their research is not able to replace public choice in this field and there is no indication of a paradigm shift. They cannot explain what was explained before by public choice and new facts as well. Quite the contrary: Public choice explanations continue to be more convincing in central fields of analysis common to both approaches. This is especially true of the newer contributions of Persson and Tabellini, which ignore public choice research almost entirely.

WHAT’S THE DIFFERENCE?

What are the reasons for the differences in the work of Persson and Tabellini and the public choice tradition? Are there more fundamental reasons why Persson and Tabellini reach different conclusions than public choice, and ignore the public choice tradition?

We would like to discuss especially two basic differences that distinguish the two approaches, leading to different questions, different research designs, and consequently different analytical results, namely: (1) the point of reference, and (2) the importance assigned to individual liberty.

1. Consent versus conflict: Political economics and public choice have different points of reference. Political economics starts its analysis within the framework of existing representative democracy (Persson and Tabellini 2000, 251 et sqq) and focuses on the coercive capabilities of the state. Within this framework political decisions are legitimate if they are supported by a majority of votes. As majority decisions allow for the exploitation of minorities, the main topics within political economics are redistributive conflicts. This is reflected in

the overview article “Political Economics and Public Finance” (Persson and Tabellini 2002), where the existing literature is classified along the dimensions of one-dimensional redistributive conflict, multidimensional redistributive conflict, and analysis of the effects of different constitutional arrangements on redistribution. The analytical results of political economics are therefore mostly trade-offs between different second-best solutions for redistributive conflicts within the political systems of representative democracies.

Public choice on the other hand starts at the level of the individual. The yardstick for the general legitimacy of the state and its decisions is the willingness of each citizen to belong to a state and accept its decisions. Thus, public choice holds voluntarism as a core consideration of constitutionalism (Buchanan 1954). Institutionally, the point of reference of public-choice scholars lies in the model of Wicksell (1896), in which a commonly elected parliament (which represents the preferences of all citizens) decides (nearly) unanimously, i.e., it bargains till (near) unanimity is reached. This process ensures that all preferences – and not just the preferences of a majority – are accounted for in the final decision. Deviations from unanimity are only acceptable if bargaining costs would otherwise be prohibitively high. New research in public choice is intended to reduce these costs by new voting procedures such as those mentioned here.

Based on this approach, public choice scholars see their main task as developing arrangements that facilitate voluntary society. This distinguishes them sharply from the approach of Persson and Tabellini.

2. Liberty versus abstract efficiency: Based on the different points of reference, the two approaches derive different criteria to evaluate political decisions and political institutions. Persson and Tabellini assign central importance to efficiency considerations of political decisions within representative democracy (Ursprung 2003, 224 et sqq). The efficiency is abstract, even mathematical, in that it resembles output:input ratios in mathematical functions, with no concern for the human processes *per se*. External consequences, not choices, are all that matter. Typical questions asked by Persson and Tabellini are: Presidential or parliamentary?, Majoritarian or proportional? The criteria to evaluate these institutional arrangements are comparisons of efficiency in categories like underprovision of public goods versus rents to politicians.

Public choice scholars on the other hand argue that abstract efficiency considerations are not the only criteria. Like the American Founders, they assign great importance to individual liberty as a criterion to evaluate political decisions and institutions. The concern for liberty flows partly from the Smithian presumption that liberty generates wealth, partly from the presumption that liberty, and the consequent individual moral responsibility, generates good moral and spiritual consequences. That is why vital explorations like Buchanan’s classic essay “Natural and Artifactual Man” (1979) belong naturally to the public choice character of political economy: “Man wants more liberty to become the man he wants to become” (112). The extent of liberty—freedom from governmental attenuations of one’s property and freedom of association—is one of the criteria

upon which a political system should be judged. A typical question asked by public choice is therefore how we can improve the institutions of representative democracy to increase individual liberty and limit imposition, exploitation, and degradation by governmental means (Buchanan and Congleton 1998). In Persson and Tabellini's work, in contrast, liberty is absent (as in so much of economics training; see Johansson 2004). Their policy conclusions, therefore, despite the integration of political processes, often are not much different from those of a benevolent dictator. Public choice, however, integrates a calculus of individual liberties and comes therefore to largely different questions, results, and advice.

CONCLUSION

For many economists, James Buchanan was a trailblazer, founding the paradigm of public choice in political economy and revolutionizing the way economists see and analyze the state. Especially upon the basis of his works, the fields of public choice and constitutional political economy emerged, to which a large family of researchers adhere.⁸ For this Buchanan was awarded a Nobel Prize.

As remarkable as it may seem, current works completely ignore this research tradition and assume that it is best to "start from scratch" in political-economic analysis.

We have tried to show that such naïveté does not lead to even rediscovery of the wheel, but rather misses central points. This is epitomized by the fact that the contributions of Persson and Tabellini on constitutional political economy have been unable to provide any meaningful advice for institutional decision – their explicitly declared scientific goal!

Scientific trailblazers open new horizons for research and should be honored for doing so by the academic community. To recognize work as pioneering and important is probably the highest academic compliment a researcher can get, and that is what Acemoglu wanted to confer in his praise for Persson and Tabellini.

Merely following existing trails makes scientific quantum leaps impossible. But to close one's eyes and pretend to be where no one has been before is to harm the people who may use the pioneers' insights to improve their political systems.

⁸ See Dennis Mueller (1985) for the relationship of the public-choice paradigm and the Public Choice Society.

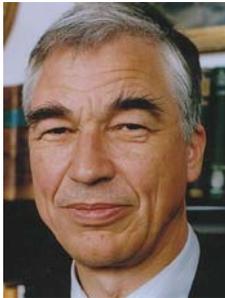
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SYMPOSIUM: TRAILBLAZERS TOO LIGHTLY MENTIONED?

The Empirical Institutions-Growth Literature: Is Something Amiss at the Top?

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ABSTRACT

The past two decades have witnessed a resurgence of economic research on the most fundamental question: What causes economic growth? The research has suggested numerous determinants such as geography, physical capital, human capital, technology, population growth, and international trade.¹ More recently, however, empirical growth research has focused on “institutions.” For example, the theme of the World Bank’s 2002 *World Development Report* was “Building Institutions for Markets.”

Although growth theory’s focus on institutions is a more recent phenomenon, economists’ acknowledgment of institutions is nothing new. In 1776, Adam Smith proclaimed that the path to economic prosperity begins with a general presumption of freedom from government intervention, and, ever since, classical liberal economists have continued the tradition (e.g., Hayek 1954, Friedman and Friedman 1980). Finally, beginning with the work of Douglas North, the link between institutions and economic performance gradually worked its way into the more academic discussions of growth theory (e.g., North and Thomas 1973, North 1990).

One obvious reason for the long-standing lack of attention on institutions in the empirical growth literature is the inherent difficulty in measuring institutions. Although measures of some aspects of institutions have existed for some time, such as the Freedom House indexes of political and civil freedom, measures of a more comprehensive view of institutions and especially economic

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¹See the excellent survey by Temple (1999) for references to the relevant empirical growth studies.

institutions have been more elusive. This changed, however, with the publication of *Economic Freedom of the World: 1975-1995* by James Gwartney, Robert Lawson, and Walter Block (1996). Their Economic Freedom of the World (EFW) Index was the most extensive measure available in terms of its coverage of countries, time, and attributes of freedom.

Several other indexes of economic freedom are also noteworthy. Wright (1982) extended the Freedom House indexes of political and civil liberties to include a rating of economic freedom, but coverage is limited to a relatively short time period. Another attempt by Freedom House to publish a measure of economic freedom appears in Messick (1996), but publication of this measure has been discontinued. Scully and Slottje (1991) construct an index of economic liberty, but this measure also has a limited time dimension. The Heritage Foundation publishes a measure of economic freedom which is similar in many respects to the EFW index, but is available for a shorter period of time (see Holmes et al 1998). The EFW index has been more widely used than any of these alternatives, most likely because of its coverage of a longer time period. Because of its widespread use, the discussion that follows restricts attention to the EFW index.

The EFW index is based on the classical conception of individual liberty, which emphasizes personal choice, private property, and freedom of exchange. An influential preliminary formulation of the index was Rabushka (1991). The EFW index currently encompasses five areas of freedom which are aggregated into a single summary index of economic freedom. The five major areas of the index are (1) size of government; (2) legal structure and security of property rights; (3) access to sound money; (4) freedom to trade internationally; and (5) regulation of credit, labor, and business. The underlying components (data) that comprise each area are listed in the Appendix. All underlying component data are converted to a scale from 1 (representing the least free) to 10 (most free). Each underlying component is equally weighted to construct an area index for each of the five areas. Then, equal weight is given to each of the five areas in constructing the EFW index (i.e., the five area indexes are averaged).² The index is available for a large number of countries in five-year intervals from 1975-1995, and annually since 1995.³

As might be expected, the publication of the EFW index prompted an explosion of empirical research on the institutions-growth relationship. A recent survey by de Haan, Lundstrom, and Sturm (2006) cites at least 28 empirical studies that use the EFW index in some form to investigate the institutions-growth relationship. They cite another 12 studies that use the EFW index to investigate the determinants of freedom itself. However, these numbers pale in comparison to the overall use of the EFW index in the literature.⁴ A recent

²Earlier versions of the index experimented with different weighting schemes and data sources.

³The current version of the EFW index is available at <http://www.freetheworld.com>.

⁴Although the opening discussion focuses on the institutions-*growth* relationship, the same general conclusions regarding publication trends apply to the larger body of recent empirical work relating

check of the Social Sciences Citation Index (SSCI) indicates 194 citations of the EFW index since its inception. Table 1 provides a complete list of the journals in which these citations have appeared. In addition, several journals not included in the SSCI, such as *The Cato Journal*, *Constitutional Political Economy*, and *European Journal of Political Economy*, have published many articles citing the index. A partial count of citations appearing in these journals is provided at the end of Table 1. This partial count, providing a total of 17 additional citations, is taken from references in the survey article by de Haan et al (2006).

Despite the healthy number of citations to the EFW index, closer examination of the citation list reveals an interesting phenomenon with respect to the use of the index in the economics literature. Specifically, very few of the citing articles have appeared in top-tier journals. The next section of the paper discusses the large literature that has emerged since the EFW index was developed and the journals in which this literature has appeared. In a separate strand of literature, a number of empirical papers appearing almost exclusively in top-ranked journals have also addressed the relationship between institutions and economic performance. These articles have rarely cited either the EFW index itself or the large body of research which uses the index. This strand of the literature is discussed in the last section of the paper.

ARTICLES CITING THE EFW INDEX

Prior to the publication of the EFW index, a relatively small number of empirical studies had addressed the role of institutions in determining economic outcomes. In his excellent review of the empirical growth literature, Temple (1999) cites only three articles in this area. These include Knack and Keefer (1995) who use indicators of property rights, Mauro (1995) who uses measures of corruption, and Barro (1997) who uses an indicator of political rights. Looking at freedom to include political, civil, and economic aspects, other early studies which include such features include Kormendi and Meguire (1985), Scully (1988), Barro (1991), and Levine and Renelt (1992). Given the small number of studies and the often narrowly-defined measures of institutional characteristics noted here, it would seem that an empirical project constructing a multifaceted measure of economic freedom would represent a significant contribution to the literature.

As noted above, at least 28 articles have been published which cite the EFW index in their analysis of institutions and growth. Numerous other articles use the index to investigate other (non-growth) aspects of institutions. However, very few of these articles appear in top journals. Only eight of the more than 194 articles that cite the EFW index appear in top-20 ranked journals based on the recent journal

institutions to other aspects of economic performance—such as investment, income levels, volatility, etc. Indeed, much of this literature grew out of the initial interest in relating institutions to growth.

rankings provided by Kalaitzidakis, Mamuneas, and Stengos (2003). Journal rankings for the articles citing the EFW index are provided in Table 1.

The journals that have published the largest number of articles citing the EFW index are *Public Choice* (17 articles), *European Journal of Political Economy* (13), *Kyklos* (9), *Economic Inquiry* (7), *The Independent Review* (6), and *Contemporary Economic Policy* (5). The highest ranked of these journals is *Economic Inquiry* (ranked 36), followed by *Public Choice* (43), and *Contemporary Economic Policy* (60). Although not included in the Kalaitzidakis et al rankings, *European Journal of Political Economy* would likely be ranked in the same general area as *Economic Inquiry*.

Dawson (1998) was one of several early empirical studies of cross-country growth incorporating a measure of economic freedom to be published after the appearance of the EFW index.⁵ This study was initially submitted to the *Journal of Economic Growth* (*JEG*), where the editor declined to publish the paper based primarily on a single referee's report. One of the referee's main comments questioned the use of the EFW index, arguing that the paper "contains absolutely no theory justifying the Gwartney freedoms indicator." A later version of the paper—still using the EFW index—was eventually published in *Economic Inquiry*. Based on the rankings by Kalaitzidakis et al, this article represents the highest ranked journal in which a study of institutions and growth using the EFW index has appeared. The point here is not to question the judgment of the *JEG* editor or referee in their review of this paper. However, the circumstances do suggest a reluctance to use the EFW index even at a time when alternative measures of economic institutions were limited.⁶

Others have also noted reluctance among many researchers to use the EFW index. In their review of the EFW-based literature, de Haan et al (2006) argue that this hesitancy is likely because researchers "doubt whether the data are reliable, given the strong ideological position of the organizations providing them" (158). de Haan et al conclude, however, "that the index is both reliable and useful" (182). There is no doubt that the EFW measure of economic freedom, as with almost any measure of anything, is not perfect, and that it may not be useful for every possible application involving the analysis of institutions. Potential concerns that may steer researchers toward other measures include the EFW project's idea of economic freedom, the occasional resort to policy *outcomes* (rather than *rules*) as components of the index, concerns about the subjectivity of the data, the choice of aggregation technique, and the handling of missing data.⁷ Nevertheless, the attempt at

⁵Other articles that use the EFW index are equally worthy of discussion and may have appeared even earlier. However, the Dawson (1998) article was selected for discussion here because of the author's specific knowledge of the history of the paper and access to relevant referee reports.

⁶The *Journal of Economic Growth* is not ranked in the Kalaitzidakis et al (2003) study (possibly because it is a relatively new journal), but it is arguably one of the top field journals in the area of concern here. Nevertheless, the main point here is not to debate relative journal rankings, but rather to establish a general reluctance regarding the use of the EFW index in empirical work.

⁷An extensive discussion of these potential shortcomings and related analysis is provided by de Haan et al.

measurement of such an elusive aspect of economic reality has made possible new understanding of the role of institutions. As Lawson (2006) describes:

A primary purpose for the creation of the EFW index was to inject some much needed scientific fact into the ongoing debate about the merits of free-market economic systems versus interventionist systems. What had characterized this debate for most of its history was a paucity of data and evidence. With the creation of the EFW index we are now in a position to begin to address the problem of economic organization as scientists should by measurement of reality and testing of hypotheses. (400)

In addition, de Haan et al note the remarkable parallel between the EFW index and the so-called “Washington consensus,” demonstrating how the main elements of reform programs suggested by the IMF and World Bank match with components of the index (see de Haan et al, Appendix).⁸

From the body of research of the last decade, a consensus has emerged. The conclusion of a critical assessment of recent evidence using the EFW index by de Haan et al (2006) is that “studies that have applied some kind of sensitivity analysis and sensible specifications generally find support for a positive relationship between changes in [economic freedom] and growth” (182).

ARTICLES APPEARING IN THE TOP JOURNALS

Of the more than 194 articles that cite the EFW project, only eight come from journals ranked in the top 20 by Kalaitzidakis et al (2003).⁹ However, these are not the only articles in top journals that have addressed the institutions issue. In the years following the initial publication of the EFW index, a completely separate strand of literature on institutions emerged—a literature which completely ignores the contributions of the EFW index and the empirical evidence based on it. This new strand of literature appears almost exclusively in the profession’s top journals. The following is a discussion of this literature. For obvious reasons, the discussion

⁸Despite the parallel between the EFW index and the Washington consensus, a group of World Bank economists now maintain their own broad measure of institutions—called “governance” indicators—that includes the rule of law, government effectiveness, political instability, and regulatory burden, among other things. Initial work on this project is by Kaufman, Kraay, and Zoido-Lobaton (1999); the EFW index is not cited.

⁹These articles include Acemoglu and Johnson (2005) in the *Journal of Political Economy*; Antras (2003), Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2003), and Glaeser, Johnson, and Shleifer (2001) in the *Quarterly Journal of Economics*; Levine, Loayza, and Beck (2000) in the *Journal of Monetary Economics*; Freeman (2006) and Cutler, Glaeser, and Shapiro (2003) in the *Journal of Economic Perspectives*; and Hodler (2006) in the *European Economic Review*. The citation of Gwartney et al in Acemoglu and Johnson (2005) appears to be a simple error, as they clearly used data from the Heritage Foundation in their study. In an apparent oversight, a paper by Easton and Walker (1997) that cites Gwartney et al appears in the *American Economic Review Papers and Proceedings*, but is not reported in the SSCI. The second author, Walker, is affiliated with the Fraser Institute.

will focus on *empirical* studies of the relationship between institutions and economic performance.

Robert Hall and Charles Jones (1999) provide one of the first empirical studies of the relationship between institutions and economic performance to appear in a top-tier journal. Their term for institutions is “social infrastructure,” which they define as “the institutions and government policies that determine the economic environment within which individuals accumulate skills, and firms accumulate capital and produce output” (84). They note the relationship between institutions and the protection of private productive units from confiscatory diversion. Conceding that an ideal measure of social infrastructure does not exist in practice, they resort to using a proxy obtained by combining two indexes: (1) an index of government anti-diversion policies; and (2) an index of openness to international trade. Two of the four major areas of the Gwartney et al (1996) EFW index relate directly to “freedom to keep what you earn” and “freedom to exchange with foreigners” (16). Thus, it would seem that the EFW index, or at least two of its underlying areas, might provide direct evidence on precisely the issues addressed by Hall and Jones. Furthermore, by the time the Hall and Jones study was published, a number of studies using the EFW index to investigate the relationship between institutions and economic performance were in print. Hall and Jones did not acknowledge either the EFW index or any of the evidence based on it. Although long editorial and publication lags might explain the lack of acknowledgement in this case, such an explanation does not apply to a number of more recent articles appearing in top journals.

Daron Acemoglu, Simon Johnson, and James A. Robinson published a series of influential articles addressing the role of institutions in macroeconomic outcomes. Indeed, in the announcement of the AEA’s 2005 John Bates Clark Medal award, Acemoglu is credited with “several papers that argue that institutions play a more prominent role in development than was generally accepted.”¹⁰ The articles appear in the *American Economic Review*, *Journal of Political Economy*, *Quarterly Journal of Economics*, and *Journal of Monetary Economics*.¹¹ As an example of this work, Acemoglu et al (2001) use average protection against expropriation risk and Acemoglu et al (2003) use a measure of constraint on the executive to estimate the relationship between institutions and economic performance. The EFW index or some of its underlying components might have been tapped for alternative, multifaceted measures of institutions. In addition, despite the extensive discussion in a nearly 100-page treatise on institutions and growth in the *Handbook of Economic Growth*, Acemoglu et al (2005) mention neither the EFW index nor any of the empirical work relating the index to economic performance.

Dani Rodrik, Arvind Subramanian, and Francesco Trebbi (2004) take on the task of determining empirically the relative importance of three potential “deep

¹⁰(http://www.vanderbilt.edu/AEA/JBCMedalist_Bio.htm).

¹¹Articles include Acemoglu et al (2001, 2002, 2003, 2005) and Acemoglu and Johnson (2005). Recall that the citation of Gwartney et al in Acemoglu and Johnson (2005) appears to be an error (see footnote 9).

determinants” of growth: institutions, geography, and trade. Their conclusion, as indicated by the title of their study, is that “institutions rule.” Their measure of institutions is a composite indicator of property rights and the rule of law. Rodrik et al note that an advantage of their measure in comparison to others used in the literature is that it “in principle captures more elements that go toward determining institutional quality” (footnote 6), suggesting a desire for a broad measure of institutions. Although it is impossible to ascertain just how broad a measure was desired, the EFW index is unquestionably more multifaceted than the measure they used and arguably allows for the broadest economic-institutions measure currently available. Despite the fact that the paper attempts to reconcile various strands of the empirical literature relating institutions, geography, trade, and growth, the EFW index was not cited nor was any of the available empirical evidence using the index to relate institutions and growth.¹²

Edward Glaeser, Rafael La Porta, Florencio Lopez-De-Silanes, and Andrei Shleifer (2004) take the study of the institutions-growth relationship a step further by asking whether institutions *cause* growth. Despite an extensive discussion of the various measures of institutions used in the literature to determine which is most appropriate for addressing causality, neither the EFW index nor any of the studies which use the index to relate institutions and growth are cited. Two earlier studies that explore the causality issue specifically are also ignored. Farr, Lord, and Wolfenbarger (1998) use the EFW index in a causality study of institutions and income levels, and Heckelman (2000) uses the Heritage Foundation’s measure of economic freedom to study causality between institutions and growth.¹³

These prominent studies were chosen as examples to illustrate the occurrence of top-ranked journal articles that do not acknowledge the contribution of the EFW project and related empirical research. Other examples are available in the literature, such as Dollar and Kraay (2003), Sala-i-Martin et al (2004), and Levine (2005). There is no question that each of these studies has contributed significantly and in an ingenious way to our understanding of the institutions-growth relationship. The point here is not to question the merits of this work. However, these studies are part of a broader effort within the profession to understand the role of institutions in the development process. The authors of the EFW index and the researchers who use it have contributed in their own right to that understanding.

Admittedly, it is impossible to make an indubitable case that certain studies *should* have used the EFW index instead of other alternatives to measure institutions. There are a number of valid reasons why any particular measure might not be suitable in certain circumstances. Availability of the measure for the desired sample period or number of countries, the desired “breadth” of the measure, issues relating to aggregation methodology or subjectivity of the data, and problems involving the selection of underlying components used to construct the index are a few potential

¹²Interestingly, in an earlier study focusing on social conflict, Rodrik (1999) cites Gwartney et al (1996).

¹³Two additional studies by Dawson (2003) and Vega-Gordillo and Alvarez-Arce (2003) use the EFW index to address the causality issue, but given the proximity in the timing of publication it is difficult to argue that these articles should have been cited by Glaeser et al.

TRAILBLAZERS TOO LIGHTLY MENTIONED

reasons. It is more difficult, however, to justify the relevant top-journal literature's widespread lack of acknowledgement of the large body of EFW-based empirical work. Explaining the occurrence of this dichotomous literature may be as simple as conceding that authors who publish only in top journals also cite only top journals. Indeed, a quick check of the citation lists in the articles discussed above suggests that citations from the likes of *Public Choice* or *European Journal of Political Economy* are rare. Nonetheless, especially in an age when the cost of a literature search is minimal, such practices should be discouraged, lest we accept the existence of two distinct classes of discourse. If club elites have compromised scholarship in the case of the empirical institutions literature, one may wonder where else the hubris may express itself. I understand that other contributions to this symposium may speak to the more general syndrome.

Table 1:
Journals Included in the Social Sciences Citation Index Publishing
Articles Citing the EFW Index

Rank	Journal	Articles Citing EFW	Rank	Journal	Articles Citing EFW
3	J Political Econ	1	130	International Rev Law Econ	1
5	Quarterly J Econ	3	135	J World Trade	1
10	J Monetary Econ	1	137	Applied Econ Letters	2
12	J Econ Perspectives	2	139	J Developing Areas	1
14	European Econ Rev	1	146	Politicka Ekonomie	1
25	J Environmental Econ Mgmt	1	148	Betriebswirtschaftliche	1
32	J Econ Behavior Org	2	149	Desarrollo Economico	1
36	Econ Inquiry	7	157	South African J Econ	1
37	World Bank Econ Rev	1	NA	Academy Mgmt J	1
39	J Development Econ	3	NA	American Bus Law J	1
41	IMF Staff Papers	2	NA	Annals American Academy	1
43	Public Choice	17	NA	Annals Regional Science	1
46	J Urban Econ	1	NA	Asian Survey	1
47	International J Industrial Org	1	NA	Australian Econ Rev	1
48	J Law Econ Org	1	NA	Canadian Public Policy	1
49	J Law Econ	2	NA	Catholic University Law Rev	1
55	World Development	3	NA	Communist Post-Communist	1
56	Southern Econ J	2	NA	Community Dentistry Oral	1
59	J Banking Fin	1	NA	Comparative Political Stud	2
60	Contemporary Econ Policy	5	NA	Crime Law Social Change	1
63	J Institutional Theoretical Econ	3	NA	Dados-Revista De Ciencias	1
64	Applied Econ	2	NA	Development Policy Rev	1
69	Oxford Rev Econ Policy	1	NA	Drustvena Istrazivanja	1
81	Kyklos	9	NA	Econ Policy	1
92	Brookings Papers Econ Activity	1	NA	Electoral Stud	1
93	Econ Development Cultural	1	NA	European J Industrial Relations	1
101	J Productivity Anal	1	NA	European J Political Research	1

Rank	Journal	Articles Citing EFW	Rank	Journal	Articles Citing EFW
NA	Forest Policy Econ	1	NA	Long Range Planning	1
NA	Habitat International	1	NA	Middle East J	1
NA	Harvard J Law Public Policy	2	NA	Org Stud	1
NA	Human Rights Quarterly	1	NA	Personality Individual	1
NA	Independent Review	6	NA	Politische Vierteljahresschrift	2
NA	Intelligence	1	NA	Post-Communist Econ	2
NA	Internationale Politik	1	NA	Professional Geographer	1
NA	International Forestry Rev	1	NA	Progress in Planning	1
NA	International Interactions	1	NA	Psychologische Rundschau	1
NA	International Org	3	NA	Publius-J Federalism	1
NA	International Political Science	1	NA	Quality Progress	1
NA	J Accounting Research	2	NA	Regional Stud	1
NA	J African Econ	1	NA	Research Policy	1
NA	J Asian African Stud	1	NA	Rev Agricultural Econ	1
NA	J Artificial Societies Social	1	NA	Rev Development Econ	1
NA	J Bus Ethics	1	NA	Rev International Political Econ	1
NA	J Bus Fin Accounting	1	NA	Social Forces	1
NA	J Bus Research	1	NA	Social Indicators Research	3
NA	J Communication	1	NA	Social Philosophy Policy	1
NA	J Consumer Affairs	1	NA	Social Science J	1
NA	J Corporate Fin	2	NA	Stud Comparative International	2
NA	J Democracy	1	NA	Telecommunications Policy	2
NA	J Econ Growth	1	NA	Terrorism Political Violence	1
NA	J Econ Surveys	2	NA	Texas Law Rev	1
NA	J Fin	2	NA	Virginia Law Rev	1
NA	J International Bus Stud	3	NA	Washington Quarterly	1
NA	J International Money Fin	1	NA	World Politics	1
NA	J Labor Research	1		<i>Total SSCI Citations</i>	<i>194</i>
NA	J Legal Stud	1		Sundry Non-SSCI Citations**	
NA	J Modern African Stud	1	NA	Cato J	3
NA	J Money Credit Banking	1	NA	Constitutional Political Econ	1
NA	J Portfolio Mgmt	1	NA	European J Political Econ	13
NA	J Rural Stud	1		<i>Total Non-SSCI Citations</i>	<i>17</i>
NA	J Science Industrial Research	1		<i>Notes: Article count applies only during years the journal has been included in the SSCI.</i>	
NA	J Sociology	1		<i>*Journal rank is from Kalaitzidakis et al (2003), Table 1. NA indicates the journal was not included in the rankings.</i>	
NA	J Southeast Asian Stud	1		<i>**Other citations in journals not included in the SSCI taken from the survey by de Haan et al (2006); represents only a partial count of citing articles.</i>	
NA	J Southern African Stud	1			
NA	J World Bus	2			
NA	Korean J Defense Analysis	1			
NA	Labour Econ	1			
NA	Latin American Politics Society	2			

APPENDIX

Areas and Components of the EFW Index. [Link](#)

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SYMPOSIUM: TRAILBLAZERS TOO LIGHTLY MENTIONED?

Peter Bauer: Blazing the Trail of Development

IAN VÁSQUEZ*⁶

ABSTRACT

PETER BAUER (1915-2002) WAS A PIONEER AND A GIANT IN THE FIELD of development economics. His contributions to the understanding of economic progress, beginning in the 1940s with studies of the rubber industry in Malaya, spanned more than five decades and dealt with a range of the most important development issues, including many that were not considered important at the time.

According to Amartya Sen (2000, ix, xi), “Peter Bauer is in a class of his own as an outstanding economist. The originality, force, and extensive bearing of his writings have been quite astonishing...[He is] one of the great architects of political economy.” In 1984, the World Bank published a volume of essays from ten leading post-war development economists (Meier and Seers 1984). It included Bauer along with such luminaries as Arthur Lewis, Paul Rosenstein-Rodan, and Gunnar Myrdal.

That level of peer recognition, however, was atypical during most of Bauer’s career. Bauer remained one of a few “voices in the wilderness”¹ largely because he stood virtually alone in challenging the development orthodoxy that held central planning, forced savings, protectionism, and official international aid as main tenets. Probably more typical of professional sentiment was Walt Rostow’s (1990, 386) description of Bauer as a “neo-classical gadfly” whose

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¹ Gustav Ranis (2004, 6).

usefulness was as a “devil’s advocate” to the complex issues being considered by other development economists.

With the success of outward-oriented East Asian economies and the collapse of development planning, Bauer’s views have generally been vindicated. From the vantage point of the 21st century, it is easy to forget that the hold of the development orthodoxy was strong long after there was ample evidence of its failings. As late as 1985, for example, Indian Prime Minister Rajiv Gandhi (1985)—who would be the



Peter Bauer
Photo courtesy of Sally Yates

first to introduce market reforms in India in the late 1980s—wrote that despite vast problems in collecting and analyzing data, “the solution perhaps lies in improving the tools of collection and analysis of data and not in abandoning the planning effort itself.” No doubt, Bauer would have been unsurprised that the political leader of the country that had epitomized the development path advocated by the post-war orthodoxy was still clinging to the allure of planning despite decades of dismal performance. Institutional inertia and vested interests can explain Gandhi’s early attitude.

Less understandable is why intellectuals, and specifically economists, took so long to arrive at more market-liberal ideas as a guide to policy in developing countries. Even less understandable is why, in the post-communist era, Bauer’s contributions are often still neglected or marginalized by the economics profession.

An example of this neglect is a survey article on trade policy and development by Anne Krueger (1997) appearing in the *American Economic Review*:

The improvement in living standards, life expectancy, and economic growth prospects in developing countries ranks among the most important success stories since the Second World War. Growth in some has been dramatic, and while progress has been far from uniform, there are grounds for optimism that future growth prospects can be even better than performance to date.

One factor accounting for that success has been improved understanding and adoption of economic policies much more conducive to satisfactory economic growth than was the case in the 1950’s and 1960’s. That better understanding, in turn, resulted from a combination and interaction of research and experience with development and development policy.

Ideas with regard to trade policy and economic development are among those that have changed radically. Then and now, it was recognized that trade policy was central to the overall design of policies for economic development. But in the early days, there was a broad consensus that trade policy for development should be based on “import substitution.” By this was meant that domestic production of import-competing goods should be started and increased to satisfy the domestic market under incentives provided through whatever level of protection against imports, or even import prohibition, was necessary to achieve it. It was thought that import substitution in manufactures would be synonymous with industrialization, which in turn was seen as the key to development.

The contrast with views today is striking. It is now widely accepted that growth prospects for developing countries are greatly enhanced through an outer-oriented trade regime and fairly uniform incentives (primarily through the exchange rate) for production across exporting and import-competing goods. Some countries have achieved high rates of growth with outer-oriented trade strategies. Policy reform efforts removing protection and shifting to an outer-oriented trade strategy are under way in a number of countries. It is generally believed that import substitutions at a minimum outlived its usefulness and that liberalization of trade and payments is crucial for both industrialization and economic development. While other policy changes are also necessary, changing trade policy is among the essential ingredients if there is to be hope for improved economic performance.

Krueger goes on to ask how this change in policy came about, and “what was the contribution of economists and their research to the process?” Krueger’s research, of course, played a key role in making the case for more open trade regimes for developing countries, along with that of other leading researchers such as Jagdish Bhagwati, Ian Little, and T.N. Srinivasan, whom she cites. Yet Krueger does not mention Bauer. In a related essay on the development experience, she surveys the contributions of numerous leading development economists but also does not mention Bauer (Krueger 1995).

Krueger is not alone in the marginalization of Bauer. Another example (many could be given), is Jean Waelbroeck’s 30-page review of the three volumes of the *Handbook of Development Economics*, a review that appeared in *World Bank Economic Review* (Waelbroeck 1998). Waelbroeck surveys the findings of the three volumes (which include Krueger’s 1995 article) and promises to identify “areas of development economics not covered there,” but

Chief Works by Peter T. Bauer

- “The Working of Rubber Regulation,” *The Economic Journal*, 1946.
- “Economic Progress and Occupational Distribution,” with Basil S. Yamey. *The Economic Journal*, 1951.
- The Rubber Industry: A Study in Competition and Monopoly*. Longmans, Green & Co., 1948.
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- United States Aid and Indian Economic Development*. American Enterprise Association, 1959.
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- Equality, the Third World and Economic Delusion*. Harvard University Press, 1981.
- “Remembrance of Studies Past: Retracing First Steps.” In *Pioneers in Development Economics*. Oxford University Press, 1984.
- Reality and Rhetoric: Studies in the Economics of Development*. Harvard University Press, 1984.
- “Creating the Third World: Foreign Aid and its Offspring.” *Journal of Economic Growth*, 1987.
- The Development Frontier: Essays in Applied Economics*. Harvard University Press, 1991.
- From Subsistence to Exchange and Other Essays*. Princeton University Press, 2000.

does not cite Bauer. Of the *Handbook's* 46 articles, only seven of them cite Bauer.

Indeed, a literature search of the *American Economic Review* beginning in 1911 when it was first published and extending through 2004 finds only seven articles with references to Bauer and three book reviews in which Bauer is cited.² Articles in the World Bank's in-house journals, the *World Bank Economic Review* (from 1986 through January 2007) and the *World Bank Research Observer* (from 1986 through 2006) cite Bauer only six times. (An Excel file detailing these search results is linked at the end of this article from Appendix 1.)

The omission is doubly striking as Bauer both addressed many of the main development issues early on and examined the possible causes of what he called the "spurious consensus" on economic development. Indeed, throughout his career, Bauer (2000, 15) repeatedly pointed to "a widespread disregard of evident reality" in his field, and would come to observe that "Impressive advances coexisted with alarming retrogression." The advances included contributions to the theory of international trade and the economics of property rights, and the recognition of transaction costs. The lapses included the neglect of fundamental economic principles, conceptual confusions, methodological pretentiousness, and the lack of direct observation. Some of what Bauer saw as troubling in the economics profession—for example, over-reliance on formal analysis and the mathematization of the field—still exists and may help explain the neglect of Bauer even among those who arrive at the same insights and general policy prescriptions as Bauer.

BAUER'S VIEW OF DEVELOPMENT

Bauer's analysis of policy and development were strongly informed by a well-defined view on the meaning of development, a perspective that he formed early in his career:

I regard the extension of the range of choice, that is, an increase in the range of effective alternatives open to people, as the principle objective and criterion of economic development; and I judge a measure principally by its probable effects on the range of alternatives open to individuals. This implies that the process by which development is promoted affects the assessment, and indeed the meaning, of the result. The acceptance of this objective means that I attach significance, meaning, and value to individual acts of choice and valuation, including the individual time preference between the present and the future; and my position is much influenced by my dislike of policies or measures which are likely to increase man's power over man, that is to increase the

² That does not include Bauer (1956).

control of groups or individuals over their fellow men. (Bauer 1957, 113-14).

From the beginning then, Bauer expressed a set of values that both guided his thought and were non-patronizing to his subject of study, and that ran counter to the views of those advocating extensive state interventionism in developing countries. Bauer's views were certainly in conflict with those of Myrdal, who believed in comprehensive central planning as a way of transforming entire societies, institutions and the attitudes and behaviors of people. "The success of planning for development," Myrdal (1968, 67) wrote, "requires a readiness to place obligations on people in *all* social strata to a much greater extent than is now done in any of the south Asian countries. It requires, in addition, rigorous enforcement of obligations, in which compulsion plays a strategic role." Candid authors like Myrdal and Robert Heilbroner (1963, 20-21, 126f) made clear the profoundly illiberal nature of many of the policies favored by the development consensus. Such views, of course, turned out to be spectacularly wrong.

But Bauer's emphasis on personal choice also put him at odds with much of the economics profession which often justifies policies on purely technical grounds—such as on an emphasis on output—with little or no regard to the preferences or the freedom of choice of the people affected by the policies proscribed. Bauer's approach clearly placed him in the classical tradition, rather than the neoclassical tradition, and as Lal (1987, 45, 46) points out, his views draw from an older rhetorical tradition as well, making many economists—such as Srinivasan—uncomfortable with Bauer even though they may reach much the same policy conclusions. According to Lal, the rhetoric of such mainstream economists to justify the market comes from second-best welfare economics "couched in the Arrow-Debreu language."

A further characteristic that distinguished Bauer's approach was his recognition of the limitations of both statistical evidence and the use of mathematics and the quantifiable in the study of development. What to much of the profession was and is a sign of scientific rigor to Bauer was a misplaced focus on seemingly measurable factors, such as capital, and to the neglect of influences, such as the historical context and background conditions, far more important to development. "It has encouraged confusion between the significant, on the one hand, and the quantifiable (often only spuriously quantifiable), on the other" (Bauer 2000, 19).

What matters most is direct observation and reliance on primary sources. That belief made Bauer exceptionally interdisciplinary, relying on the work of historians, business accountants, anthropologists, and even travel writers. Thus his criticisms and his approach may have alienated him from much of the economics profession even after the tepid pro-market consensus was formed. Indeed, late in life Bauer (2000, 20) would still lament: "What has become of the traditional method of direct observation, reflection, tracing of connections,

reaching tentative conclusions, and referring these back to observation and to established propositions of the discipline, or to findings of cognate disciplines? Such procedures are no less informative than quantitative analysis. For instance, with the traditional approaches the economist was much more likely to be aware of the gap between theoretical concepts and the available information.”³

Bauer’s critiques of growth models reflect his distrust of over-reliance on formal analysis and are largely valid to this day. Growth models may have encouraged the emphasis on the aggregative and quantitative approach in development economics, and also conferred an air of rigor to such analysis. But Bauer (1984, 34) warned that conventional growth models were “unhelpful and even misleading” because they ignored the fact that the parameters were affected by the chosen variables, which themselves he came to recognize as “unimportant.”⁴ People’s attitudes or the political situation, variables omitted by growth models, are far more important to progress than the stock of capital, and attempts to increase that stock by tariffs, for example, will affect a model’s parameters and have an impact on development far greater than any increase in capital stock.

Growth models thus “become travesties” (Bauer 1972, 285) that are used to justify wrongheaded policies and neglect direct observation. “As a result of this neglect, development economists often analyse societies, systems and situations which they do not know: they literally do not know what they are talking about” (Bauer 1972, 289).

Such insights and rhetoric did not endear Bauer to most of his colleagues and the agencies that give grants to development economists.⁵ But Bauer’s insights do help to answer one of Kreuger’s (1997, 2) questions in her review article on the evolution of thinking on trade and development: “How could it happen that a profession, for which the principle of comparative advantage was one of its key tenets, embraced such protectionist policies?” Development economists were neglecting important principles and facts because they were not looking at the way people in developing countries actually lived. In his writings, Bauer not only took issue with the main findings of the “spurious consensus,” but often showed why there was a gap between the development orthodoxy and reality.

³ In a review of papers recently published in the *Journal of Development Economics*, Susan Anderson and Peter Boettke (2004, 307) observe that “formalistic tendencies still dominate,” and they criticize the minimal attention paid to institutional history.

⁴ Bauer (1972, 284) further warns that “While the choice of variables on the basis of logical convenience, simplicity, or elegance of analysis, is often fruitful in the natural sciences, this is not usually so in social studies, where recognition of the complexity of the problem is indispensable for valid results.”

⁵ On the proportion of development economists receiving support from the development agencies, see Klein and DiCola (2004).

This is not the place to review Bauer's myriad contributions to the development debate.⁶ Rather, by examining the trade and development issues Krueger (1997) highlights, we can get a better understanding of Bauer's thinking and why he fell outside the mainstream of his profession. The broad issues she highlights include: the behavior of peasants said to be traditional because they supposedly did not respond to price incentives; the dependence of developing countries on primary commodity production, something which free trade would further exacerbate; and the idea that capital accumulation and industrialization were critical for growth.

BAUER ON GROWTH AND EXCHANGE

Bauer's first contributions to development economics included his publications on Malaya (1948) and West Africa (1954).⁷ In each place he spent considerable time and was meticulous in documenting the central role of local populations in the rapid spread of the cultivation of cash crops. He was able to show how Asians and Africans, generally lacking formal education, had transformed the economies of those regions within a few decades. While there were no rubber trees in Malaya or cocoa trees in British West Africa in 1885, Bauer noted that millions of acres of cash crops had been planted there by the 1930s, mostly owned by non-Europeans.

His research and observations established a pattern of scholarship that both challenged received wisdom and set its own high standards of method. Peasants, it turned out, did indeed take the long view in planting crops that take years to mature, responded to price signals, and otherwise responded to market incentives. Their supply curves did not bend backwards. Theodore Schultz's (1964) study of traditional agriculture, cited by Krueger (1997), later was important in undermining the idea that peasants are nonresponsive, but Bauer was perhaps the first to show the folly of that idea.

In studying Malaya and West Africa, Bauer (1954, 3) found it "necessary to restrict abstraction rather severely, and to investigate factors and influences which are often regarded in modern studies in economics as institutional elements (or as data given to the economist). This survey therefore includes a review of some factors which are normally omitted from most modern textbooks on economics, and even from some of those professedly dealing with applied economics." As such, Bauer was able to document aspects of the move from subsistence production to wider exchange that were unknown or ignored by orthodox development economists. Elkan (1982, 247) claims that Bauer's early work "foreshadowed the discovery of the 'informal sector,'" while Yamey (1987, 22) states, "I believe [Bauer] was the first economist to recognize the

⁶ For a good general review of Bauer's thinking, see Dorn (2002), *Cato Journal* (2005), and Blundell (2002).

⁷ See Bauer (1948) and Bauer (1954).

extent and economic significance of what has come to be known as the informal sector.”

One phenomenon that was typically ignored in the development literature was the role of traders. Traders, Bauer observed, open up possibilities for farmers otherwise engaged in subsistence production to invest in production for trade. A large part of capital formation takes the form of non-monetary investment—for example, the clearing and improvement of land which requires personal effort—that is not captured by official statistics. Yet Bauer observed that in the aggregate, such activity from small farmers was significant, and its neglect by academics and policymakers led not only to misperceptions about economic activity, but also to flawed policies including taxation of farmers who were thus discouraged from engaging in capital formation.

Bauer thus early on had a healthy skepticism of official statistics and refuted the popular notion that large amounts of capital were necessary for growth. To Bauer (1987, 6), “Lack of money is not the cause of poverty, it is poverty,” and to have money is the “result of economic achievement, not its precondition.” He explained (1981, 248) that what is required are “changes in attitudes and mores adverse to material improvement, readiness to produce for the market instead of for subsistence, and the pursuit of appropriate government policies. Much of capital formation is not a pre-condition of material advance but its concomitant. Housing is one example . . . infrastructure (roads, railways and the like) is also a collection of assets and facilities which do not precede or determine development, but are largely developed in the course of it.”

In this sense, Bauer saw no reason why the role of capital would be any different in the Third World than it was in the West, where other factors, such as institutions that support an exchange economy, were the keys to economic progress. The notion of a vicious cycle of poverty was contradicted not only by the experience of the West, whose initial condition was poverty, but of what Bauer observed in the Third World. The prevalence of the “vicious cycle” idea further confirmed the neglect of evident reality so widespread in his branch of economics. His views on capital also led him to reject foreign aid as essential for growth and to criticize forced savings schemes, which were a central part of import-substitution.

The role of traders in bringing about development was underappreciated in other important ways. Traders regularly provided credit to small farmers and served as intermediaries with manufacturers and the outside world. But the lines separating farmers, traders and manufacturers were often not easily drawn, a fact usually ignored by policymakers and development economists. Farmers were often also traders, and successful traders often became leading manufacturers. Moreover, consumer goods brought in from abroad were not detrimental to savings and investment; rather they acted as incentive goods leading to greater productivity and investment. The development of

agriculture, because of trade, was complementary to the development of industry (just as consumption and investment were complementary). Neglect of these facts also led to inappropriate policies.

Still, Bauer was quick to point out that development did not depend on the development of manufacturing, which in turn did not depend on coercion or central planning. To suggest otherwise was to be ignorant of economic history and to confuse correlation with causation: “this argument for industrialisation, as somebody once said, is analogous to the suggestion that smoking expensive cigars will make people rich as it is rich people who smoke expensive cigars.” (Bauer 1972, 143).

Bauer went on to suggest why poor countries should not shun agriculture:

There are various reasons why in many poor countries a large measure of continued reliance on agriculture, notably on agricultural production for sale, is likely to represent the most effective deployment of resources for the promotion of higher living standards. One reason is the familiar argument based on comparative costs. Another, less familiar, reason is that production of cash crops is less of a break with traditional methods of production than subsidised or enforced industrialisation. Agriculture has been the principle occupation in most of these countries for centuries or even millennia. Thus in the production of cash crops the difficulties of the adjustment of attitudes and institutions in the course of the transition from subsistence production to an exchange or money economy are not compounded by the need to have to acquire at the same time knowledge of entirely new methods and techniques of production. After some time spent on the cultivation of cash crops, people find it easier to get used to the ways, attitudes and institutions appropriate to a money economy. This greater familiarity with the money economy facilitates effective industrialisation. In these conditions of transition from a subsistence to a money economy, conditions widely prevalent in poor countries, production of cash crops and effective industrialisation are complementary through time. The unfavourable contrast often drawn between agriculture and manufacturing, to the detriment of the former, is an example of a time-less, unhistorical approach to economic development, an approach which is inappropriate to the historical development of societies. (Bauer 1972, 144-45).

We now know, of course, that import substitution industrialization led to a tremendous bias against agriculture, as well as other economic distortions inimical to growth. East Asian countries that abandoned that model confirmed Bauer’s insight, as did, sadly, countries that did not. Indeed, Bauer (1957, 79)

warned against the “restrictive measures” being applied in much of Africa and the developing world: “these economies have not experienced the comparatively long spell of relatively unrestricted economic activity undergone by developed countries in the past; this early emergence of effective economic restrictionism may appreciably retard their rate of economic progress.”

In other areas related to development thinking on trade, Bauer’s critiques were equally prescient and devastating. Examples include his critique of the United Nations Conference on Trade and Development (Bauer 1972, first published in 1967) and his discussions of agricultural marketing boards, the supposed deterioration of the terms of trade, commodity agreements, and balance of payments crises. Throughout, Bauer (1972, 457) did not tire of pointing out that “Now, as in the past, the most advanced of the underdeveloped regions and sectors are those in contact with developed countries.” Among the leading development economists, his exposition of the effects of trade on poor countries was by far the most conceptually sound.

BAUER’S INFLUENCE

Any attempt to explain Bauer’s marginalization within his profession is necessarily conjectural. What would explain, for example, Little’s (1961) criticism of Bauer as a “political adolescent” followed years later by an apparent conversion of views—as expressed in his book *Economic Development* (Little 1982)—consistent with Bauer’s own market-liberal views but in which Little refers to Bauer only in one footnote (which itself does not reference Bauer’s thinking)?⁸

In her survey article on trade policy, Krueger (1997, 7) refers to the 1950s and 1960s and observes that “For more than a decade, the growing disparity between theory and practice was all but ignored.” She adds that, “One of the puzzling aspects of the evolution of thinking about policy is the degree to which proponents of open trade regimes failed to refute the allegation that free trade would forever leave developing countries specialized in production of agricultural commodities” (11). Evidently, Krueger was either ignorant of or unimpressed with the refutations offered by Bauer.

It may be, as Lal (1987, 46) points out, that the discomfort of mainstream economists with Bauer is due to “an epistemologically unsound positivist view of economics as a science.”⁹ Bauer, by contrast, warned against approaching the study of economics as though it were akin to a physical science. Data is important, but so are relationships between phenomenon that can only be discovered through direct observation including factors that are not easy to quantify such as attitudes or the time dimension.

⁸ See Lal (1987) for this account.

⁹ Lal cites McCloskey (1983) as making this more general point about the economics profession.

Following a different methodological tradition, much of the research on trade that helped overturn the development orthodoxy was empirical. Krueger rightly notes that much of it also depended on measurement tools, such as cost-benefit analysis, that could be applied across countries. As such, the trade research provided powerful evidence on the costliness and arbitrary nature of protectionism. Krueger is probably right when she notes that this research and the development experience itself played the key roles in undermining the prevailing consensus. But it seems that pro-trade economists would have been more effective had they been less dismissive of Bauer.

Indeed, Bauer's classical liberal sensibilities allowed him to see things that were arrived at years later by others. Examples of this include his emphasis on institutions, customs, and government policies as the key determinants of development, and his dim view of the politicization of life that comes with increased state interventionism, an insight that would later be developed by research on bureaucracies and rent-seeking.

In Southeast Asia and West Africa, he was able to see economic progress that "was not the result of conscious efforts at nation building (as if people were lifeless bricks, to be moved about by some master builder)... What happened was in large measure the result of the individual voluntary responses of millions of people to emerging or expanding opportunities created largely by external contacts and brought to their notice in a variety of ways, primarily through the market. These developments were made possible by firm but limited government, without large expenditures of public funds and without the receipt of large external subventions" (Bauer 1984, 31). The fact that advanced sectors of the economy co-existed with traditional sectors was evidence to Bauer of the spread of economic progress, especially when put into a reasonable time frame and compared to the similar historical experience of the West; it was not evidence of enclaves or the lack of backward or forward linkages.¹⁰

Bauer's particular approach to the study of development, though uncongenial to some, afforded him those and other insights. Yet another explanation as to why those insights were underappreciated was the fact that to younger generations of economists, they were simply unknown. Lal (1987, 43) reports that, given negative reviews, Bauer's work was long "written off" by Lal's contemporaries. William Easterly notes that "It is amazing how much of the research and thinking of my like-minded co-authors and me was anticipated decades ago by Bauer, without us realizing it. A not so obvious example of this is Bauer's skepticism about investment and capital accumulation as a very important force in economic development, which people like Ross Levine, Lant Pritchett, and I have shared in several papers in the last decade" (Easterly 2005).

¹⁰ For a good review of the rise of Europe that is informed by Bauer's insistence on examining centuries of historical background, see Raico (1994).

In the end, Bauer's influence may be greater than is generally appreciated. As development economics has matured and gained a more sophisticated appreciation of the complexity of the growth process, prominent scholars have favorably cited Bauer in recent years. And students of development economics seeking insight and inspiration will continue to read Bauer. Yet, now that the general consensus favors market-oriented policies, I believe that Bauer would have been skeptical of recent initiatives undertaken under that banner. One example of such initiatives is the currently fashionable effort by some aid agencies to promote "sound" policies and institutions. In critical ways, Bauer is still ahead of the debate.

Bauer (2000) once described Indian economist B.R. Shenoy as a hero and a saint. Shenoy dissented from policy opinions that prevailed in his country in the 1950s.¹¹ To Bauer, Shenoy was a hero because he publicly resisted development fads, and he was a saint because he remained serene "in the face of neglect, disparagement, even abuse." Bauer claimed that Shenoy had personally influenced Bauer's own conduct and opinions. "Shenoy united moral courage, intellectual integrity, and technical competence to an exceptional degree. The few people who possess this combination of attributes are of great value, both in public life and in academic study. They are particularly valuable in the study of society, where they are especially rare." Bauer concluded: "May the succession of Shenoy and his like never fail, East or West." Quite so.



Sculpture of Peter Bauer
By Lyn Constable Maxwell
[\(Link\)](#)

APPENDIX

Excel file explaining search and listing citations to Peter T. Bauer in the *American Economic Review* (1911-2004), *World Bank Economic Review* (1986-2007), and *World Bank Research Observer* (1986-2006). [Link](#)

¹¹ For compilations of Shenoy's writings, see Shenoy (2004a) and Shenoy (2004b). For essays on Shenoy and other Indian market-liberal scholars in the post-World War II period, see Shah (2001).

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CHARACTER ISSUES

The Role of Economists in Liberalizing Swedish Agriculture

HENRIK LINDBERG*

ABSTRACT

INSIDE THE SWEDISH PARLIAMENT SAT MEMBERS FROM THE FIVE major parties in Sweden. Outside, more than 20,000 angry farmers marched around Parliament and the Royal Castle carrying placards and shouting slogans. The “revolt” took place the 23rd of May, 1985 while the politicians were discussing overproduction adjustments.

The scene is unusual for Sweden, and makes vivid how acute the issues concerning agricultural policy had become during the 1980s. Food surpluses in Sweden, Europe, and the United States had no obvious market as the world market was filled with subsidized milk, meat, grain and other products. How to get rid of it? The whole sector in Sweden was girdled in controls on land management, cultivation, quantities, and prices (Flygare and Isacson 2003).

The system was changed fundamentally in the early 1990s. Deregulation was decided in 1990 and implemented thereafter. Although agricultural policy is looked upon as especially hard to change because of the vested interests, the reform was a change of system. The 1990 deregulation was approved in a political consensus—the Swedish way.

The change brought Sweden from one of the most regulated and subsidized countries to one of the least regulated. It is part of the story of the resilience of the Swedish system (Bergh 2006, 458).

A starting point to understand the change of system in agricultural policy is that ideas matter. They influence politics.¹ However, they do not exist in a vacuum, but are promoted by idea carriers, such as economists from time to time. To have effect, ideas must have actors imbued with purpose and motivation.²

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¹ See for example Hall 1993, Sabatier 1991, Blyth 1999, and Hayek 1949.

² On the role of economists in ending conscription in the United States, see Henderson (2005).

It is hard enough for a citizen to keep track of the government agencies, political organizations, and industry and trade associations of her own country, especially in history, but it is even harder for foreign institutions. Here I generally avoid names of specific Swedish institutions (which would be translations, anyhow) and instead use generics like “producer groups” and so on.

The present research is based substantially upon interviews I conducted with many of the major players (listed after the References list). When I write of one of them “looking back,” etc., I mean as spoken to me in interview.

EARLY CRISIS AND INTERVENTION

During the 1920s grain prices fell dramatically in Sweden and later stabilized. The 1930s saw a general slump, and growing protectionism further hurt Swedish exports. As in the rest of Europe and the United States, the 1930s mark the origins of the modern agricultural policy in Sweden (Morell 2002, 179-181; Magnusson 2000). With support from the Agrarians and Social Democrats, regulations were imposed on production and food processing (Schon 2000).

A major intervention often generates unintended difficulties, scams, and exploitations. Rather than admitting error and removing the first monkey-wrench, government typically responds to aggrieved parties seeking protections and privileges of their own. Intervention begets intervention, resulting in a cluster of related interventions and regimented industries and markets (Mises 1929). The syndrome is sometimes called the *intervention dynamic* (Ikeda 1997). A classic example is agriculture.

Prices were fixed and agricultural exports subsidized. The system especially served large farms and landlords. “Poor man’s butter,” margarine, for example, was taxed to protect butter. Every time they paid their taxes or grocery bills, poor people opened their wallets for the wealthy (Morell 2001).

Farmer organizations got control of output. They were handed the exclusive authority to impose fees and administer various parts of policy. The fundamental idea was to replace market mechanisms with a more or less planned economy where prices and output were decided by authorities.

When the crisis began, the producer organizations were weak and without much influence. The new system gave the farmer organizations resources, power, and legitimacy (Rothstein 1992). The original impetus for major intervention was as much a matter of general collectivist philosophy among the political classes as it was groups scheming for privileges and handouts. The new agricultural policy was introduced in the 1930s at the same time that Keynesianism gained a breakthrough, and, incidentally, the agricultural policy was connected to the forming of an interventionist labor-market policy (Schön 2000, 331-333; Lewin 1967, 89-91).

1947, THE MAGNA CHARTA OF SWEDISH AGRICULTURAL POLICY

During the Second World War, the authorities controlled prices and rationed certain foods (Morell 2001, 181-184). An “iron triangle” was forming between producer organizations, politicians with farming interests, and a growing agricultural bureaucracy. Milton and Rose Friedman (1984) discuss how the status quo “iron triangle” of beneficiaries, politicians, and bureaucrats typically acts to frustrate efforts at policy correction.

By the end of war some new regulations had been imposed, but since the new measures had their origin in a very specific situation, policymakers soon found it necessary to take a broader grip of the policy field. The committee in which these questions were to be answered was, from its start in 1942, heavily dominated by farmer interests. 1947 saw the formulation of the agricultural “Magna Charta,” as it is often called in Swedish discourse. The decision was to make the wartime system of controls permanent.

Three goals were authorized in official documents: (1) a production goal that basically meant autarchy in foods; (2) an income goal to protect farmer incomes and to achieve parity with other groups such as industrial workers; (3) an efficiency goal that was to be achieved by a structural transformation in agriculture (Hedlund and Lundahl 1985, 105-106).

The central measures were tariffs and price controls. The tariffs were meant to protect the Swedish farmers from outside competition and the price controls would keep their incomes high and stable. Since farmer organizations were formed as co-operatives and gradually included more and more producers, the risk of competition was gradually reduced. Other regulations restricted who could buy land or enter farming. In classic corporatist fashion, government power enabled agriculture to become increasing cartelized.

During the 1950s and 1960s, structural transformation in agriculture was rapid. Mainly economists issued severe criticism of the system. The system’s goals were vague and partly contradictory. The instruments came to be seen as ill adapted to achieve the official goals. The price negotiations became increasingly complicated and expert-intensive. Even though food prices rose, farmers felt that their incomes lagged behind, as others enjoyed accelerated living standards. In the 1950s and 1960s, discontent continued to spread (Flygare and Isacson 2003, 230-235).

EARLY ECONOMISTS: AGAINST SPECIAL INTERESTS, BUT NOT NECESSARILY CONTROL

Starting in the 1930s, Swedish economists criticized agricultural policy. Two of the most famous were Gunnar Myrdal and Johan Åkerman. Ideologically apart, they both criticized costly overproduction (Hedlund and Lundahl 1985, 74-75, 82). As members of the Agricultural Committee in 1940s, Ingvar Svennilsson and Erik Lundberg aired reservations and said production goals should be reduced (SOU

1946:46, 516-526). During the 1950s and 1960s, Claes-Erik Odhner, a trade-union economist, argued that the agricultural sector was too large and that structural rationalization was possible (Odhner 1966). A reallocation of labor from agriculture to industry was seen as a major precondition for further increases in living standards.

Coalitions fight for influence by framing issues (Barber 1993). In the labor market two interests, employers and unions, were organized and expressed. In the agricultural market, only one interest was organized and powerful—the Federation of Swedish Farmers. No consumer interest was organized. Therefore attempts were made to organize an opponent to make price negotiations more balanced. In 1953, the Consumer’s Committee for Agricultural Issues was created.³ Odhner was appointed chairman and became a driving force in the public debate. In 1963, the Consumer’s Delegation was founded to take part in price negotiations, but its influence was nevertheless obstructed by their duty to negotiate *reasonable* prices, not lower prices. Another problem was that their representatives lacked expertise in the regulations and processes (Ljusberg 2005). Producer interests controlled the basic data and local knowledge of agricultural operations. Jan-Erik Nyberg, a former head of the Consumer’s Delegation, had the opinion that their representatives were prisoners of seasoned participants and the process in general. The “consumer” representation gave the system more legitimacy, but without doing much to counter the iron triangle (Micheletti 1990, 94-97; Ljusberg 2005).

In the 1960s, several economists, including Odhner but especially Assar Lindbeck and Odd Guldbrandsen, argued for another solution that would give priority to industry rationalization rather than income protection. With the help of so-called price pressure—where the guaranteed prices were gradually lowered—more farmers could be induced to exit the sector, which would also reduce their political weight. Also, Lindbeck and Guldbrandsen pointed out the effects on the national economy. With some policy adjustments, the total costs could be significantly reduced (Lindbeck and Guldbrandsen 1966, 105-107, 161-167; Odhner 1966, 170-172).



Assar Lindbeck

Agricultural policy was heavily debated and many actors, especially economists, participated. The issue flared up during the 1966 election campaign. It appears that some actors attempted to form a coalition of the labor unions and business interests to support the arguments from the economists. The emergent proposal was not radical, but it was more than just marginal adjustments. The controversial parts were stepping up pressure on a group that felt entitled to existing privileges, and openly seeking to induce farmers to give up their farms and leave the business (Lindberg 1995, 22-25). The analysis began quietly to paint the farmer interests as deserving reproach.

³ It was created by three organizations, the Confederation of Swedish Trade Unions, the Central Confederation of Professional Employees, and the Cooperative Union and Wholesale Society.

But the farmer groups initiated a counteroffensive 1966-1967 in newspapers and media to defend the regulations and high prices. They kept the focus of debate on how to utilize the existing policy instruments (Lindberg 1995).

In the 1960s, the prevalent vision was a modern industrial society with production by large-scale units and top-down coordination (Magnusson 2000). The vision was widely held with great optimism. The vision sometimes warred with traditional special-interest groups, and indeed joined forces with other opponents of old privilege. Economists were generally the voice of “efficiency,” but among the Swedish government economists of the day, that did not necessarily mean they were particularly liberal.

In 1967, the government went in the direction of rationalization, as against income maintenance, indicating that the growth and efficiency arguments had some traction. But the major instruments, the tariffs and price regulations, remained (Guldbrandsen and Lindbeck 1968, 115-117). The changes implemented in the 1960s and 1970s had fewer consequences than some had feared and others had hoped.

GROWING DISSATISFACTION

The more efficiency-and-growth oriented agricultural policy did well during the 1960s, but there were many farmers who did not accept the developments. The dissatisfaction was directed against the huge structural transformation that had taken place. More than 75 percent of farmers had left the business during the postwar period (Schön 2000, 427). In the 1970s, opinions bristled and people contested the virtues of growth, large-scale production, and the ongoing structural rationalization. It wasn't just an industry, but regional ways of life and social powerbases that were contracting.

Furthermore the world market was changing. Grain prices rose sharply and great famines plagued the Third World and appeared on television. Why should Swedish farmers limit their production when starvation abounded? Why not allow more planting and export the surplus abroad or give it away as aid? (Swedborg 1980, 49-51; Flygare and Isacson 2003, 237-238).

The Agricultural Policy decision of 1977 gave these opinions support and increased the regulations, tariffs, and food prices. The Conservative government and above all the Agrarian party were especially keen on giving family farmers more benefits. In classic intervention-dynamic fashion, to compensate consumers, the government introduced a special form of subsidy on “basic food.” Internationally, the development was not unusual, as most industrialized nations reacted that way to the new circumstances and above all, they let the producer lobbies rule the game. The producer organization in Sweden had been strengthened since 1970 when a merger between the economic organization (SAL) and the political organization (RLF) gave them resources and unity to take the offensive. The agricultural iron

triangle also dominated the Commission on Inquiry on Agriculture and had no reason to moderate its demands (Öberg 1994, 186-195).

THE FRANKENSTEIN REGULATORY SYSTEM

In the first half of the 1980s food prices went downward again and new marginal adjustments of the policy followed, but only after massive protests from the producer groups. The situation in the mid 1980s thus had its roots in the “horse-trade” between the Agrarians and the Social Democrats in the 1930s. An unforeseen consequence of the policy, large surpluses, was in a way beneficial during the Second World War and thereupon the policy fundamentals were formed in 1947. Producer groups that were weak and divided initially had been gradually strengthened as producer’s cooperatives more and more dominated the foods industry. Step by step the market was controlled, natural forces disturbed, and further regulations introduced to “correct” the outcomes. Concurrently, the bureaucracy expanded. Even according to official documents, the agricultural system had reached a deadlock (SOU 1984, Prop 1983/84, 7-8).

It may be said that in 1980, the conventional view of the developments 1930 to 1980 was that they were a necessary way to protect domestic prices from the convulsions of world markets. The farmer organization was conventionally described as an organization with the character of a union, imbued with the values of cooperation and solidarity. Considering the circumstances special to agriculture, the regulatory system was just being socially responsible. Some problems arose, but on the whole the system was seen as stable and passable. The government food policy report contained several criticisms (SOU 1984), but in the end the reform proposals did not affect the founding goals and instruments. In some respects the controls were even strengthened.

APPARATCHIK ECONOMICS

As noted, from the start a few vital economists had dissatisfactions that continued to smolder. However, an obvious tendency of *agricultural* economists in Sweden had been their docility towards the status quo. According to Bolin (2005) and Brorsson (2005) that was part of the culture at the Swedish University of Agricultural Sciences—much like the agricultural economics programs in the United States (Pasour 2004). Almost all of the specialists in agricultural economics were—or wished to be—in farmer organizations, the bureaucracy, or government advising. That is, they had clear incentives not to challenge the iron triangle.

No expert scholars seriously challenged the status quo. The system was so complex and hard to grasp that only a few experts could understand how the system really worked. Then, from professional esteem, pride, and material prudence, they became apparatchiks (Hayek 1960, 291).

Bolin, Meyerson and Ståhl (1984, 76-80) noted, after interviewing the central policymakers in the inner circle, that the power over agricultural policy was integrated and did not rest with identifiable individuals. It was rather a group of tightly connected policymakers with a similar view on both problems and solutions, favoring the existing policy. According to my interviews with the former minister of Finance Kjell-Olof Feldt and his co-worker Michael Sohlman, politicians in this policy field often had connections with farmer groups (and often were farmers themselves). As is typical of parties with a hold on the coercion monopoly, they tended to ignore outsiders unless put under pressure to address them. Outsiders had to adapt to the establishment or struggle on their own. Only a few, like the economists of the 1960s, questioned pieces of the system.

THE CHALLENGE

Events of the 1980s show the force of ideas and the potential of economists to realize a certain calling. The *raison d'être* of the policy was boldly challenged.

If one should identify a single moment, the choice is rather simple. In 1984 the Centre for Business and Policy Studies published *The Political Economy of the Food Sector: The Case of Sweden*.⁴ The main authors Olof Bolin, Per-Martin Meyerson, and Ingemar Ståhl, were economists but many of the contributors to the book also had expertise in agriculture. They took an economic approach that was bold in its independence and readiness to reject or dismiss conventional rationales for the system. Their basic question was: what economic effects did the existing policy have?

Three features of the book had explosive force. At the time, public choice theory was weak in Sweden, though a notable exception existed in one of the three authors,



Olof Bolin

Lund university economist Ingemar Ståhl.

Nobody had applied public choice to agricultural policy. In the book, the authors choose to explain the existing system in terms of the “iron triangle,”⁵ i.e., politicians, bureaucrats, and farmer organizations. The authors concluded that they had gained power and used it at the expense of consumers and taxpayers (Bolin, Meyerson and Ståhl 1984, 68-69; Bolin 2005; Meyerson 2005).

Another important novelty was the express framework of doing the “social accounting” with a full set of books,



Ingemar Ståhl

⁴ As revealed in personal interviews, the same opinion is shared by the authors of the book as well as Mats Lundahl, Mats Hellström, Bengt Rydén and Gunnar Wetterberg.

⁵ The concept “Iron Triangle” was borrowed from the Norwegian Political scientist Gudmund Hernes (1975), which influenced the Norwegian “Power Study” research project and the SNS (Rydén 2005).

including the extensive coordination of farming, processing, distribution, and consumption. The claim to comprehensiveness helped to make the conclusions authoritative. It would be incumbent upon a critic to demonstrate error or to offer an alternative account that was comprehensive. (Of course, the comprehensiveness was to the nation, not humanity.)



Per-Martin Meyerson

The sections on processing were novel. The agricultural cooperatives dominated as purchaser, and they also had the lions' share of the food processing industry, stifling competition not only from abroad but also from domestic companies not connected to farmer-run businesses. With this dominance there was always a threat of monoposonistic/monopolistic behavior. Naturally, they enjoyed privileges against entry (Bolin, Meyerson and Stahl 1984, 119-121).

The third novelty was the cost estimates. Who gained and who lost? How did the gains compare to the losses? The effects of the existing policy were analyzed in static and dynamic perspectives. They showed that, irrespective, the national economy would gain considerably if the regulations were abolished. Yes, *abolished*. The gains would be large enough to compensate all the farmers who otherwise would lose from the change (Bolin, Meyerson and Stahl 1984, 105-106).

A year after *The Political Economy of the Food Sector* there appeared another book *War Preparedness or Protectionism?* The authors were two Lund University economists, Mats Lundahl and Stefan Hedlund, and there seems to have been some contact between them and the other group, although not much. This book didn't attract as much attention, but it was part of a more serious critique of agricultural policy. The "war preparedness"



Mats Lundahl



Stefan Hedlund

arguments for the need for self-sufficiency were thoroughly examined and rejected. Supposed policy goals did not match policy outcomes. The real outcome was protectionism, administered by powerful producer interests and the agricultural bureaucracy (Bolin 2005; Lundahl 2005).

When a diagnosis of error is issued and comes to be seen as authoritative, it makes one who willfully obstructs correction a villain. With the serious economic analysis came a new view of the people who constituted the iron triangle.

PUBLIC DEBATE

The Political Economy of the Food Sector instantly sparked an enormous debate. The arguments for decontrol had never been so clear and the proposal so radical. The very day it was released, the farmer organization (LRF) and its chairman Sven Tågmark arranged a press conference where they attacked the book and its authors as “mistaken, cynical and not reliable” (Bolin, Meyerson and Stahl 1985, 34). The book was met by cutting rejoinders in the press, at meetings, or in debates. It is clear that the farmer group felt threatened by the whole challenge—the analysis, the proposals, and the less than flattering portrayal of organized interests (Bolin, Meyerson and Stahl 1985, 33-39).

Persecution of political challengers and cultural dissidents is of course commonplace throughout human history, and helps to explain why enlightened ideas don't prevail as generally as they should. One of the authors, Meyerson, recalls that inside the Federation of Swedish Industrialists, where he was employed, representatives from the foods industry tried to get him sacked. More than a decade earlier something similar happened to Assar Lindbeck when he wrote about sugar regulation. Challenging the system was obviously a risky business (Meyerson 1997, 167-175; Bolin 2005, Meyerson 2005; Rydén 2005).

When the press debate was studied afterwards it turned out to be dominated by, on one side, the major farm group (LRF) and likeminded interest organizations, and, on the other side, economists. Representatives of the consumer groups didn't take part. Very little came from established agricultural economists and others in the agricultural sector. Almost nobody in the bureaucracy said peep. The politicians, too, mostly fell dumb. Two MPs from the Agricultural Committee in parliament, both farmers, from the Conservative and the Center party, argued against the book in the debate, but they were the only ones. The debate was primarily between economists and the producer interests (Bolin, Meyerson and Stahl 1985, 25-26). When things got hot, few stood alongside the farmer groups, leaving them to appear as special-interest pleaders.

It might be incautious to draw conclusions from the sample of articles in the database “Artikelsök”, but it indicates that there was an increase of debate following *The Political Economy of the Food Sector*. Of ten articles published in *Ekonomisk Debatt* 1979 through 1990, nine came after the book was published in 1984. The book was cited many times—it was very unusual for a book to have such attention. As for statements in the press from the minister of agriculture, Svante Lundqvist, and his state secretary had only five from 1982 to 1986, whereas Mats Hellström and his state secretary Michael Sohlman had 27 from 1986 to 1990.

In anticipation of the 1985 election, the farmer group (LRF) and its supporters may have planned to take on the Social Democrat government. When *The Political Economy of the Food Sector* was published the organization was forced back into a defensive position. The alternative to the existing policy instead turned out to be a more or less complete deregulation and the producers' power to formulate the problem was broken.

THE FINANCE MINISTRY GETS INVOLVED

We can hardly expect the ruling policy coalition inside the iron triangle to initiate reforms. But outside this coalition, at the Ministry of Finance which got the central role in the government again after the Social Democrats regained power 1982, ideas were developed about a more thorough scrutiny.⁶ The Ministry believed that the agricultural regulations caused overproduction and higher food-prices. The Finance Minister Kjell-Olof Feldt (1982-1990) encouraged his co-workers at the Ministry to be open-minded and to suggest reforms that would cut expenses a bit and deregulate some markets to ease inflation (Feldt 1991, 343-348; Feldt 2005; Sohlman 2005; Wetterberg 2005).

When instructions were given to the food policy committee in 1983, Finance Minister Feldt tried to convince the Ministry of Agriculture to bring forward the issue of deregulation. The proposals ultimately led to a clash between Feldt and Agriculture Minister Svante Lundqvist. According to Feldt, Lundqvist didn't want to irritate the farmer group and was reluctant to appear affirmative on deregulation (Feldt 1991, 113-114; Feldt 2005). In consequence, the attempted reform, which was to stay within the existing system but make substantial adjustments, failed. Looking back, the former Minister of Agriculture Mats Hellström felt that the producers' interest could rule the regulations and affect outcomes in negotiations. Therefore it became clear that minor attempts to reform the system did not work (Hellström 1999; 2005). Despite the failure in 1982-83, the Ministry of Finance continued work on the issue (Sohlman 2005; Feldt 2005). They focused on bringing out new facts about the consequences of the regulations, mostly concerning outlays and inflation.

AN ALLIANCE BETWEEN THE ECONOMISTS AND THE FINANCE MINISTRY

When the agricultural policy debate took shape in 1984 there was soon a sort of concurrence between the academics—mostly economists—and the Ministry of Finance. Inside the Ministry the important papers and books were studied, especially *The Political Economy of the Food Sector* and *War Preparedness or Protectionism?* (Sohlman 2005; Feldt 2005).⁷ The new thinking gained its first foothold in officialdom in the Ministry of Finance (Sohlman 2005; Feldt 2005; Lönnqvist 2005).

The alliance between the economists and Finance, according to Michael Sohlman, led to the appointment of the economist Gunnar Wetterberg to investigate grain regulation, a centerpiece policy as it also affected meat prices. If the grain regulation were dismantled the whole system would collapse. Wetterberg, a pragmatic

⁶ An important role was played by the expert Inga-Britt Ahlenius, an advisor in the inquiry of 1983 foods committee. She is mentioned by former Minister of Finance Kjell-Olof Feldt, former minister of Agriculture Mats Hellström, Michael Sohlman and Gunnar Wetterberg as a responsive person with influence.

⁷ It is interesting to note that the former Agriculture Secretary Ulf Lönnqvist could not recall either of the two books (Lönnqvist 2005).

policymaker, had been involved in the Communist Party of Sweden (VPK), and had written articles about the need to reform agriculture. He had support from people like Sohlman and Per Molander at Finance (Wetterberg 2005; Sohlman 2005). In 1988, Wetterberg issued a report and proposed a reform that was more gradual, involving compensations, but no less fundamental than those of the economists (Wetterberg 1988).

Wetterberg had been an analyst at the Expert Group on Public Finance (ESO), a think tank close to Finance. The Group decided investigations to undertake, but independent authors were responsible for the results and recommendations (ESO 2007-04-18). The outfit also arranged top-flight seminars at which officials and analysts, such as authors of *The Political Economy of the Food Sector*, sat down and talked seriously about things. In a policy area bound by interest groups, it was important that an “outside” organ could be enlisted by “inside” officials seeking serious analysis and argumentation (ESO 1987).

An additional example of the alliance between the economists and Finance comes from a panel investigation organized in 1987 under the Nordic Ministries of Finance. Among those appointed were Bill Fransson⁸, an expert at Finance, and Ewa Rabinowicz, one of the secondary authors of *The Political Economy of the Food Sector*. The results were in line with the former economic studies (Nordiska Ministerrådet 1989).

FINAL BLOWS

Agenda setters need rich specialized knowledge to reformulate the issue, outmaneuver resistance, and induce acceptance. They need expertise not only in agricultural and regulatory process, but in the discourse situation.

Looking back, Michael Sohlman says that, while the previous blasts weakened the old system, the stones that crushed it weren't cast until 1988-89. First there was a study of the preparedness rationale. It was authored by expert Per Molander, who was employed by the National Defence Research Establishment.⁹ All along, since 1947, the idea that Sweden should be self-sufficient in basic foods served an underlying rationale. As Hedlund and Lundahl had previously argued, Molander questioned the entire rationale, as well as the system's effectiveness in “preparedness”¹⁰ (Molander 1989).

The other “stone” identified by Sohlman was the OECD's 1987 report about Swedish agriculture. It highlighted the system's costs and damage to international trade. OECD gave advice at a Ministerial level to member states to start immediately

⁸ Bill Fransson belonged to the same group as Carl-Johan Åberg, Erik Åsbrink, Ulf Larsson Bengt K Å Johansson and Michael Sohlman at the Ministry of Finance and even more so after the link between the Minister of Finance and Prime Minister Feldt (Feldt 1991, 140).

⁹ Molander later became head secretary of the Agricultural Policy Committee.

¹⁰ Moreover, the Defense Policy Decision of 1987 stated that the possibility a blockade lasting years should not be a concern.

to deregulate towards a more market-based system, abolishing fixed prices and reducing tariffs (OECD 1987; Hellström 2005).

Along with the Wetterberg report of 1988, these blows finalized and validated an official change of thinking.

NEW AGRICULTURE CHIEFS WITH INSTRUCTIONS TO DEREGULATE

Real reform would get absolutely nowhere without support from the Ministry of Agriculture. The Minister Svante Lundqvist and his State Secretary Ulf Lönnqvist never really showed it.¹¹ But in autumn 1986 they left office and were replaced by Mats Hellström and Michael Sohlman.

Hellström and Sohlman had clear instructions from the Social Democrat Prime Minister Ingvar Carlsson: Deregulate the system (Hellström 2005).

Sohlman had a background at Finance, which helped the two Ministries coordinate the reform process. At Agriculture, more economists and other analysts were appointed—as well as more women and younger people. Within Agriculture, the new faces came with a new outlook (Sohlman 2005; Hellström 2005; Feldt 1991).

At the same time, media presence was stepped up. Between 1987 and 1989 the politicians' share of op-ed articles rose. The debate earlier initiated by economists and other academics was replayed by policymakers. Incidentally, the Conservative and Liberal parties kept a low profile while the Social Democrats spoke out for reform.

1990, THE WALL COMES DOWN

In June 1990, when the Swedish parliament voted in a general consensus for a new agricultural policy, it was seen as a major shift to a new, more market-oriented system. Negotiated prices were replaced with freer pricing and competition increased in all parts. After generations of top-down administration, consumers would again be the driving force.

The bureaucracy was cut, though not completely abolished, since their new duty was to monitor the reforms (Flygare and Isacson, 2003, 254-255).

In what way was the reform more than an adjustment? Daugbjerg and Studsgaard (2005) claims that it involved a paradigm shift from a state assisted

¹¹ This is not to deny that Lundqvist and Lönnqvist advanced the movement. In addition to actions previously noted, they initiated and oversaw a commission to investigate the reasons behind the high and rising food prices and especially the operations of the middlemen—the wholesale and retail traders. On the commissions the farmer groups were sparsely represented (SOU 1987). According to former agricultural secretary Jan-Erik Ljusberg (2005), the government did not want the farmers to dominate again. The commission made numerous comparisons with other European and non-European countries where food prices were considerably lower. Even if the major criticism concerned the middlemen and their monopoly tendencies, the commission also implicated the price controls and tariffs (Micheletti 1990; Ljusberg 2005; Thomaeus 2005).

paradigm to a market-liberal paradigm in which agriculture was seen as an economic sector like any other. In that way Sweden and New Zealand were, among agricultural reforms, exceptional and unique (Daugbjerg and Studsgaard 2005, 104; Scrimgeour and Pasour 1996).¹²

By 1994-95 there was substantial evidence that the reform of 1990-91 was successful. Prices that had risen sharply in the 1980s started to fall substantially, and consumers were more content with the new choice of goods and the quality of the food (SOU 1994:119, 11-22). Increasing competition among the middlemen broke the monopoly-like position among the farmers industries step by step and with the liberalization consumer demand decided what was to be produced (Flygare and Isacson 2003, 254).

JOINING THE EU

In 1994-95 Sweden joined the EU and its common agricultural policy (CAP), and controls were partly recreated in new forms. Agricultural trade within the EU was free but tariffs and quotas do protect against outside competition. The EU-entry led to re-bureaucratization since the aims of the EU agricultural policy—income, production, and productivity goals—are comparable to those of the former Swedish policy (Flygare and Isacson 2003, 254-255). On the other hand, prior to Swedish membership in the EU in 1995 the food and beverage industry was protected from import competition by both tariff and non-tariff barriers to trade. The reduction of these barriers did increase competition and continued to reduce the high Swedish food prices (Wilhelmsson 2006, 26). The increasing economic integration of food markets have led to a convergence of prices where high-cost countries, like Sweden, has benefited the most (Jørgensen 2005, 21-22). Finally, one might conjecture that because Sweden entered the EU with a highly liberalized agricultural sector, that may have helped slightly to recenter EU agricultural negotiations in a more liberal direction.

CONCLUSIONS

On and off, since the origins in the 1930s, economists protested the worst features of agricultural policy. Those wielding the power of economic reasoning themselves constitute “a player,” giving potential to an oppositional coalition, a challenge to the iron triangle. But reasoning alone will never change policy. It must be joined with officials and opinion makers. Such a coalition emerged in the decades

¹² Carsten Daugbjerg and Jacob Studsgaard (2005) have compared the reforms in New Zealand and Sweden during the 1980s and 1990s. In both countries a new dimension of conflict was established. Formerly a program of farmer support, agricultural policy was transformed into an issue of overall social well-being, macroeconomic policy, and inflation. Also, those who initiated the reform succeeded in changing the arena of decision-making.

leading up to 1990, and the new thinking succeeded in vastly reducing agricultural controls in Sweden.

When the economists started to analyze things with public choice, they opened the eyes of policymakers and the public. They showed that the power in agricultural policy was closely linked with industry representatives, the agricultural bureaucracy, and politicians working in the field. Moreover, the existing system could perfectly well be explained as service to vested interest, rather than the official goals.

Who benefited and who lost from the policy? By making clear that deregulation would bring large benefits to consumers, the debate was altered. Journalists caught on.

Economists played an important role in the story, but of course there were heroes in all roles—journalism, politics, and the bureaucracies themselves. In some respects the iron triangle was outflanked and vanquished, but in other respects it was infiltrated, affected, and softened from within.

Economists became idea carriers. Their efforts to enlighten people and alter opinion led to substantial correction of gross, longstanding errors in agricultural policy.

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ECONOMICS IN PRACTICE

Where Would Adam Smith Publish Today? The Near Absence of Math-free Research in Top Journals

DANIEL SUTTER* AND REX PLESKY**

[ABSTRACT](#)

IF HE WERE AROUND TODAY, WOULD ADAM SMITH BE ABLE TO PUBLISH in top economics journals, assuming he would want to? Our investigation shows that he would have serious difficulty unless he mastered some mathematical techniques and modes of thinking.

Critics have accused top economics journals of being closed to math-free analysis and scholarship. If valid, such a closedness could significantly affect the economics profession. An inability to publish in prominent journals disadvantages kinds of thought and research that either do not much rely on math, or perhaps are compromised or degraded by math – for example, the kind of research that earned Nobel prizes for Myrdal, Hayek, Coase, Schelling, Buchanan, and that surely would have earned a prize for Keynes had he lived long enough. It appears that these pantheon economists in their prime today would be totally unable to place their classic works in top journals. Their contributions would fall to obscurity unless they could do the math, which they might well be able to do, *and* unless the substance and clarity would survive the make-over.

The math modes tend to advantage individuals with the requisite human capital, mentalities, and characters. The combination of the pyramidal structure of the discipline and majoritarian department politics might make the selection effects long lived (Klein 2005). And with tenure, decisions made today could have impacts for decades to come.

Economics is a field calling for exploration of how the accumulation of models, data, and other learning are best formulated and interpreted, and such

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exploration should involve diverse voices heterogeneous with respect to training. Few critics of formalism contend that there should not be *any* outlets or prestige for mathematical articles. Model building definitely has virtues, and regression analysis is obviously tremendously important, and we do not seek to denigrate these types of research. But scholars with a deep understanding of history, politics, policy analysis, law, and business, among other fields enrich the discipline. Such a heterogeneous cast of characters would produce diverse research. Economics is impoverished if only some types of learning reaches top journals and some of the diverse characters are permanently banished to the profession's back benches.

In evaluating such concerns, it is good to have evidence on whether the top journals are open to math-free research. Sometimes commentators see patterns that do not exist. Consider the "hot hand" phenomenon in basketball. Many fans and announcers believe that players will get hot and go on a rush where they can hardly miss a shot. But Gilovich, Vallone and Tversky (1985) found no support for the hot hand. Faulty perceptions may also underlie charges of media bias. Vallone, Ross and Lepper (1985) found that highly partisan viewers of news segments on Israeli-Palestinian relations perceived bias against their side in the coverage, but the bias was in subjects' selective recall -- remembering parts of stories favorable to the other side and forgetting bits favorable to their side. Our investigation seeks to qualify the current percentage of math-free papers in top journals to inform further debate.

Several previous studies have examined the mathematical content of economics journals. Leontief (1982) found that over half of the papers in the *American Economic Review* between 1972 and 1981 contained mathematical models without data. Examining the 1980s, Morgan (1988) documented a similar pattern for the *Economic Journal* and found that the percentage of math-free papers in the *American Economic Review* continued to fall. Examining four top general interest journals from 1963 through 1996, Coehlo and McClure (2005) found that mathematical research increased over time and that model-building crowded out empirical research. These papers focus on the balance of modeling and empirics. In the present paper, the primary distinction is that between math and "math-free" research. Math-free research in this paper means verbal or graphical analysis, case study evidence, and empirical work which does not rely on multiple regression analysis. Thus math articles here include not only model building but also papers with regression tables. However, our research does speak to the balance of modeling and empirics.

OUR APPROACH

We examined ten leading economics journals to assess the number of math-free papers published. We excluded explicitly mathematical or econometric journals, such as *Econometrica*, the *Journal of Econometrics*, or the *Journal of*

Mathematical Economics. We also did not include nominally general interest but widely recognized mathematical journals such as the *Journal of Economic Theory*, *Review of Economics and Statistics*, the *Review of Economic Studies*, and the *International Economic Review*. We did not include the *Journal of Economic Perspectives*, which publishes many math-free papers but does not work by the usual open submission. We chose the following ten journals, all of which were among the thirty six top journals used by Scott and Mitias (1996) to rank U.S. economics departments:

Six top general journals (listed alphabetically)

American Economic Review (AER)

Economic Inquiry (EI)

Economic Journal (EJ)

Journal of Political Economy (JPE)

Quarterly Journal of Economics (QJE)

Southern Economic Journal (SEJ)

Four top field journals

Journal of Development Economics (JDE)

Journal of Law and Economics (JLE)

Journal of Monetary Economics (JME)

Journal of Public Economics (JPubEc).

The field journals chosen were leading journals in four of the prominent fields of the profession. We recognize that many special journals publish much math-free research, but they are not “top” journals.

Our sample excludes comments, replies, review articles, and book reviews.¹ We also exclude presidential, invited, or Nobel Prize lectures, because these do not represent normal publication pathways. Our totals for the *AER* include the shorter papers but exclude the annual American Economics Association Papers and Proceedings issue.

We employ three nested criteria:

Weak criterion of math-free: A paper is weakly math-free if it has no numbered equations *and* no tables of regression results.

Intermediate criterion of math-free: A paper is intermediately math-free if it is weakly math-free and we judged it to be without mathematical or regression analysis.

Strong criterion of math-free: A paper is strongly math-free if it is intermediately math-free and it is neither experimental economics, about economics instruction, nor part of a special issue or symposium.

¹ Our sample contained no literature review articles and several journals published no book reviews.

The weak criterion counts provide a simple “first cut” to identify papers with a minimal degree of math. We make no effort to distinguish articles based on the complexity and simplicity of their mathematical or statistical content, and thus our weak criterion excludes some not very sophisticated articles. But sometimes an article presents regression results in equation form in the text as opposed to in tables. Also, some papers present mathematical material without numbering any equations or expressions. Thus, some papers with mathematical content can pass the weak math-free criterion. We then evaluated the papers meeting the weak criterion and excluded those that nonetheless contained mathematical or regression analysis, yielding our second criterion.

Several types of papers meet our intermediate criterion yet may not constitute math-free regular research articles. One type is experimental economics. Although some experimental papers have large enough sample sizes for regression analysis, some feature only verbal statements of hypotheses and means tests. Consequently some experimental papers meet our intermediate criteria. But experimental papers do require a suitable laboratory and funding and thus may overstate the accessibility of journals to genuinely math-free research. Articles in special issues and symposia, sometimes based on conferences or workshops, constitute another type. These articles again depart from normal publication pathways, since an author must be invited to participate in the conference or symposium. Finally, several math-free papers addressed economics instruction (specifically in the *SEJ*'s Targeting Teaching feature), again departing from normal research. Our strong criterion omits experimental, symposium, and instruction articles from the sample (thus counting them neither as mathematical nor math-free).

We feel that the “strong” criterion should be regarded as the appropriate criterion for the issues explored here. It is “strong” only in relation to the other two criteria, which we see as simply steps toward the formulation of the appropriate criteria.

An Excel file contains the citation for all the papers in the ten journals meeting the weak, the intermediate, and the strong criteria. In addition, the file contains author information and a brief description of all papers meeting the intermediate criterion.

THE NEAR ABSENCE OF MATH-FREE RESEARCH

Table 1 presents our findings concerning technical papers in the ten journals. Overall 5.8, 3.0 and 1.5 percent of the papers in the sample met our weak, intermediate and strong criteria for math-free. The *EJ* was most accessible to math-free research. The *EJ* was the only journal to exceed 10 percent weakly math-free papers, and accounted for over 40 percent of the strongly math-free papers. The *QJE* ranked second in weakly math-free research at 9 percent while the *JPE* was second in strongly math-free at 3 percent. The other general interest journals - *AER*, *EI*, and *SEJ* - published 5-9 percent weakly and one percent

strongly math-free. The field journals all had 5 percent or less weakly and 2 percent or less intermediately math-free. The *JDE* was least accessible, with only two papers weakly and zero intermediately math-free. Our findings confirm Anderson and Boettke's (2004) characterization of the *JDE* as favoring formalism over a "detailed institutional narrative that makes sense of the policy history, developments, and alternatives" (307).

Table 1: Math-free Papers in Top Journals

Journal	(1)	(2)	(3)	(4)	(5)	(6)
<i>AER</i>	168	18	46	5	2	1 [2]
<i>JPE</i>	115	10	52	6	3	3 [3]
<i>QJE</i>	80	16	26	9	4	1 [1]
<i>EJ</i>	149	21	50	13	7	5 [8]
<i>EI</i>	88	15	38	7	3	1 [1]
<i>SEJ</i>	108	23	31	6	5	2 [2]
<i>JLE</i>	46	39	7	2	2	2 [1]
<i>JME</i>	126	7	63	5	2	0
<i>JDE</i>	149	9	39	1	0	0
<i>JPubEc</i>	222	13	61	4	2	0 [1]
Totals	1251	15	46	6	3	2 [19]

Note: Column headings are as follows: (1) Total articles; (2) percentage with no equations; (3) percentage with no regression tables; (4) percentage weakly math-free; (5) percentage intermediately math-free; (6) percentage strongly math-free, [number of strongly math-free articles].

Math-free research was more common in the general interest journals than the field journals. The general interest journals published 8 percent weakly, 4 percent intermediately, and 2 percent strongly math-free. The field journals published 3 percent weakly, 1.7 percent intermediately, and 0.4 percent strongly math-free. Our sample includes only four field journals, so the lower math-free rate among field journals may not hold up with a larger sample of field journals.

Of course any general-versus-field comparison will be highly sensitive to the fields included.

Our findings confirm that the *JLE* is a highly empirical journal. Table 1 shows that only 7 percent of its articles do not have a single regression table, which means that 93 percent of its articles contain at least one regression table. The next most empirical was the *QJE*, with 74 percent. At the other end, 50 percent of *EJ* articles had regression tables, followed by the *JPE* with 48 percent, *JPubEc* with 39 percent, and *JME* with only 37 percent. As for articles with numbered equations, the *JME* led the pack, with 93 percent of articles with at least one numbered equation. At the other end, the *JLE* had 61 percent with at least one numbered equation.²

A LOOK AT THE STRONGLY MATH-FREE ARTICLES

For all ten journals, 73 articles met the weakly math-free criterion, 38 were intermediately math-free, and 19 strongly math-free. Of the 38 intermediately math-free articles which do not meet the strong criterion, 10 were experimental, 6 were conference or special-issue papers, and 3 were economic education papers (in the *SEJ*). Removing those 19, which do not contain math but are excluded because they arguably provide a misleading picture of the accessibility of top journals to math-free research, we are left with 19 strongly math-free articles, listed here (all from 2003 or 2004):

American Economic Review:

John Komlos, "Access to Food and the Biological Standard of Living: Perspectives on the Nutritional Status of Native Americans"

Alexander Field, "The Most Technologically Advanced Decade of the Century"

Economic Inquiry:

E. Woodrow Eckard, "The 'Law of One Price' in 1901"

Economic Journal:

David Greenaway and Michelle Haynes, "Funding Higher Education in the UK: The Role of Fees and Loans"

Richard Dickens and David Ellwood, "Child Poverty in Britain and the United States"

² Coehlo and McClure (2005) investigate whether mathematical complexity crowds out empirical research. Their main results come from a time series of articles from the *AER* between 1975 and 1995. Our cross sectional results provide some modest support for crowding out. The journal with the largest percentage of papers with no equations, the *JLE*, also has the highest percentage of papers with regression tables, while the journal with the fewest papers with regressions, the *JME*, has the lowest percentage of papers with no numbered equations. The five journals (*JLE*, *QJE*, *SEJ*, *EJ*, *JDE*) with at least 60 percent of papers with regression tables had 17 percent of their papers with no numbered equations, which slightly exceeds the 14 percent for the five journals with fewer than 60 percent of papers with regression analysis.

Mike Brewer, Tom Clark and Allissa Goodman, “What Really Happened to Child Poverty in the UK in Labour’s First Term”

Knut Roed and Raaum Oddbjorn, “Administrative Registers – Unexplored Reservoirs of Scientific Knowledge?”

Barham Pranab, “Journal Publication in Economics: A View from the Periphery”

Paul Fenn, Alastair Gray and Neil Rickman, “The Economics of Clinical Negligence Reform in England”

Costas Meghir, “A Retrospective on Friedman’s Theory of Permanent Income”

Michael Mandler, “Status Quo Maintenance Reconsidered: Changing or Incomplete Preferences?”

Journal of Political Economy:

Thomas Piketty, “Income Inequality in France, 1901-1998”

Muriel Niederle and Alvin Roth, “Unraveling Reduces Mobility in a Labor Market: Gastroenterology with and without a Centralized Match”

Richard Thaler and Shlomo Benartzi, “Save More Tomorrow: Using Behavioral Economics to Increase Employee Savings”

Quarterly Journal of Economics:

Thomas Piketty and Emmanuel Saez, “Income Inequality in the United States, 1913-1998”

Southern Economic Journal:

Paul Rubin, “Folk Economics”

Jason Taylor and Jinill Kim, “The Pre- and Postwar Price-Output Paradox Revisited”

Journal of Law and Economics:

Maria Arbatskaya, Morten Hyiid and Greg Shaffer, “On the Incidence and Variety of Low Price Guarantees”

Journal of Public Economics:

Brad Barber and Terrence Odean, “Are Individual Investors Tax Savvy? Evidence from Retail Discount Brokerage Accounts”

Kocher and Sutter (2001) find that 25.7 percent of papers in 15 top journals came from just 10 U.S. schools. We examined the affiliations of the authors of the 19 strongly math-free articles, which had 33 authors (with authors of more than one paper counted multiple times in the total). Of the 27 authors with an academic affiliation, 15 were at universities with the world’s top 60 economics departments, according to Tom Coupé’s world rankings for 1990-2000 (Coupé 2003). Two of math-free the top journals - the *JPE* and *QJE* - are even more closed. Five of the seven authors for the strongly math-free papers in these journals were from a top 60 department, and the lowest ranked of these top universities is Carnegie-Mellon (#32). Thus economists not already holding a position at a top ranked department face long odds publishing math-free research in top journals.

IS MODEL BUILDING LOSING MARKET SHARE?

Leontief (1982) criticized what he saw as the trend in economics toward “mathematical models without any data,” which was diminishing the relevance of the discipline. Leontief found that over 50 percent of the papers in the *AER* in the 1970s met this description. Morgan (1988) found that half of the papers in the *EJ* in the 1980s were similarly exclusively mathematical. Examining the period from 1963 to 1996, Coelho and McClure (2005) found a positive and statistically significant time trend coefficient in a regression of the percentage of articles that included “lemma” or “multiple equilibrium,” in each journal *AER*, *JPE*, *QJE* and *EJ*.

Table 2: Trends in Model-Building Share

Journal	1972-76	1977-81	1982-86	2003-04
<i>AER</i>	50%	54%	42%	44%
<i>EJ</i>	34	50	52	43
<i>JPE</i>				49
<i>QJE</i>				22
<i>EI</i>				35
<i>SEJ</i>				26
<i>JLE</i>				5
<i>JME</i>				61
<i>JDE</i>				39
<i>JPubEc</i>				59

Table 2 reports the earlier findings of Leontief and Morgan for the *AER* and *EJ* and our corresponding percentages for each journal. For the *AER*, the measured years 1982-1986 coincide with the editorship of Robert Clower (1981-1986), which are thought to have been “off trend” in a math-free direction, and hence may not be representative of the trend. The preponderance of pure model building moderated from 54 percent in the late 1970s to 42 percent in the 1980s and 44 percent in our sample. For the *EJ*, the trend toward mathematics for its own sake has been halted, with the current percentage of 43, down from 52 percent in the 1980s. But two journals in our sample, the *JME* and *JPubEc*, feature about 60 percent mathematical models without regression tables.

The numbers are percentages of papers which feature mathematical models without regression analysis. The totals for the *AER* for 1972-76 and 1977-81 are from Leontief (1982), while the totals for the *EJ* and 1982-86 *AER* totals are from Morgan (1988). The totals 2003-04 are from our sample, calculated as papers with no regression tables minus papers meeting the intermediate criterion.

THE FUTURE

We found that only 1.5 percent of papers published in 10 top journals in 2003-04 met a strong criterion for math-free. And one journal, the *Economic Journal*, accounts for 40 percent of the strongly math-free papers. Also, more than half of the authors of these papers with academic affiliations were at top-60 ranked world departments. And perhaps the most surprising and disturbing result is the absence of math-free research from three leading field journals – one out of 497 papers in the *JME*, *JDE* and *JPubEc* were strongly math-free.

We are inferring the accessibility of journals based on published research, which is a bit tricky since we have no evidence on the acceptance rates of math-free and mathematical submissions. Conceivably, math-free research might have a higher acceptance rate despite the low overall proportion of published papers. But it seems unlikely, since the supply of math-free research should be reasonably elastic to a perceived receptiveness of editors. With 98.5 percent of papers mathematical, it would appear that many referees and editors treat modeling or regressions as a necessary condition for publication.

The emphasis on mathematical modeling and regression analysis imposes a toll on the profession. Adam Smith spent his early years studying literature, history, ethics, political and moral philosophy, and then teaching literature and rhetoric to college students. Today to succeed in the profession he would need to study model building and regression analysis well enough to publish in “good” journals, and he (and the rest of us) would have lost the value added from the studies displaced. The same would apply for many Nobel prize winners who published their work in an economics profession less tied down to model building and regression analysis. The emphasis of top journals on regression analysis also disadvantages other types of research. Many times this leads to the assembling of a data set large enough for regression analysis at the cost of a richer quantitative portrait of a smaller sample. Is ignoring data sets too small for robust econometric analysis good social science? While rigorous statistical testing of hypotheses is an invaluable tool of social science, a corner solution of all regression analysis is unlikely to be optimal.

APPENDIX

Spreadsheet of 19 strongly math-free articles with full citations. [Link](#)

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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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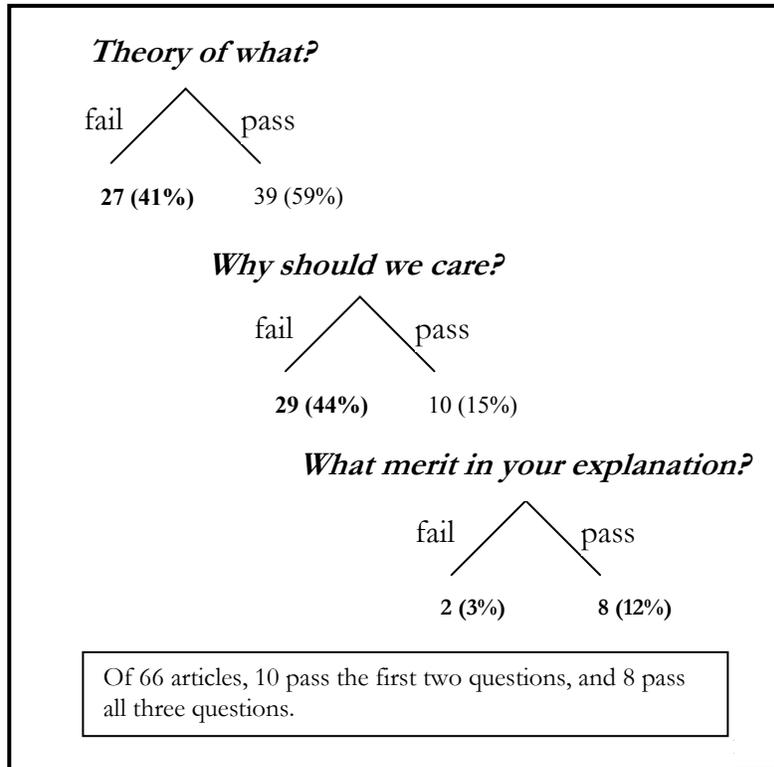
⁶ The authors thank Niclas Berggren and Warren Gibson for valuable comments. Pedro Romero is an H.B. Earhart fellow and thanks the Earhart Foundation for financial support.

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?

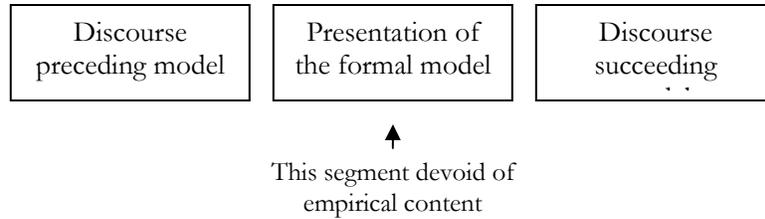
The assessment of *Theory of what?* draws on the prior sub-tests. Sufficient for passing is that the article provides any kind of illustration of the explanandum, either factual or fictitious. But that condition is not necessary. The article might be scored “yes” here by virtue of the economic context 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:

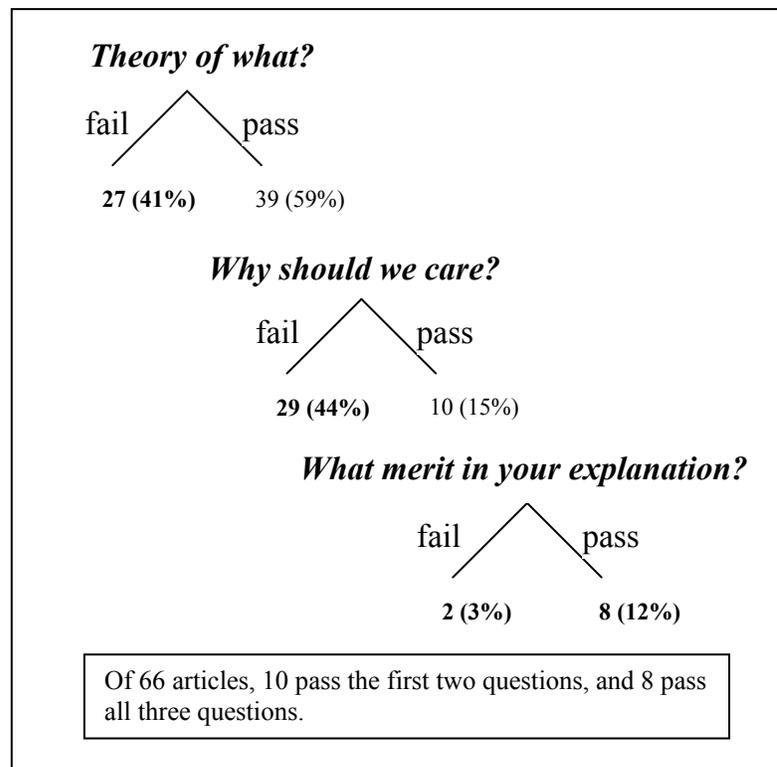
1. We included the special issue (Vol. 119, no. 1, Nov. 2004) on “Macroeconomics of Global Capital Market Imperfections,” based principally on papers presented at a conference held at

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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ECONOMICS IN PRACTICE

The Internet and the Structure of Discourse: The Websites of Economists at Harvard and George Mason

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ABSTRACT

[E]conomic ideas – ranging from new but unelaborated concepts through isolated propositions about causality, all the way to full-blown theories – arise in the highbrow part of the economics profession and then diffuse first within the profession and then sometimes outside it to journalists, bureaucrats, politicians, and other citizens.

– Robert Solow (1989, 75)

Robert Nelson (2004) draws on theology and religious history to distinguish “scholasticism” and “pietism.” In the scholastic approach “a church hierarchy interprets the ways of God to the faithful. An official priesthood transmits the results of a long history of internal church discussions and debates.” In the pietistic view “there is a more direct relationship between the individual and God. Protestantism in general preaches that salvation is ‘by faith alone’ – without any essential intermediary role for a church hierarchy” (474). Nelson uses the terms to characterize two approaches in economics. He analyzes the words of many prominent economists, notably the retrospectives in the centennial issue of the *Economic Journal*, and argues that the analogy fits a tension within the economics profession. As signaled by the opening quotation by Robert Solow (which is also used by Nelson), scholastics are more concerned with speaking to each other, distilling results, and passing them down through intermediaries to the laity. Pietistic figures are more interested in speaking directly to the

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everyman. They see themselves, not as the representative of an exalted priesthood, but as a facilitator or prompter of direct access to important truths and insights.

In the matter of scholasticism vs. pietism, Nelson usefully distinguishes between *structural* and *topical*. The structural refers to the way that discourse is structured. The image of structural scholasticism is typified by the vision given by Solow in the opening quotation: “highbrow” elites at the apex of a pyramid. Structural features give rise to and authorize *topical* features. Scholastic structure gives rise to and authorizes exclusive discourse and “scholastic” refinements, in the dictionary sense of “scholastic.” Nelson suggests that mathematics is the new Latin (476). Likewise, pietistic structure gives rise to public discourse in the vernacular.¹

If one wanted to pursue Nelson’s distinction in an empirical fashion, it is plain where to look for scholasticism: “The official keepers of the faith of the new ‘church’ of this modern scholasticism are found in the leading university departments of economics and at the ‘top’ journals (which are almost always edited at one of the elite departments)” (476). The National Research Council and other sources rank Harvard the number one economics department among research universities.² Harvard economists edit *The Quarterly Journal of Economics*, consistently ranked a top economics journal.

As for the pietistic, Nelson writes:

Organizations such as the Foundation for Economic Education, the Cato Institute, and the Institute for Economics Affairs do not appeal to priestly authority, and do not lobby the powers that be to impose their doctrines on others (they oppose the government production of schooling). They appeal to the common understandings of interested lay observers (477).

An economics department known to have such an orientation is that at George Mason University. In fact, many professors at GMU have worked much with think tanks and other “pietistic” operations. What typifies the work of many of such organizations is not merely policy advocacy, but also a kind of economic instruction that has been described as “homiletic” (Doherty 2007, 156). (The first definition of *homily* given by the *American Heritage Dictionary of the English Language* (4th ed.) is: “A sermon, especially one intended to edify a congregation on a practical matter and not intended to be a theological discourse.”)

Nelson (2004) says that pietistic economic organizations tend to favor the free market. He writes:

The three examples given are libertarian examples, and certainly other examples from other ideological quarters could be given. However, we

¹ In the topical dimension, Nelson’s scholastic-pietistic tension directly parallels the scholastic-public discourse tension set out in Klein (2001).

² A complete ranking list can be viewed at Joseph Newton’s website. [Link](#). Harvard is ranked number one in “[a] recent ranking of economics departments by publications in the top 30 research journals,” calculated between 1995 and 1999 (Kalaitzidakis et al 2003). Also, in the *US News* ranking (2005), Harvard also scores among the top five schools to obtain an economics PhD.

would argue that the more mainstream “liberal” and conservative organizations and periodicals tend to approach issues politically rather than economically; they appeal more to the sporting nature of the political contest. As for the leftist periodicals outside the center, we would argue that their emphasis on elucidating economic principles is very weak. Thus, in endeavors of economic pietism, I see a certain prominence to libertarian ideas (477).

We have not made an independent assessment of pietistic economic organizations and publications, and, although there are clear counter-examples such as the Economic Policy Institute, *Challenge* magazine, and Brad DeLong’s *Semi-Daily Journal*, it seems to us that there is some truth to Nelson’s observation.³ Accordingly, in as much as free-market thinking is typical of pietistic economics, pietism is again well represented by the George Mason department, where most members are exceptionally supportive of economic liberalizations.

The various impressions suggest that there is reason to characterize Harvard as relatively scholastic and George Mason as relatively pietistic. We investigate economists’ websites to detect signs of such a contrast.

THE INTERNET AND THE STRUCTURE OF DISCOURSE

Nelson (2004) also describes a change in economic thought. The profession began as pietistic and has become more scholastic. Adam Smith stands as an example of the pietistic origins of political economy—Jacob Viner (1927, 218) described *The Wealth of Nations* as “a tract for the times.” Smith’s audience stretched far beyond the academy. Moreover, Smith was critical of academic scholasticism (1776: 760-781). Since Smith’s time the gap between the “highbrows” and the everyman has grown, partly or perhaps mainly because of what Nelson calls “structural scholasticism.” Part of our motivation here is to ask whether the gap is being reduced by technological advancements and the Internet. Will the Internet alter the structure of economic discourse? Will it bring greater professional standing to policy advocacy and economic homiletics?

It is our impression that blogs and the Internet have attracted new audiences to economic research. Notable among the economics blogs are four maintained by economists in our “sample”: *Greg Mankiw’s Blog*, *Marginal Revolution* (Tyler Cowen and Alexander Tabarrok), *Econlog* (Bryan Caplan and Arnold Kling), and *Café Hayek* (Donald Boudreaux and Russell Roberts). Almost daily they discuss economic ideas and research. *Café Hayek* excels in economic homiletics—the illumination of key insights and verities with a variety of illustrations.

³ For online directories of hundreds of free-market policy organizations in the United States, see the State Policy Network’s directory ([Link](#)) and, worldwide, the Atlas Economic Research Foundation think tank directory ([Link](#)).

Lay economic interest seems to be expanding with the capacity to navigate and find material on the web. Until recently, accessing research required library privileges, photocopying, and a lot of schlepping. Collecting the publication records of economists was very costly – a researcher had to contact each individual through the mail or fax. Access to biographical information is a major benefit rendered by the Internet. Nowadays there is Wikipedia and a growing openness about where one is coming from (Klein 2006). Discourse is plainer, more candid, more natural—just as the pietistic churches abandoned Latin and used the vernacular.

The Internet also empowers writing. Anyone can criticize anyone. If people find your stream of commentary and criticism useful or insightful, you will have readers. Even bloggers who do not attract many readers learn by doing; they learn some economics by writing about economic issues. It seems to us that the Internet has made economic discourse more contestable and more equitable, as well as more fragmented.

Scholastic endeavors also gain. Improved communications facilitate the gathering of research materials and collaboration between authors (Hamermesh and Oster 2002; Kim et al 2006).

We wanted to see how the two character types, scholastic and pietistic, use the Internet to formulate and distribute their ideas. Are economists embracing accessibility and transparency by hosting their information and publication records on the web? Are they posting articles for lay readers? Are they using the Internet to communicate what they consider to be the important things? How accessible is the thought of those in the relatively scholastic department compared to those in the relatively pietistic department?

To avoid possible misunderstanding, we should say that this investigation is not intended as criticism of Harvard. Yes, we would like to see academic economics move in the pietistic direction, and we are partial to GMU, our home institution, but, like Adam Smith (1776, 761f), we recognize that quality control within academia necessitates a significant degree of scholasticism, that the tension is one of marginal conditions, not corner solutions. The tension is inevitable—which is demonstrated in the way major Protestant religions evolved into scholastic hierarchies (Nelson 2004, 475). This investigation is a comparison of web utilization by the two departments, with the Nelson distinction providing motivation and framing.

WHAT AND HOW WE COUNTED

We restricted ourselves to gathering data from the web. Starting from the Harvard economics department homepage, we visited each faculty members⁴ website in full⁵,

⁴To remain faithful to the faculty lists as they appeared on the two websites, we included the following 9 visiting professors as Harvard Economics faculty: Yu-Chin Chen, Kenneth Hendricks, Guido Imbens, Elena Krasnokutskaya, Jeffrey Miron, Andrew Postlewaite, Geert Ridder, Roberto Rigobon, and Kenneth Sokoloff. The GMU Economics website did not include any visiting professors but did include one instructor, Thomas Rustici, whom we included.

⁵By “in full” we mean that we counted as available all materials to which one could access by “drilling down” from the individual’s departmental website.

during late 2005 and continuing through late 2006.⁶ We printed out each faculty member's curriculum vitae (CV), when available, tallied their publications, and compared the tallies with the number of publications available for download. The same process was then performed for the GMU faculty. (We did not “tip off” GMU faculty members—in fact, we kept the investigation secret.) Before completing the investigation, we checked for any CV and website changes as of December 1, 2006 so all data, for both departments, speaks for the situation as of late 2006.

We wanted to sort publications by type. The ideal would have been to create a categorization that separates scholastic from public-discourse. Unfortunately, we found that such a categorization would have required investigation of each individual publication and a judgment on our part of whether a particular article or book chapter was intended for a general audience. Instead, we opted for a cruder categorization based on how scholars tend to organize their CVs:

Peer-Reviewed Articles (PRA) are articles accepted or published by a peer-reviewed journal. “Peer-review” itself speaks of a scholastic emphasis. We count a publication as peer-reviewed based on the way the faculty member categorizes it in his or her CV or website. We realize that there is great heterogeneity in the journals where these publications are placed, and that the Harvard economists get into more prestigious journals. In no way are we trying to adjust for quality.

Working Papers (WP) are papers listed on CVs in any stage other than accepted/published,⁷ and can generally be thought of as preliminary drafts of what will become peer-review articles.

Other Publications (OP) include a variety of material, including books (written or edited), book chapters, short notes, policy studies, and articles for the general reader.⁸ Admittedly, these are a mixed bag, but all, even the few comparable to a chapter in *Handbook of Game Theory*, are generally more oriented toward the general reader than what that economist publishes in refereed journals. Most of these publications are oriented toward the general reader much more so than the typical peer-reviewed journal article.

The Appendix at the end of this article links to an Excel file that contains our counts. The final counts of publications listed on the CVs appear on the left side of worksheets A and B. The same system was used to keep track of publications available for download (either on the original or a linked website), reported in the right side of the worksheets.

⁶ The time spread was needed to find, print, count, and record the data. By the time we had printed and counted the publications for economists low on the alphabetical list, a few professors had updated their CVs. Double-checking in December 2006 was our way of bringing the data to a moment in time.

⁷ Some scholars separated complete (but unpublished) working papers, from incomplete research. Our counting system counts them as the same. Worksheets E and F list how each professor characterized the works we counted as “Working Papers.”

⁸ In the linked appendix, worksheets C and D list how each professor characterized the works we counted as “Other Publications.”

WHAT WE FOUND

Relative Proportions of “Scholastic” to “Pietistic” Writings

To make clear what we are doing, we first present a table of raw numbers, but we will quickly move past it. That initial table is Table 1, below, which naively reports counts using only the available CVs and websites in the two departments (the data is contained in worksheets A and B).

Table 1: Publication counts and percentages by type, based on information found in online CVs and website

	Harvard Economics		GMU Economics	
Peer-review articles (PRA)	3722	68.08%	1203	37.79%
Working papers (WP)	472	8.63%	147	4.62%
Other pubs (OP)	1273	23.29%	1833	57.59%
Total pubs (TP)	5467	100%	3183	100%

Next, to get a handle on the individual’s actual production in each category, we took the *higher* of: (1) tallies from the CV, and (2) tallies of items available for download. These adjustments deliver Table 2, which we think give better proxies of proportions of publication types (worksheets G and H report the tabulation).

Table 2 (which does not control for department size) indicates that Harvard focuses on peer-reviewed articles and working papers, together making 77 percent of their total publications. In contrast, for George Mason, it is Other Publications that predominate, making up 63 percent of their total publications.

Next we estimate figures “per capita,” and that entails further manipulations of the capita numbers. For PRAs, it makes no sense to proceed as though a professor with no CV or website has zero. Thus, we threw out professors who had neither an available CV nor an online listing of publications. We also threw out individuals who did not provide a CV and had suspiciously low quantities of publications (for example older and fully tenured faculty without a list of publications nor a CV available), leaving us to conclude that there was no information about their real record of PRAs (see worksheet I for a listing of the excluded professors from each department). For Other Publications and Working Papers, however, we did not throw anyone out; that is, included professors who had zeros, because for those publication categories it is more plausible that their lack of reporting represents a true absence of such work.

Table 2: Proxy publication counts and percentages by type, based on online CVs and websites

	Harvard Economics		GMU Economics	
Peer-review articles (PRA)	3734	64.46%	1219	32.94%
Working papers (WP)	733	12.65%	132	3.57%
Other pubs (OP)	1326	22.89%	2350	63.50%
Total pubs (TP)	5793	100%	3701	100%

The manipulations described in the previous paragraph worked out as follows: The denominator for Harvard's mean PRA was 60 (70 minus the 10 excluded faculty listed in worksheet I). The same 60 economists are used to identify the median PRA. The denominator for GMU's mean PRA was 19 (31 minus the 12 excluded faculty listed in worksheet I). The remaining 19 economists are used to identify the median PRA. For the OP and WP means and medians the full departments were included (70 for Harvard and 31 for GMU).

Table 3 reports the means and medians as described above for each department:

Pietism calls for wide dissemination and repetition of basic insights, and at GMU we observe a high number of "Other Publications" per capita.⁹ Scholasticism, in principle, can justify the concentration of attention on a small number of seminal works, the ideas of which are then absorbed and disseminated by others throughout the scholastic hierarchy.

Relative proportions appear in bold in the bottom row of Table 3. They reflect the characters of the two departments. We should emphasize, however, that neither department is all one type: Harvard produces a great deal of "Other Publications" and GMU produces a great deal of work for peer-review.

The numbers should not be taken too seriously. For example, they do not attempt to adjust for any difference in the age profile across the two departments. Also, it should be noted that means and medians differ greatly because "sluggers" pull up the mean. For example, for the George Mason calculation of mean Other Publications, James Buchanan has 529, Gordon Tullock has 440, and Walter Williams has 518. Finally, the proxies necessarily understate actual publications, since CVs and download inventories can only understate actual achievements, so these proxies are not to be taken as estimates of actual publications.

⁹ One might suspect that by not throwing out individuals with zeros for Other Publications, the procedure is biased in favor of GMU, which generally uses the web more than Harvard. In fact, the percent of faculty with zero Other Publications in this calculus is higher for GMU than Harvard, so we doubt a pro-GMU bias in the OP per capita calculation. In other words, throwing out the OP zeros would *help* GMU relative to Harvard.

Table 3: Proxy publication types “per capita”: means and medians

	Harvard Economics		GMU Economics	
	Mean	Median	Mean	Median
Peer-Review Arts	62.2	38.5	64.2	35
Working Papers	10.5	7	4.3	0
Other Pubs	18.9	5	75.8	19
Total Pubs	82.8	52	119.4	50
OP: (PRA + WP)	0.3	0.1	1.1	0.5

EXTENT OF WEB UTILIZATION

The data also indicate that George Mason’s pietistic quality has led it to exploit the Internet as a communication medium. The following table is about whether material is available for download, and hence does not throw anyone out of the tally.

In Table 4 George Mason outpaces Harvard’s mean usage of the Internet on all margins except for working papers.

Table 4: How much do the departments use the web?

	Harvard Economics		GMU Economics	
	Mean	Median	Mean	Median
PRA on the web	9.8	4.5	11.1	0
WP on the web	8.7	5	1.5	0
OP on the web	4.4	0	28.16	1
TP on the web	22.9	11.5	40.7	9

Table 5: Ratios of web-available publications to proxy publications

	Harvard Economics	GMU Economics
Total available PRA: Total proxy PRA	0.18	0.28
Mean available PRA: Mean proxy PRA	0.16	0.17
Median available PRA: Median proxy PRA	0.01	0.00
Total available WP: Total proxy WP	0.83	0.35
Mean available WP: Mean proxy WP	0.83	0.33
Median available WP: Median proxy WP	0.71	0.00
Total available OP: Total proxy OP	0.23	0.37
Mean available OP: Mean proxy OP	0.23	0.37
Median available OP: Median proxy OP	0.00	0.05
Total available TP: Total proxy TP	0.28	0.34
Mean available TP: Mean proxy TP	0.28	0.35
Median available TP: Median proxy TP	0.22	0.18

It seems natural to compare the two previous sets of data: proxy publication levels and how extensively each department uses the net. Table 5 makes comparisons between the content actually available on the web (recorded in the right-side columns of worksheets A and B) with our publications estimates from Table 3. The results suggest that, except for working papers, GMU leads slightly in making its material available for download on the Internet.

BLOGS

Again, the blogosphere allows for almost any motivated writer or thinker to speak to dozens or even thousands of readers. The “Comments” function at a blog enables readers to speak back and be heard. The blogging efforts of the two departments are summarized in Table 6.

Harvard’s Gregory Mankiw runs the only blog of his department, while George Mason’s faculty members contribute to seven active blogs. (After the moment of investigation, Harvard’s Dani Rodrik started a blog.)

Table 6: Blogging in Each Department

Harvard University	Contributors
1 Greg Mankiw's Blog http://gregmankiw.blogspot.com/	Gregory Mankiw
George Mason University	Contributors
1 The Austrian Economists http://austrianeconomists.typepad.com	Peter Boettke plus three others*
2 Café Hayek http://cafehaye.typepad.com/hayek	Donald Boudreaux Russell Roberts
3 EconLog http://econlog.econlib.org	Bryan Caplan Arnold Kling*
4 Marginal Revolution http://www.marginalrevolution.com/	Tyler Cowen Alexander Tabarrok
5 Neuroeconomics http://neuroeconomics.typepad.com/	Kevin McCabe plus five others*
6 Overcoming Bias http://www.overcomingbias.com	Robin Hanson plus 35 others*
7 Tyler Cowen Ethnic Dining Guide http://www.tylercowensethnicediningguide.com	Tyler Cowen

**not a faculty member*

CONCLUDING REMARKS

It is natural to look to the Harvard department, the apex of the economics pyramid, for exemplification of the scholastic approach. George Mason, on the other hand, has an unusual emphasis in policy discourse and economic homiletics. Taking those two departments as representative of “scholastic” and “pietistic,” a comparison of website usage reflects the character of each. Harvard economists are clearly devoted to publishing in peer-reviewed, scholastic forums leaving them relatively less time to pursue other publications. Their web presence reflects this preference as it puts striking emphasis on sharing current working papers. George Mason reveals a preference for

reaching audiences outside of the academy and into the laity. They publish extensively in alternative outlets and make a fuller use of the web to share their ideas and research.

The Internet serves as a powerful tool in generating and disseminating economic ideas. Measured web activity is a metric that can help us locate a department's character on the scholastic-pietistic continuum.

APPENDIX

Excel file containing the data, in the following worksheets. [Link](#)

Worksheet A: Harvard University Department of Economics

Worksheet B: George Mason University Department of Economics

Worksheet C: Harvard University's Other Publications (OP)

Worksheet D: George Mason University's Other Publications (OP)

Worksheet E: Harvard University's Working Papers (WP)

Worksheet F: George Mason University's Working Papers (WP)

Worksheet G: Harvard University's proxy publication counts.

Worksheet H: George Mason University proxy publication counts.

Worksheet I: Faculty excluded from PRA mean and median calculations.

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